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The Bancroft Library

University of California
Berkeley, California

Arthur L. Schawlow

OPTICS AND LASER SPECTROSCOPY, BELL TELEPHONE LABORATORIES, 1951-1961, AND STANFORD UNIVERSITY SINCE 1961

With an Introduction by
Boris P. Stoicheff

Interviews Conducted by
Suzanne B. Riess
in 1996

Since 1954 the Regional Oral History Office has been interviewing leading participants in or well-placed witnesses to major events in the development of Northern California, the West, and the Nation. Oral history is a method of collecting historical information through tape-recorded interviews between a narrator with firsthand knowledge of historically significant events and a well-informed interviewer, with the goal of preserving substantive additions to the historical record. The tape recording is transcribed, lightly edited for continuity and clarity, and reviewed by the interviewee. The corrected manuscript is indexed, bound with photographs and illustrative materials, and placed in The Bancroft Library at the University of California, Berkeley, and in other research collections for scholarly use. Because it is primary material, oral history is not intended to present the final, verified, or complete narrative of events. It is a spoken account, offered by the interviewee in response to questioning, and as such it is reflective, partisan, deeply involved, and irreplaceable.

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This photograph shows a flash of light from a ruby laser breaking a blue inner balloon without damaging the outer balloon. The red light from the laser passes through the clear outer balloon, but is absorbed by the dark blue inner balloon and produces a hot spot which breaks it. To show the balloon in the middle of the brief instant of breaking, a photographic flash lamp is triggered when the sound of the breaking balloon reaches a microphone. This gives a one millisecond delay after the laser pulse.

Photograph by Kenneth Sherwin and Frans Alkemade

San Francisco Chronicle, April 30, 1999

Arthur Schawlow

Arthur Schawlow, a Stanford University physicist and 1981 Nobel Prize winner for his pioneering work in lasers, died Wednesday at Stanford Hospital after a prolonged illness. He was 77.

Professor Schawlow and Charles Townes, professor emeritus of physics at the University of California at Berkeley and Professor Schawlow's brother-in-law, shared credit for inventing the laser, which made possible fiber-optic telecommunications, outpatient corrective eye surgery and CD music players, among many applications.

The two men developed the design for the laser in the 1950s and built a working laboratory model in 1960. But they never made any profits from the discovery because they were working for Bell Laboratories at the time. Both men are in the Inventors Hall of Fame in Akron, Ohio.

A native of Mount Vernon, N.Y., Professor Schawlow was interested in electrical, mechanical and astronomical things from childhood. He earned bachelor's and graduate degrees in physics from the University of Toronto.

He met his collaborator, Townes, a recognized leader in the field of microwave spectroscopy at Columbia University, while on a postdoctoral fellowship there.

In 1961, Professor Schawlow joined the physics department at Stanford, where he continued his research in optical and microwave spectroscopy, superconductivity and lasers. He was popular among students for both his science knowledge and his sense of humor.

At Stanford, Professor Schawlow was given the nickname "the laser man" because of his popular classroom demonstrations of the way the new tool he had helped develop worked.

In a favorite illustration of the laser's pinpoint selectivity, he used what he jokingly called a "ray gun" laser to shoot through a transparent balloon to pop a dark Mickey Mouse balloon inside — without damaging the outer one.

Stanford physicist and Nobel laureate Steven Chu recalled visiting a physics lab where "The Sayings of Art Schawlow" had been posted on a wall. One example: "To do successful research, you don't need to know everything, you just need to know one thing that isn't known."

Professor Schawlow is survived by his son, Artie, of Paradise, Calif.; daughters Helen Johnson of Stevens Point, Wisc., and Edie Dwan of Charlotte, N.C.; and five grandchildren.

A memorial service is planned at Stanford University, but no date has been scheduled.

Cataloguing information

SCHAWLOW, Arthur L. (b. 1921) Physicist

Optics and Laser Spectroscopy, Bell Telephone Laboratories, 1951-1961, and Stanford University since 1961, 1998, x, 383 pp.

Schawlow family background, Depression years in Toronto; early aptitudes in radio engineering; college and university studies in math and physics, and WWII interruption; Malcolm Crawford and thesis research on atomic beam light source; post-doc at Columbia University, 1949-1951; co-author, with Charles H. Townes, of *Microwave Spectroscopy* (1955), dealing with theory and experimental techniques of microwave spectroscopy; marriage in 1951 to Aurelia Townes, and move to Bell Telephone Laboratories: working on superconductivity, in 1957-1958 collaborating with Townes on the optical maser (laser), and publication of "Infrared and Optical Masers"; discussion of the atmosphere at Columbia and at Bell Labs, pressures, publications, patents; joins physics faculty at Stanford University: research group in laser spectroscopy, Ted Hänsch, students, administrative matters, other faculty; interest in teaching, motivation, ethical issues, funding and the military, telling stories, timing, hindsight; expert jazz collector; Nobel Prize in Physics, 1981, and other honors; son Arthur, Jr., and discussion of the treatment of autism.

Introduction by Boris P. Stoicheff, Department of Physics, University of Toronto.

Interviewed 1996 by Suzanne B. Riess.

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INTRODUCTION by Boris P. Stoicheff

ARTHUR LEONARD SCHAWLOW

The many contributions to science of Arthur Leonard Schawlow as a teacher, science writer, and creative physicist have won for him a renowned national and international reputation, highlighted by the award of the Nobel Prize in Physics in 1981, and the National Medal of Science in 1991. Two prestigious Arthur L. Schawlow Awards, given annually, honour him as one of the laser pioneers: a Medal of the Laser Institute of America for laser applications, and a Prize of the American Physical Society for contributions to laser science. On a more personal note has been the adulation of his many students and co-workers who published a volume, *Laser Spectroscopy and New Ideas: A Tribute to Arthur L. Schawlow*, on his 65th birthday in 1986, and who also organized *The Arthur Schawlow Symposium* on his retirement five years later. These were heart-felt gatherings of the many people whom he had touched with his friendship, consideration, and joy and wonder of science.

Arthur was born in Mt. Vernon, New York, but his family moved to Toronto, Canada in time for him to take his primary and secondary education there. His sister Rosemary recalls that Art had some problems in early school. In fifth grade, one of the teachers made life miserable for Art in public school, so that the family was advised to register him at another school, the Normal Model School where teachers were being trained. His progress was very good in everything but writing and art. Under a teacher's pressure, his writing improved, but his clumsy hands, by his report, were no good for drawing. When he proceeded to high school, at Vaughan Road Collegiate, to avoid art he enrolled in bookkeeping and typewriting rather than in art and botany. Nevertheless, he was in the academic stream and took the other usual subjects for university entrance. Art graduated with an excellent record, and enrolled in one of the most demanding programs of Mathematics and Physics at the University of Toronto. Thus began his career in science. He continued in graduate studies, obtaining a Master of Arts degree, followed by his Ph.D. degree in atomic physics in 1949, under the supervision of Professor Malcolm F. Crawford.

It was in 1948, when I began graduate research at Toronto, that I first met Art. His experiment in atomic beam spectroscopy and hyperfine structure was located in the basement of the McLennan Physical Laboratory, where he spent most of the day with his co-workers Fred Kelly and Mac Gray, while I was with the molecular group on the fourth floor. This was an extremely active period, with many returned veterans, all working furiously to all hours of the night. We would meet at tea time and at colloquia. And once experiments were in

progress, we seemed to have time to visit each other's labs because photographic exposure times were tens of hours, since this was the traditional method for detecting and recording spectra in those early years. It was a special pleasure to visit the basement lab, where often in the evenings Art would be serenading his atomic beam with the clarinet, which he played reasonably well. His idols were Benny Goodman and "Jelly Roll" Morton, and his repertoire mainly Dixieland jazz.

By that time, Art had an enviable collection of jazz records. He and his sister Rosemary started collecting while in high school, by buying used juke box records. This expanded to a full fledged hobby later when he was better able to afford this pastime, and also to record the music himself as tape recorders became available. As his science progressed, he was able to travel to conferences in various cities, and to devote some evenings to music halls and jazz.

Art and his colleagues published seven papers on their doctorate research including details of their apparatus and spectroscopic results. While each member of the team was responsible for designing and building an important part of the equipment and seeing to its proper working in the experiment, they each became acquainted with every piece of apparatus in the experiment. This turned out to be important to Art's later basic contribution to the laser; the two end mirrors which form the resonator are an adaptation of a device (the Fabry-Perot interferometer) used in the atomic beam experiments in the McLennan Laboratory. Art achieved early recognition for his research in atomic spectroscopy and received a postdoctoral fellowship to work at Columbia University. There began his long and fruitful association with Charles H. Townes, a pioneer of microwave spectroscopy, whom he first met at an American Physical Society meeting in April 1949.

At Columbia University, Art began research on the diatomic molecule OH using the new technique of microwave spectroscopy, and having difficulty in finding its spectrum he coined the memorable line, "a diatomic molecule is a molecule with one atom too many." He also began writing with Townes the book titled *Microwave Spectroscopy*, one of the first volumes in this field. Just about that time, Townes was involved with development of the MASER, and he tells the following story of how the idea of the MASER occurred to him. In April of 1950, he and Art were attending an American Physical Society Meeting in Washington, D.C., and they shared a room in the Franklin Hotel. Art, being a bachelor, was used to sleeping late, and Charlie, being married with four young daughters, would be up very early. So, waking up early as usual, and not wanting to disturb Art, Charlie dressed and went out to nearby Franklin Park. It was there, sitting on a bench, thinking about a government committee meeting he would be attending that afternoon on trying to find better ways of producing radiation shorter than millimeter wavelengths, that the idea of the MASER hit him. As Art

likes to point out, his own inadvertent contribution was crucial: "Imagine the result if I had wakened early!"

In May 1951, Art married Aurelia Townes, Charlie's younger sister, a fine musician and vocalist, and they raised a family of a son and two daughters. In the fall of that year, Art was employed by Bell Telephone Laboratories in Murray Hill, New Jersey and carried out research in superconductivity. His contacts with Townes continued, as Art spent many a Saturday at Columbia completing the book, *Microwave Spectroscopy*, published in 1955, and Townes was consulting for Bell Labs. Their collaboration on the possibility of extending the range of the maser into the visible region culminated in their famous paper of December 1958, "Infrared and Optical Masers," which established the principles of the LASER. Within two years, the first working device, the pulsed ruby laser, was developed by Theodore Maiman at Hughes Research Laboratories, soon followed by the helium-neon laser introduced by Ali Javan and colleagues at Bell Labs, and the era of the LASER was launched. The laser spawned a flourishing new field of science and technology, now known as "Quantum Optics", and a huge industry, commonly called "Photonics and Electro-Optics." And over the years, Art has been a continual contributor to the imaginative use of the laser in science, communications, engineering, and medicine.

With his appointment as Professor of Physics at Stanford University in 1961, Art became a major influence in the lives of many young scientists. In no time, he gathered around him a large group of able students, and a constant stream of visitors from many countries soon followed. His students enjoyed the fatherly advice given with Art's usual sense of humour and understanding: "To do successful research, you don't need to know everything, you just need to know of one thing that isn't known"; and, "Anything worth doing is worth doing twice, the first time quick and dirty, and the second time the best way you can." And when science fiction writers and journalists wrote about the death ray, and produced posters of "The Incredible Laser", showing laser cannons firing at rockets, Art's answer was that all one had to do was to polish the outer surfaces to reflect back the beam of light. In his own laboratory, he mounted such a poster on the door, adding the subtitle, "For credible laser see inside." In this heady atmosphere with its special "magic", the Schawlow Laboratory was one of the outstanding contributors in laser spectroscopy, producing many new ideas and techniques which became standards in the field.

During this early period of the laser, Art was deluged with invitations to write articles for the lay public, and to lecture to a variety of audiences all over the world. He accepted an exhausting schedule of travel, and charmed and informed audiences with his characteristic flair for telling anecdotes and performing demonstrations. One of his favourite and most vivid lecture demonstrations was to use his portable "ray-gun laser" to burst a blue

Mickey Mouse balloon placed inside a clear outer balloon, without damaging the outer balloon. This same idea had important consequences since it was applied to remedy retinal detachment with the laser: the lens of the eye transmits the red laser light, but the retina absorbs it and is heated sufficiently to weld the retina together. Art found the requisite balloons in the San Francisco Zoo, which he visited each month to stock up on his supply. One Sunday, when parents with disappointed children questioned the Sold Out sign for double balloons, they were told by the proprietor, "A crazy professor just bought out the entire stock!" Some demonstrations were carried out without apparatus, as when Art reminded audiences of the Doppler Effect, and moving towards the audience he elevated the pitch of his voice, and moving away he lowered the pitch.

Art's ad libs at seminars and colloquia are legendary. On one occasion he was speaking at Stanford on the topic, "Is Spectroscopy Dead?" He immediately defined what he meant by spectroscopy, and proceeded to give his talk, when Professor Felix Bloch asked, "What do you mean by dead?" Art blurted out, "Oh, when the chemists take over," to which he added his infectious laugh, and everyone else joined in, although chemists in the audience were not amused. Of course, Art went on to say that that was what happened to microwave spectroscopy, now done mainly by chemists and few physicists, so that chemists know much more about molecules. I vividly remember when he introduced me at a Stanford colloquium, and having given a brief biography, stressed that my undergraduate and graduate degrees were from the University of Toronto, and then emphasized, "...so he is very well-educated," followed by his belly-shaking laugh, knowing full well that his Stanford colleagues were aware that he too studied at Toronto. In Art's case, we added an Honorary Doctorate Degree in 1970 to make a total of four degrees from the University of Toronto, and branded him an "Exceedingly well-educated man."

Among Art's later contributions to science, two carried out with Ted Hänsch, then his colleague at Stanford, stand out. One was seemingly frivolous, and related to Art's contention that "anything will lase if hit hard enough." They experimented with various flavors of Knox Jello in an effort to make an "edible laser." Finally, they added sodium fluorescein to clear gelatin and, lo and behold, when pumped by an ultraviolet laser, green laser light was produced. Later, colleagues at Bell Labs used gelatin film to demonstrate the first "distributed feedback laser", a form of laser which today plays an important role in optical communications. The second contribution was the seminal idea of cooling gas atoms by laser radiation pressure. This method was developed at several laboratories and used to cool gas atoms to almost absolute zero, leading to the award of the 1997 Nobel Prize in Physics to three of their friends, Steven Chu at Stanford, Bill Phillips at NIST, and Claude Cohen-Tannoudji in Paris.

Along with these many scientific successes and accolades, Art and Aurelia lived graciously with a heart-rending sadness, the care of their nonverbal autistic son, at home and later at medical institutions. While in Sweden, at the time of the Nobel Award, Art learned of a hand-held communicator, and with that device and special calculators they were able to improve communication with their son. Art and Aurelia later helped to organize a nonprofit corporation, California Vocations, a group home for autistic people. A further tragedy was the death of Aurelia in 1991, while on her way to visit their son, living about two hundred miles from Palo Alto.

It has been my good fortune to also work in laser spectroscopy, and to keep in close contact with Art Schawlow over the years. He has been a valued friend and inspiration, and one constantly remembered for helping to bring to the world "A Wondrous New Light."

Boris P. Stoicheff
University Professor of Physics, Emeritus

April 1998
Toronto, Ontario,
Canada

INTERVIEW HISTORY--Arthur Schawlow

Arthur Schawlow, winner of the Nobel Prize for Physics in 1981 for his contributions to the development of laser spectroscopy, and a Californian since 1961, is a stellar member of the group of fine scientists, in particular physicists, who came west in the sixties, often redirecting their research focus at mid-life. Leaving behind a career that had been centered at Bell Telephone Laboratories, Professor Schawlow chose to bring his work and his family to Stanford University. There he taught and took on administrative responsibilities, and with his students and his colleagues completed major research that has advanced the knowledge and applications of laser science and spectroscopy.

This brief interview history will not summarize Arthur Schawlow's achievements. Boris P. Stoicheff has done that very well in the introduction he has graciously contributed to the memoir. A two-page biography, extensive bibliography, and other documents appended demonstrate the range and extent of the scientific pursuits and publications of Professor Schawlow. It is assumed that an historian of science will refer to Professor Schawlow's writings for a more precise chronology of the development of laser spectroscopy.

In undertaking to conduct an oral history with Arthur Schawlow it was my particular ambition to articulate the excitement and energy of moments of discovery, the life of the laboratory, and the total commitment to the work that informs the science of Arthur Schawlow. All that, as well as to get onto paper his humor and rare personal qualities. And I was not alone in such an ambition.

The reason Professor Schawlow agreed to the request of the Regional Oral History Office to do an oral history, to take the time, and to summon up the emotional fortitude often required for the interviews, as well as the time for checking and editing, was because for him the manner in which one puts the point across is keenly important to the story, whether in talking to the interested general public or his students or his peers. He wanted to illustrate the pleasures of his profession. He had begun to write an autobiography, and he felt that doing an oral history would facilitate that writing.

My first encounter with Professor Schawlow was through interviewing Charles H. Townes. Arthur Schawlow wrote the introduction to the 1995 Townes oral history, *A Life in Physics: Bell Telephone Laboratories and World War II; Columbia University and the Laser; MIT and Government Service; California and Research in Astrophysics*. The two men are colleagues--originally Schawlow was a graduate student at Columbia working with Townes--and they are co-authors, and related through marriage. That rare combination of relationships is very strong.

Here is what Schawlow wrote in his introduction to Townes: "It was Frances Townes [the wife of Charles Townes] who made sure that I became acquainted with Charles' younger sister, Aurelia... Although everything I have done in physics since then has been enormously aided and influenced by what I learned from Charles Townes, I have to say that meeting Aurelia was the best thing that happened to me in New York." That quote, with its underlying humor, is not hyperbole.

However, as the reader will learn, the life that Arthur and Aurelia Schawlow shared required far more than the usual wedded commitment because of the sad and very difficult practical family problem for them, and their daughters, of the quality of the life of their severely autistic first child, their son, Arthur, Jr. This issue is discussed fairly openly in the oral history. It is still very emotionally charged, very present, and it requires much of Professor Schawlow's time. The death in an automobile accident in 1991 of his wife Aurelia doubled his responsibility for his son, and dimmed the light of his life.

The oral history interviews began with my first meeting Professor Schawlow at his two-room apartment in Palo Alto. From there we set off in his car to his office at Stanford. I was immediately struck by the sheer amount of technology he surrounded himself with. Both locations were replete with computers, terminals, hookups, cables and tables, and books. It came as no surprise that he was adept with this technology, and that his office was bristling with it, but the fact that he lived so much in its midst at home was striking.

Equally striking, and completely delightful, was the mitigating presence of an impressive jazz music tape and CD library lining his living room walls. The sound of music leavened the table-top technology. After our two-hour morning interview sessions were finished sometimes I would be treated by Professor Schawlow to a particularly choice musical interlude, always jazz, perhaps taped from a live performance using clever, sensitive, and pocket-sized equipment! I was the recipient of the gift of two of the best tapes, technically, that anyone has ever made for me, and I listened to Bob Crosby's band as I drove between Berkeley and Stanford for our eight interviews, from August to November, 1998.

Doing an oral history, delving into the past, reviewing struggles and successes and looking at causes and outcomes, usually amounts at the very least to a diverting experience for the interviewee. However, I would say that our interviews, productive and pleasant as they very definitely were, could not distract from a feeling of a the missing center and balance in Professor Schawlow's life in 1996. He had only just moved from the Schawlow's family home in Palo Alto to a retirement community. Limited space, social readjustments, and relearning the bachelor life after long married years--dealing with such household practicalities as acquiring a sink big enough to wash a pot in--this stuff challenged Arthur Schawlow's natural good humor.

Having said all that, it was also manifest that Professor Schawlow was not disappearing into retirement. During our interviews he was and certainly continues to be much in demand. Attending meetings and what appears to be an endless cycle of award presentations kept him flying more than he would have wished. Yet when it came time to edit the transcripts that I had reviewed and organized with chapter headings, Professor Schawlow was very responsive to my queries, meticulous--not changing the text, but clarifying the meaning. If there are any errors in the oral history it is our fault, not his.

Laser, spectroscopy--these words are associated with intense, bright searching light, healing light, measurement, the furthering of knowledge. The reader will meet a man who has contributed his life to the search, and get a good sense of how he thinks, how he picks his problems, how he goes about solving them, and how he delights in the challenge.

The Regional Oral History Office, a division of The Bancroft Library, was established in 1954 to record the lives of persons who have contributed significantly to the history of California and the West. Other oral histories in science and technology are available through the Office, which is under the direction of Willa K. Baum.

Suzanne B. Riess
Interviewer/Editor

May 1998
Berkeley

ARTHUR L. SCHAWLOW
(Biography)

Arthur L. Schawlow was born in Mount Vernon, New York. He received the Ph.D. degree from the University of Toronto in 1949. After two years as a Postdoctoral Fellow and Research Associate at Columbia University he became a Research Physicist at Bell Telephone Laboratories. In 1960, he was a Visiting Associate Professor at Columbia University. Since 1961, he has been a Professor of Physics at Stanford University. He was Chairman of the Department of Physics from 1966 to 1970; Acting Chairman, 1973-74, and in 1978 was appointed J.G.Jackson and C.J.Wood Professor of Physics.

His research has been in the field of optical and microwave spectroscopy, nuclear quadrupole resonance, superconductivity, lasers, and laser spectroscopy. With C. H. Townes, he is coauthor of the book, ***Microwave Spectroscopy***, and of the first paper describing optical masers, which are now called lasers. For this latter work, Schawlow and Townes were awarded the Stuart Ballantine Medal by the Franklin Institute (1962), and the Thomas Young Medal and Prize by the Physical Society and Institute of Physics (1963). Schawlow was also awarded the Morris N. Liebmann Memorial Prize by the Institute of Electrical and Electronics Engineers (1964).

Dr. Schawlow received a Nobel Prize for Physics in 1981 for "his contribution to the development of laser spectroscopy."

Schawlow was named California Scientist of the Year in 1973. In 1976, he was awarded the Frederick Ives Medal of the Optical Society of America "in recognition of his pioneering role in the invention of the laser, his continuing originality in the refinement of coherent optical sources, his productive vision in the application of optics to science and technology, his distinguished service to optics education and to the optics community, and his innovative contributions to the public understanding of optical science." In 1977, he was awarded the Third Marconi International Fellowship. Schawlow also received a Golden Plate Award from the American Academy of Achievement in 1983. In 1991, he received a U.S. National Medal of Science for "his role in the conception of the laser and advancing its applications, particularly to laser spectroscopy."

In 1982, the Laser Institute of America established the Arthur L. Schawlow Medal for laser applications, to be awarded annually. The first medal was awarded to Schawlow "for distinguished contribution to laser applications in science and education." The American Physical Society established the Arthur L. Schawlow Prize for laser science in 1990. In 1996 he became a member of the American Inventors Hall of Fame, and also received the Ronald H. Brown American Innovator Award from the U.S. Department of Commerce. He also received the Arata Award of the Japan High Temperature Society.

He has received honorary doctorates from the University of Ghent, Faculty of Science, Belgium, 1968; University of Toronto, Canada, 1970 (Ll.D.); Bradford University, England, 1970 (D.Sc.); University of Alabama, USA, 1984 (D.Sc.); Trinity College, 1986, Ireland (D.Sc.); University of Lund, Sweden, 1988 (D.Tech.): Victoria University, Toronto, Canada D.S.L. (1994). He is an Honorary Professor of East China Normal University (1979).

Schawlow is a Fellow of the American Physical Society (Member of Council, 1966-1969), the Optical Society of America (Director-At-Large, 1966-1968), the Institute of Electrical and Electronics Engineers, the American Association for the Advancement of Science, the American Academy of Arts and Sciences, the American Philosophical Society, the Institute of Physics (Great Britain), and a Member of the U.S. National Academy of Science. He was Chairman of the Division of Electron and Atomic Physics of the American Physical Society (1974), President of the Optical Society of America (1975), and Chairman of the Physics Section of A.A.A.S. (1979). He was President of the American Physical Society in 1981. He was Chairman of Commission C.15, Atomic and Molecular Physics (1978-1981), and Chairman of the U.S. National Committee for the International Union of Pure and Applied Physics (1979-1982). In 1983 he was elected one of six Honorary Members of the Optical Society of America. He is an Honorary Member of the Gynecologic Laser Society and of the American Association for Laser Medicine and Surgery. He is also an honorary member of the Royal Irish Academy (1991).

Dr. Schawlow wrote the introduction for ***Scientific American Readings on Lasers and Light***, and three of the articles in that collection; he is author or coauthor of over 200 scientific publications. On television, he has appeared on one of the 21st Century programs with Walter Cronkite, and one of the Experiment Series with Don Herbert, as well as on films for Canadian, British, Japanese, and German T-V networks.

April, 1998

I BACKGROUND AND EDUCATION, TORONTO

[Interview 1: August 14, 1996] ##[1]

Schawlow Family, Toronto Childhood

Riess: Please start in at the beginning and tell me what you can about your parents. You said last time that you didn't know that much family history, but you'll want to include what you can recall.

Schawlow: Yes. I was born in Mount Vernon, New York--and my birth certificate says so--on May 5, 1921. We didn't live there very long: my parents moved, I understand, to New Rochelle, and then when I was about three years old, they came to Toronto, Canada.

My mother was born in Canada, grew up there, and she never wanted to talk much about--neither of my parents wanted to talk much about their early life. I think it was a fairly large family, because occasionally we'd meet a brother or sister who'd come to town and visit. But she claimed that her father was a mounted policeman at one time. I talked with a cousin who claimed that they were all farmers. I don't know. She said she was born in Petacodiac, British Columbia, which is a small town, which might have been a place where a mounted policeman would be living, but she grew up in Pembroke, Ontario.

I think that her mother died when she was born, and her father died about six years later. I think he remarried, but then after he died the family was broken up, and she lived with various people at various times, sometimes with her sister Mary, and that was not happy at all. Mary was an older sister, considerably older. She spent some time in a convent school. I think their family was Catholic, but she wasn't by the time I

[1]## This symbol indicates that a tape or segment of a tape has begun or ended. A guide to the tapes follows the transcript.

met her [laughter]--knew her, rather. So I don't know much about her.

Cecilia lived in Pembroke; we knew her then, and she was my mother's favorite sister. Cecilia made wonderful doughnuts, I remember. And she had a son, Heber, who was about my age. We visited them in Pembroke a few times, and had a very pleasant time.

During the war, Cecilia's husband, Percy Jessup--her married name was Jessup--was a plasterer. But he moved to Toronto and I think he had a job as a guard or something at a war plant, and then he died. They lived in the outskirts of Toronto for some time. My mother had another brother, Dan--both of these were considerably older--and Dan worked in a transformer factory, General Electric Transformer. He was a millwright; I didn't know what that meant, but I think now it means a man who moves things around the mill, does the heavy moving. He was a bachelor for a long, long time and he used to come to Sunday dinner very often, and he'd usually bring a brick of ice cream. That's the way ice cream came in those days, usually from a drug store--that was the only place that was open on Sunday--the ice cream was in bricks.

Riess: How did your mother meet your father?

Schawlow: Well, it's a rather mysterious thing. On my birth certificate her name is listed as Helen Mason, and her brother's last name was Carney. But I think some of the other brothers, younger brothers or half-brothers, are named Mason. So I don't know how that happened. But at any rate, I think for a while she worked as a practical nurse or assistant to nurse. Then she went to New York, I think to work with somebody there as a sort of nursing assistant. During the war, Metropolitan Life Insurance Company was very short of help and I think she worked with them, and that's how she met my father.

My father had come from Latvia. He was born in Riga, and I don't think he was legally in the United States or, for that matter, in Canada.

Riess: Was he an ethnic Latvian?

Schawlow: He was Jewish. We didn't know it at that time. He didn't tell us until we were grown up. There was a lot of anti-semitism in Toronto. I don't think there were any Jewish professors. So my mother brought us up as Protestants in the United Church of Canada, which was formed in 1925, I think, as a union of the Methodists, Congregationalists, and half the Presbyterians--the

more fundamentalist Presbyterians continued as the Presbyterian Church. We were brought up as Christians, and I didn't know my father was Jewish. I don't think he was really religious, but his background was. And he didn't tell me until I was about seventeen or something like that.

Riess: What was the occasion for telling you?

Schawlow: I don't remember exactly, but he did tell me.

Riess: When I ask about being an ethnic Latvian, the Latvian culture is very strong, and full of traditions. I wondered if he had any of that.

Schawlow: Well, it's hard to tell. He certainly didn't show it. I think there were a number of people in the Baltic states who were of Germanic origin, and particularly Jewish people, and they probably behaved more as German Jews than as Latvians. But this is conjecture. He told us various stories when we were little, which I didn't believe, like he said he came from Georgia and then he talked later about skating across the ice to go to school. [laughs] Well, that didn't fit together, but somehow I had more respect for my parents and I didn't question that.

Riess: Was he humorous?

Schawlow: Yes, at times. He worked very hard.

Let's go back into his history, what I know. He certainly had some mathematical ability, and he wanted to be an engineer. So he went to Darmstadt in Germany to study, and he got there too late for the start of the term, so he went on to visit one of his brothers in the United States. He, too, had a large family--this was the only one of his brothers I ever saw, his brother John, whose name was Schwartz. I'm told that there was some kind of a scandal and he changed his name. He ran a tobacco store and news store in Lambertville, New Jersey. Some of them changed their names: one's a Shaw, somebody's a Low, and I think there's even a brother in South Africa. There was one in Baltimore--I have my father's watch, a gold pocket watch which says "Welcome to Baltimore," and I think it's dated 1910 or something like that.

Riess: Schawlow was the original. Your father kept the name.

Schawlow: Yes, I have his birth certificate, which is in Russian, actually. Latvia was controlled by Russia in those days. It was only free for a while between the wars, I think, and then again recently.

Riess: So he came to visit his brother since he couldn't matriculate.

Schawlow: Yes, that's right. And then he got this job with the insurance company, and that's how he met my mother.

As I say, I don't think he was really legally in the United States. Coming to Canada, they told me at one point that you couldn't come with a job, it wasn't allowed. So he resigned from Metropolitan Life. Then after he got to Canada the resignation was declined, so he was able to go back to work for Metropolitan Life Insurance. He was very good, he was one of their top people in the office. He at one point was an assistant manager.

Riess: You said that he had mathematical abilities. This is what he was using in his job?

Schawlow: Well, I don't think he could use very much of it. It was a horrible job. He had to go out every night to collect, because the basis of the Metropolitan Life Insurance Company, which at that time was the largest--it was so-called industrial insurance, which was weekly premiums for the working man. He'd go out and kind of collect a quarter here, a nickel there. Particularly during the Depression, it was very hard. But he never lost his job and managed to scrape through. And we never felt poor. We sort of knew what we could do, and we were always well-fed and clothed.

Riess: It sounds like both parents, in a way, made a move that denied their religious background, and a lot of their background. I think that would be hard for them.

Schawlow: Yes, I suppose so. I think it must have been, although we never really did get to discuss it.

Riess: And your sister is older than you?

Schawlow: Yes. I like to joke that I was named after my sister, about a year and a half after. [laughter]

Riess: That's cute.

Schawlow: She's tired of hearing that. Her name is Rosemary Wolfe. Her husband was a professor of geography at York University in Toronto. He's been retired for some years, and they still live in Toronto. She got a bachelor's degree in English literature and got a master's degree, and then later went back and got a library degree and worked for a while in libraries and for a while in bookstores. But she hasn't worked for a long time.

Riess: What are your earliest memories? Do they go back to Mount Vernon at all?

Schawlow: No. I understand that when we came to Toronto we briefly had an apartment or a flat or something on Pape Avenue, but I don't really remember that at all.

Riess: What avenue?

Schawlow: P-A-P-E. Then we moved to 408 Sackville Street, but we weren't there very long because most of the time we were there it was at 436 Sackville Street. We lived there until I was eleven. Just about when I was going into high school, my father moved to a different office, which was then on the edge of town--actually in York Township, which was a separate municipality. There is now a metropolitan government.

Riess: Reading the autobiography that you've written, I wondered whether you ended up with a sense of moving around all the time and uprootedness?[1]

Schawlow: Well, a little of that, yes. This early stuff didn't make much impression on me, I was too young. But moving to the suburbs was hard. I had a very close friend next door, Gordon Kendall, and it was a sort of wrenching experience. We would talk very frequently on the phone for a while, and then gradually lost contact.

Riess: You described a back yard in one of the houses that could become an ice rink.

Schawlow: That was at 436 Sackville Street. It was hardly bigger than this room, actually. It was very small. But the winters, sometimes--not every year, some years--it would be cold enough, you'd just flood it and you'd have ice. You couldn't skate very far because, as I say, it was small--oh, maybe twenty feet square.

Interestingly enough, the house is still there. I went back a couple of years ago and took some pictures. The only thing that's changed from the outside is that they have a veranda or porch all the way around two sides--it's on a corner--and instead of having a wooden railing, it now has a metal railing. That's about the only thing I could see that obviously was changed from the outside.

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[1]Arthur Schawlow had begun an autobiography that he loaned to the interviewer for background information.

We had the ground floor. There was an elderly couple living upstairs, the Duffs. He, I gather, was a member of the MacDuff family from Scotland, sort of an aristocratic family. We heard a rumor that when he married his wife, that somehow or other they disowned him. Anyway, I don't know about that. He was a lawyer, worked for the city. They were pleasant people, but we didn't have much to do with them.

Riess: Toronto, to the extent that I know about it, and it's mostly from literature, is a kind of immigrant city.

Schawlow: Even more so now, yes. Well, at that time, yes. See, this was not long after World War I, and there was a lot of immigration from the British Isles, so a lot of English, Irish, Scotch--"Scottish," as they prefer. In fact, somebody was telling me yesterday that he asked somebody if he was Scotch, and he said, "Either Scottish or a Scot. Scotch is something you drink."

We did have some good friends, like the Anguses, that were real Scots, and I've always felt a liking for Scots since then. Both their children were deaf, Elma and her brother were pretty deaf, but Elma became quite expert at Highland dancing. We were invited once to watch her rehearse in a living room, oh, I don't know, maybe fifteen feet square, and there was a bagpiper. I've never heard any noise as loud as that! A bagpiper in a little room like that!

Riess: [laughs] Was that before or after you had your tonsils out? Maybe it affected your hearing?

Schawlow: That was probably after, I think. [laughs] I don't think that one evening, an hour or so of bagpipe, would have affected my hearing.

Riess: Another incident you talk about in the autobiography was when you were rescued by a babysitter. Was this seriously a near drowning?

Schawlow: Yes, she thinks so. [chuckles] I wasn't worried. I felt I was all right, but apparently I was getting in over my depth. It was in the lake, Lake Ontario, which is an enormous lake. It was a beach at a town called Scarborough. I don't know, I wasn't worried, but she came out and grabbed me, and maybe I would've drowned otherwise. But I didn't feel that I was drowning. Helen is still around, I saw her a year or so ago.

Riess: Helen was the babysitter.

Schawlow: Yes, Helen Egan--and her brother, Vincent, and I used to play together.

Religious and Cultural Milieu

Riess: Where has this background left you with religion?

Schawlow: Well, I'm a fairly orthodox Protestant. I've been in a lot of Protestant churches. I have to laugh--I don't know whether I put it in there [autobiography]: one time, Vincent Egan said, "You're a Protestant." And I said, "I'm not, I'm an American." I'd never heard the term Protestant before. But as we moved around we were always in the United Church--when we were in Toronto. And when I went to New York I went to the Riverside Church, which is affiliated with the Baptists but really is nondenominational.

Then, after I got married, my wife got a job as organist and choir director of the Baptist Church in Morristown [New Jersey]. This Baptist church is not at all what you think of as Baptist; it's a very liberal, Northern Baptist church. The minister was very much interested in interracial friendships and interfaith and so on. So we went there. Then, when we came out to California, after a while Aurelia got a job at the Congregational Community Church in Ladera, which is on the outskirts of Palo Alto.

After we had the third child the job was too much, so we started going to a Methodist church in Madison, New Jersey, while we were still in New Jersey. That was ok. But then we moved to California. We've been in Presbyterian and Congregational churches around Palo Alto. Recently my son and I both joined the Methodist Church in Paradise, California, and that's the only one I go to now.

So, I don't know--I don't like to be pushed on what exactly I think about religion, because I think a lot of it I don't know. But I think the world is too wonderful to have just happened. And I think that orthodox Christianity is a good conduct for life, and I hope it's true.

Riess: And I don't mean to push you at all. I guess maybe one of the ways that I would ask about how religious one is, is whether in a crisis you really pray to something.

Schawlow: Yes, I do. I'm never sure--in fact, I say my prayers every night. I really don't know for sure if there's somebody listening, but it seems to help. Somehow, I feel that there's somebody else in charge.

Riess: Well, yes, the alternative is the hardest.

Schawlow: There is a book--let's see, it's called *Cosmos, Bios, Theos*. It was by [Henry] Margenau and [Roy Abraham] Varghese. Professor Margenau, who had retired from Yale University, wrote a number of people and sent them a questionnaire about religion. I answered, and I have a page or so in that which I can probably dig up for you. I think the copy is somewhere around here. We can look later.

But I haven't gotten around like Charlie [Charles Townes] has, giving talks about religion. I remember once there was a wonderful minister filling in at this church in Ladera. He asked me if I would like to preach a sermon some Sunday, during the summer particularly, I think. I said, "Well, it reminds me of the sign at the barber shop. It says, 'We have an understanding with the bank: they don't cut hair and we don't cash checks.'"

Riess: [laughter] That's good. Actually, I would find that somewhat disconcerting. After all, a role thing is very important there, to maintain the ministerial role.

You said you felt your father had a kind of mathematical ability?

Schawlow: Well, I mostly saw him on arithmetic. One of the horrible things about that job was that every week they had to prepare their accounts, and they had to list every single policy on a great big sheet of paper, I don't know, maybe two and a half feet square or something like that. And they were long columns, and you'd have to move the policies from one week to the other as they were paid up. Then they have to add up all these columns, and the differences had to equal the amount of money that they turned in as they moved from one week to the next--it was marked as paid up. So he had to do a lot of addition.

When I was in high school I used to help on that sometimes, and I think I got pretty good at addition. He knew something about geometry, but we didn't discuss it very much. He could always beat me at chess--especially if we bet even one cent, he would beat me. But I didn't take chess very seriously. I got a book and studied it some, but I have never taken games very seriously.

He would've made a poor engineer, I think, because he had no feeling for mechanical things. My mother used to do any repairs that had to be done around the house--often in a way that sort of shocked you really, because it was rough and

ready: whatever was at hand, she'd string things together with it. I think she might've made a better engineer. I think he could've become a theoretical engineer or a scientist, and it's perhaps a pity that he didn't.

Riess: What other kinds of things do you remember doing with him?

Schawlow: Well, he was very busy, of course. We'd go for drives and occasionally walks--he would drive us out in the country. We did play chess some. And I don't really remember anything else very much.

Riess: It sounds like he worked very hard. Maybe there was a sense of, "Your father is working. Don't disturb him."

Schawlow: Well, he had to go out essentially every evening, because that's when people were home. He had to make these collections. He didn't have a lot of time.

Riess: Did your house have books, music? What was the ambience?

Schawlow: We did have a Victrola that somebody gave us at one time, a windup one, and we had a few records, I think, that had come with it. For a while somebody lent us a reed organ, and I tried to take piano lessons and practice on that, but that was hopeless, you can't play piano stuff on a reed organ. We even had a piano that somebody lent to us for a while, but I don't think we felt that we could afford to buy a piano. No, there wasn't a lot of music around the house. Oh, we had the radio, and of course, that was a wonderful thing, there was all kinds of music on the radio.

I had asthma when I was a boy. We used to go to a farm in the country for some weeks in the summer, but then I started getting asthma very badly from an allergy to ragweed. They did tests, and they gave me shots for it. Eventually, I outgrew it. I think what happens is--I've been told that the irritated linings of the bronchial passages don't go away, but they get bigger so there's room for the air to flow through.

But because of this asthma, somebody suggested I should take singing lessons. And I did take singing lessons from a very good teacher. She never told me that I really couldn't sing. I had a good voice, but I couldn't carry a tune, really. I just have a poor tonal memory.

I did sing for a while in an Anglican church boy's choir--[laughs] that's another of my religious variations. It was a small church, not a big one. I think I had a nice boy's soprano voice. It used to bother me that things didn't sound

right to me, but I couldn't tell what was wrong. She was very good. One of her sons had a somewhat successful career as a singer in the United States. The other one was an artist. She was Mrs. Louise Tandy Murch, and she lived to be almost a hundred. My sister sent me a newspaper clipping about her. But I lost touch with her when we moved out to the suburbs. I think the Depression was really beginning to bite, and my parents said I had to stop the singing lessons. Well, it didn't matter too much because I really wasn't much of a singer.

Riess: And did that really help the asthma? Was the idea that you learned a different kind of breathing? What was the point?

Schawlow: I don't know. I guess that you exercise your lungs and so on, maybe build up lung capacity.

At that time, under Mrs. Murch's influence, I thought there was no music but classical. We didn't listen to an awful lot of anything, to tell you the truth, but there was a lot of light classical music on the radio in those days. I remember there was a program on Sundays by Ernest Seitz who played the piano, light classical stuff. He and Gene Lockhart, who later became a successful movie actor, wrote "The World Is Waiting for the Sunrise."

Early Interest in Engineering and Science

Schawlow: There was a library branch within about a half a mile or so, and particularly in the summer we'd go over there and get as many books as they'd let us take out--I guess it was six or so at a time--read through them and bring them back and get some more. So I read a lot of books.

Riess: What were you reading?

Schawlow: I was interested in things concerned with engineering and science.

Riess: We're talking about little Artie. Little Artie?

Schawlow: I was never called Artie. My family called me Bud, and they still do. But yes, even then I had those interests. Once I started to use a Meccano set, I started to read Meccano magazine and that had stuff about building bridges and that sort of thing. I was interested in radio, although I didn't have any money to build anything much. I think I built a crystal set. And then we also read a lot of books, oh, of

mythology--The Iliad and The Odyssey, and some of the Norse legends, too.

Riess: And adventures?

Schawlow: Yes. There were some good books. There was a series of books about a Boy Scout named Roy Blakeley, I think. I don't know what the kids get now, I don't see any such things. These were good for, well, going on towards teenage. I read a lot of Jules Verne.

One thing I didn't mention about the cultural background: at home we had the Book of Knowledge. It was a wonderful thing. It had summaries of a lot of famous stories, so I got some idea of what they were about. I spent a lot of time reading that.

Riess: How is that different from an encyclopedia?

Schawlow: It's not written as an encyclopedia. I don't remember how it's arranged. Actually, I found a copy in a used book collection and bought one, left it up in Paradise a couple of years ago, but I haven't looked into it. Well, it was almost more like a magazine, or collections of articles on various subjects and stories. There were some stories, as I say, some summaries of famous stories.

Riess: Was it a series?

Schawlow: Well, it came out all at one time, but it was a set of books.

Riess: Did you have an encyclopedia?

Schawlow: I don't think so, no. I don't think we had an encyclopedia.

Riess: What do you think: if you had been given a chemistry set instead of a Meccano set, where would you be today?

Schawlow: Oh, goodness. I did play a little bit with chemistry sets at one time or another, but they didn't really intrigue me so much.

It was radio, really, that intrigued me, and I read a lot of books about radio even starting then. And there were people who had old radio magazines that I could get and read through some of them, I think even when I was on Sackville Street--I left at age eleven, but I'd finished grade school by then.

Riess: Let's go back to the grade school years. You were skipping some grades in school.

Schawlow: Yes, I was. Until I met Miss Bray.

Riess: Were you head and shoulders above your classmates? Why did they push you on so? Now they tend not to do that kind of thing.

Schawlow: I don't know. I guess I could do anything that they put in front of me, and I had a good memory at that time, I could learn things fast.

I don't really know. I guess I was a lot better than most of the others. One thing I do remember, and I think it was a very good thing, when I went to the Model School I was a couple of years younger than most of the others in the class, and it was a selected group, too. I felt that some things I could do better than them. Still, it kept me from getting a swelled head, thinking I was smarter than everyone else.

I've known a number of scientists who apparently were the boy genius all their life, and they're really pretty arrogant. But I learned that there were other people that are pretty good, too. I'm not very competitive; in fact, I think I'm about the most uncompetitive person you ever saw. And I avoid competition--probably one of the reason I don't like games: I don't like to lose and I don't like to see somebody else lose, either. So I never really worried too much about what others were doing, I just did what I was asked to do--didn't go much beyond it, either.

Riess: I guess a lot of physicists and engineers have a love of radio as the beginning of their life story.

Schawlow: It was so exciting, really. I remember when we got our first radio--it must have been about 1925 or 1926, and it was battery-operated--all the kids on the block would come around to listen to "Santa Claus' Adventures on the way from the North Pole," sponsored by our local department store, Eaton's. Also, the newspapers had articles every week on how to build radio sets with circuit diagrams. There was a lot of excitement.

Riess: You were offered the means to make this thing.

Schawlow: It was wonderful. The radios were made out of standard parts, and you could put together almost anything that was known then out of standard parts. For a while, you could build things cheaper than you could buy them. But then eventually they got into mass production and it really wasn't possible to do it. Well, people moved to the short waves, whereas the broadcast

band was pretty much standard factory items. People built their own short wave sets, and I did too a little bit.

Riess: Can you remember struggling with the concept of radio waves, of how they were was transmitted?

Schawlow: No, I can't remember struggling with it.

Riess: You understood it right away?

Schawlow: Either I understood it, or I didn't worry about it. [chuckle]

Riess: I'd sort of like to know.

Schawlow: I guess I understood something. [pauses] I may have gotten something out of that *Book of Knowledge* about it; they may well have had a section on radios and how they work. No, I don't remember ever worrying about it. But my knowledge was not very deep.

Riess: I'm interested in your general curiosity as a kid. For instance, when you're out taking a ride with your father in the car, and you see the telephone wires looping down the highway, does that make you start to think about--?

Schawlow: It does more now than it did then. I remember, maybe twenty years or so ago, I was in England taking a ride on the train. It was an electric train, and I was thinking, "What a marvelous thing it is: this invisible electricity flows through here and moves this huge train." I guess I had a sense of wonder and interest all along the way, but I learned it in little bits and pieces.

Riess: You're saying that it was the sheer pleasure of building things that was more appealing?

Schawlow: I think understanding things was more appealing, but then building, too. I really wasn't very good at building because I was very clumsy. And I didn't really have a lot of money to spend on it, either. Building it and having something work, and produce some music out of the air--that was pretty exciting.

Riess: Dealing with what you describe as your clumsiness was--you obviously surmounted it.

Schawlow: Well, I got people to do things for me. [laughs]

Riess: Is that really true or is this just some kind of legend that you have of yourself?

Schawlow: No, it's true. In fact, my students and technicians don't want me to touch the equipment some of the time. I learned some tricks to do things, finally. I realize the reason now why I don't like mice on computers is that you have to position the pointer, the cursor, exactly, and I find that hard to do. I really find it hard to get that thing placed exactly where it's supposed to go. I can do it, but it's not easy.

I don't think I ever passed in art class; however, they let me through anyway. As I think I wrote down in that draft for a biography, when I got to high school I had to choose between either taking art and botany, or bookkeeping and typing. I knew I couldn't pass art, so I took bookkeeping and typing because I really am very clumsy.

Riess: That is a surprising anecdote to me, because you were obviously smart, and I should think any school counselor would say you've got to take botany because that's the academic track.

Schawlow: I don't think we had a school counselor then. I'm not sure they'd been invented.

Riess: But it turned out to be a good thing to have taken typing.

Schawlow: Yes, it was good. I was all right. I'm not a great typist; I can type fast, but not accurately. I think computers were invented for me because I can make my mistakes and fix them.

Riess: I noted in your autobiography, when you were talking about using your hands, that a psychologist was consulted. Why?

Schawlow: Did I say psychologist? It was some kind of a doctor.

Riess: [referring to pages of the autobiography] "Someone, I think it was a psychologist, told my mother I would never make my living by my hands."

Schawlow: I see. Well, now I don't know. It might have been just a medical doctor, but I guess she had noticed I was clumsy. When I had this trouble with the teacher in the--I guess you'd call it fifth grade, but it was junior third, they number them junior and senior first, junior and senior second, and so on.

Riess: Please go back and tell that story, because it won't be on our tape. After you'd skipped one grade and skipped another grade, you landed in the hands of--

Schawlow: --this teacher [Miss Bray], a woman who had liked my sister very much, but somehow didn't like me, and claimed I was

stupid, and also claimed I liked throwing spitballs. I had to ask my mother, "What's a spitball?" I really didn't know.

So my mother took me to a psychologist who gave me an I.Q. test. And I hate to give you a number for printing--I can tell you--but it came out as 152. As I say, I hate to put that down in writing because I.Q. tests are very unreliable--I mean, quantitatively: I might have gotten more one day, less another day. Anyway, that's when she arranged for me to go to the Normal Model School. I guess he suggested it, probably.

But as I say, the teacher had said I was stupid--others hadn't thought so.

Riess: Yes!

Schawlow: I guess I can't be sure who it was who had suggested the Meccano set.

Riess: You knew you wanted to be an engineer? Had you met an engineer? Did you know what an engineer really did?

Schawlow: No. Well, I had read a lot of books about engineering, I mean about the achievements of engineers, and I knew about building bridges and highways, and all that sort of thing. But did I know about the day-to-day work where they have to sit at the drafting tables and draw complicated diagrams? No, I didn't know about that.

I did meet one radio engineer, briefly, who was some friend of a friend. And this man had a hard time. He got a bachelor's degree in electrical engineering and couldn't get a job. During the Depression, for a while he was winding coils in a radio factory, strictly a technician's job. I don't know what became of him later, but I knew that wasn't what engineers were supposed to do. I thought they were supposed to invent and design equipment.

Riess: This was a time of a great flowering of engineering, wasn't it?

Schawlow: Well, there was a lot of engineering going on. Of course, engineering had really started in the mid-nineteenth century. I mean, I read about the people who designed the railroads, Isambard Kingdom Brunel, who built the Great Western Railway, and some wonderful bridges, and also the first steamship for running cable across the Atlantic. Much later, when we went to England in the 1970s, I went to Bristol and saw one of the bridges that he had built. I thought that was pretty wonderful stuff.

Electrical engineering, of course, that really didn't begin with Faraday. I mean Faraday's invention of the dynamo was necessary, but it took a while before it really became an engineering thing, not science. But it was--well, electricity and Edison and so on, and electric light distribution things, those were before the twentieth century, I think. They were pretty much underway.

Riess: Did you imagine yourself being a kind of master builder along these lines?

Schawlow: I could imagine myself being a master builder, but I really couldn't have done it.

Riess: Had you heard of physics?

Schawlow: Not very much; I guess I'd heard of it, yes. I was interested in electricity, mechanics, and so on, so I guess I knew that that was the sort of thing that physics dealt with. I know not everybody had. I remember once, during the war-time years I think it was, I met the mother of one of my friends and I mentioned that I was studying physics. She didn't know what that was, thought it had something to do with medicine.

High School, Vaughan Road Collegiate Institute

Riess: I think we're at the point where you made the move to the other neighborhood.

Schawlow: Yes. I went to high school there, yes. And I was, of course, the youngest one in my class, but I didn't have too much trouble with the coursework. I don't know, my sister seemed to think I just breezed through it, but I felt I was working. I always had a lot of things to occupy me: I was still interested in radio and beginning to build a shortwave set, a two-tube shortwave set, things like that.

Riess: Did you always do that from magazines and kits, or did you have some mentor who helped you?

Schawlow: Not kits. Mostly magazines. When I was about mid-way through high school I met a man named William James Crittle. He was a radio technician, really. He had been gassed in World War I and was living on his pension pretty much. He was a very enthusiastic radio amateur. I used to go over and talk with him after school quite often. And, as I say, he wasn't really working. I learned some things from him, but at other times I

was shocked by his ignorance of fundamentals. I had mentioned something about the crest of a radio wave, and he thought that was up at the top of the atmosphere. Whereas the crest is the place where the electric field is the maximum of the electromagnetic wave.

Then I tried to build a super heterodyne radio, and it didn't work, so he took it apart and rebuilt it for me. So I never really built a very big radio set; two tubes was as about as far as I succeeded.

Riess: You took Latin, French, and German. Were you good in languages?

Schawlow: I don't know. I had no trouble, and I was always near the top of the class, but I never learned to speak any languages--well, they didn't really try to teach you to speak. I'm not like these kind of people who pick up another language every year, but I never had any trouble with it. I always could do very well with what we were asked to do. I tended to do that with my coursework; whatever I was asked to do, I did. But I didn't go beyond it much.

Riess: Except in the things that you loved? The physics and chemistry?

Schawlow: Well, the physics and chemistry, I read a lot around them, but I didn't really try to go deeper into the particular things that we were being told to study. In the third year of high school we started to take a physics course, from a man named Harston who obviously didn't know very much. He was also the part-time physical training instructor. It was all right, but not very stimulating. The fourth year, I think we took chemistry. And then the fifth year, chemistry and physics.

Those last three were from a man named Robinson, C.W.T. Robinson, who was known to everybody as "Speedy," because he had a rather slow way of talking--although amazingly, he had been a fighter pilot in World War I. We had five years of high school, thirteen grades in Canada. I think they still do, but I really don't know why because the Americans, at least those that come to Stanford, are just as well-prepared as we ever were. But perhaps I couldn't have taken so many languages if it hadn't been for that. Anyway, in the last year he just told me to do all the problems in the book at my own pace. That was pretty good, so I learned everything that was in that textbook; but I didn't try to get another, more advanced textbook or anything like that. I sort of read the popular accounts of what was going on.

In high school mathematics I was at the top of the class, could do very well. Got to university--it was much tougher. There were people there who really had mathematical talent--I had to struggle. And then when we got on toward the fourth year, the last year of college--I don't know, it's fortunate that we didn't finish the year, because the war was on and they put us to work teaching classes--I found that physics was getting very mathematical, and I didn't like it.

I liked to visualize things, and I think that's one of my abilities--although I haven't got a good eye. I always tell people that I think in terms of fuzzy pictures, but I'm pretty good at that. I sort of train myself to think, "What's the essence of this? What's this all about?" It got sort of discouraging as the physics became more a matter of equations and formulas.

But then after I graduated I came across this wonderful book by Karl Darrow--I think he called it *An Introduction to Contemporary Physics* [Van Nostrand, 1926]. Karl was a nephew of the famous lawyer Clarence Darrow, and for many years he was the secretary of the American Physical Society. Anyway, this book described the basic experiments on which modern physics was based, what they did and what they found, and that was the kind of physics I liked--not writing out equations.

Riess: That really didn't happen until the end of college?

Schawlow: Yes. Well, it didn't really get that bad until then. I don't know, it seemed like physics, a lot of it was with concepts and learning facts about things, how things worked. But then they sort of get into the more formal mathematical treatment and I didn't like that.

Riess: Physics wasn't sold to you as the underlying principles of everything?

Schawlow: Well, I guess it really wasn't sold to me.

Riess: Sorry, I didn't really mean that.

Schawlow: I don't know. Well, physics certainly seemed already by then to be the basic laws of the way things worked. But for instance, we didn't have transistors, or semiconductor devices, and so it wasn't really fully appreciated the way physics, solid state physics, would open up a whole world of devices and so on. It certainly was the way that structures, like bridges, had to be designed to withstand the stresses--

Some Beliefs, and Some Disbeliefs ##

Riess: [looking at Cosmos, Bios, Theos] Why are people so fond of asking scientists for the answer? After all, they don't ask art historians for the answer.

Schawlow: Well, the man who edited that book, Cosmos, Bios, Theos, was a physicist, and so perhaps that's why he thought of asking scientists.

Riess: But you know it's more than that, too.

Schawlow: Yes. Yes, I guess so. I think that you confront the universe and perhaps learn something about it that wasn't known. And there's, of course, a long history of complaints that science conflicts with religion. I don't think it should. But on the other hand, religion has very often tried to explain the things that we don't understand, and then science comes along and explains them, and they feel, "Oh, boy, God's been moved out of that part of the universe, too."

You know, centuries ago everything seemed magic, we didn't understand anything much. But as we have science we do understand a lot more in a straightforward way. Still, there's so much we don't understand that I think there's an awful lot of room for religion--certainly a guide for ethics. As I think I said a while ago, the world is just so wonderful that I can't imagine it was just having come by pure chance.

Riess: When you say that, "The world is so wonderful," what do you picture right away when you say "the world is wonderful"?

Schawlow: I think the beauty of the trees and flowers and so on, and the fact that people can exist and have produced such marvelous artistic creations, in sculpture, painting, and music. Of course people ask, If God exists, why does he allow such terrible things to happen? And there certainly is a lot of evil in the world--and a lot of good, too. In every family, usually, the parents provide love for the children, at least in most families, and that's a wonderful thing.

Riess: What do you think about afterlife?

Schawlow: I don't know what to think. As I've mentioned even to Charlie, I don't see any place in this universe for a heaven. We've explored it pretty thoroughly, so that if there is any, it has to be very different from anything that we can imagine here. It's not tucked just above the clouds, there, we're sure of that. On the other hand, if you think that the whole human

being is encoded in a tiny bit of DNA, which is so small that you couldn't see it without a microscope, then perhaps the essence of a human being is somehow transmitted to a different sort of universe.

You know, in some ways, I think that the soul, such as it is, is sort of the operating system of the human. It's more software than hardware, in the modern metaphor. Of course, that metaphor may be thoroughly dated in a little while. But you know, there were some people who, I guess, were religious skeptics. They said, "Well, let's weigh the body as the person dies and see if the soul is escaping." I think that doesn't make any sense.

But unfortunately, as you get older it gets harder to feel confident that there's an afterlife, or that it's anything at all like life. Perhaps if I spent more time in church I would feel stronger. One of my daughters has gotten very passionately fundamentalist and would like me to become so, too, but I don't think it's in me.

Riess: Why does it change as you get older? I would think it would work the other way.

Schawlow: It's getting closer.

My mother, too. She sort of lost her faith as she got older. I don't know, really. I guess I'm just honestly saying that I do not know, and I don't think that anybody can know. On the other hand, unless the story of the resurrection is a total lie--and it seems to be well attested--then there are some things that are beyond our ken.

And I don't understand our daughter, this one I mentioned who feels that salvation comes from the sacrifice of Jesus. Well, it's an interesting biblical concept of sacrifice, which is not really a modern concept at all: I mean, why you have to sacrifice something to get a good end, I don't know. On the other hand, if you had to have Jesus die and then be resurrected, that certainly shows you something that you don't get out of the books. Maybe I'll eventually be able to accept the concept.

One of the things that I got, a piece of software, is a Bible search program. I looked up the word "faith," and it hardly occurs at all in the Old Testament!

As far as I understand the Old Testament--I'm not a biblical scholar, but I've been in a lot of church services and

I've heard a lot--I think that some of the Jewish people believed that there were other gods, but their god was the supreme one. I don't think that they really believed that the other ones didn't exist. I don't know--but at least you could read it that way, I think. But there certainly are some strange things. The Bible, of course, is a wonderful guide to human behavior, what works and what doesn't work. There's such a variety of things there.

In church a few weeks ago the minister was discussing the story of Abraham and Isaac, where he was ready to sacrifice his only son. That's a strange story. In the end, I gather God said to him, "Now I know I can trust you" or something like that.

Riess: That's about faith, I guess.

Schawlow: I guess so.

I'm not the person to give you a good religious education, because I just sort of learned. I think I have one principle in doing science: start off believing everything. Because otherwise, I've seen people who are skeptical about everything new, and they don't believe anything, and they miss the boat. But on the other hand, you can question anything. You don't question everything, because then you're just a crackpot, but you can question anything. And so, I guess I tend to have that attitude toward religion. I don't know.

Riess: How do you figure out which thing to question? That's the question in science.

Schawlow: Yes. Partly instinct and partly a matter of seeing what doesn't make sense. If things don't fit together, then you try and see what's missing.

Riess: I spent some time with a book that's been much discussed and reviewed, called *The End of Science* [by John Horgan, Addison-Wesley Press, 1996].

Schawlow: Ooh! What nonsense--absolute nonsense. I haven't read the book, but I read the reviews of it and I think it is nonsense. First of all, I gather it acts as if particle physics is all that there is, and--

Riess: It does. And cosmology--at least in terms of your fields.

Schawlow: Yes, and those are not my fields at all.

I think there are some wonderful questions in atomic physics and condensed matter physics. I'm fascinated now by the questions of nonlocality, where in quantum mechanics things don't seem to be anywhere until you measure them. So you get correlations between distant places more quickly if they start out correlated, and say, two particles move apart in opposite direction--when you measure them, the measurement on one affects what you can measure on the other one. It's considered to be instantaneous, but there isn't really proof of that. In fact, I'm trying to look to see what has been measured and what could be measured. So I think the fundamental questions of quantum mechanics and its interpretation are far from finished.

Riess: The author is provocative. He does quote [Hans] Bethe as saying that important discoveries will continue in solid state physics, but that there are no exciting, big discoveries left.

Schawlow: Depends what excites you.

I've seen particle physics develop kind of as a spectator; it really didn't exist when I was a student. All we had was the proton and the neutron and the electron. Now they have this whole zoo of particles; they have more particles to explain things than the ancient astronomers had epicycles.

Riess: Physics can be a kind of playground for popularizing writers, and for religious writers too.

Schawlow: Anybody's free to speculate anything they want, but fortunately, nature has provided us with a great analog computer, experiment, which will tell us how to solve our equations.

I have read several semi-popular books on the interpretation of quantum mechanics lately. The religious speculations, I just don't see how they can tell me anything that I don't know. But I may be wrong, there.

Riess: Okay, well, let's go back to--

Schawlow: Actually, let me say one more thing about religion. There are enormously different cults and religious sects, and I think it's not unreasonable, because I think God--if he's as wonderful as we believe--is also very complex, and that different people have to see him differently. Of course, like the blind man and the elephant story. But you can't expect a peasant and a philosopher to have the same picture of God. I think God is big enough to cover them all, even for science writers--they can have their picture of God.

Riess: And even if they're trying to prove that he's not there, that means that they're concerned about him.

Schawlow: I don't think they'll ever prove that, any more than you can prove existence. I think we just have to learn to live with uncertainty, and you sort of place your bets on what you think is most reasonable, which is where I come down. Maybe I'm wrong--certainly the Bible complains about people of little faith.

Riess: Is the Bible that is in your computer program the King James version?

Schawlow: Yes. You can get other versions, but I have the King James version.

Riess: At least you get good writing.

Schawlow: Marvelous. Incredibly beautiful writing.

Entering College, University of Toronto

Riess: To the extent that I know you through your autobiography, I think I've let you leap too far forward.

We were getting from high school into college, and the decisions that were involved there, and the choice of subjects that you had. You graduated young from high school.

Schawlow: Yes. I was just sixteen.

Riess: What were the possibilities, in terms of higher education, in Toronto?

Schawlow: Well, there was one university, and as I say, because of money we couldn't even think of going anywhere else. In fact, if we could get into the university, that was going to strain all our resources.

If I hadn't been able to get into the university I would probably have tried to become some kind of a technician, a radio technician or something like that. I don't know--there are schools that teach that, or you can learn it by experience. But, as I say, one didn't think of going to places like MIT. Either you got into the university or you didn't.

I think I wanted to get into the university, and probably thought I would end up teaching high school. It was sort of the thing that I could imagine. I don't think anybody I knew, except doctors or dentists or teachers, had ever gone to college. People who lived around us hadn't. And so I really didn't have much of an idea what it was like.

They have these big formal exams at the end of the last year in high school, which are given by the provincial department of education. They occupy several weeks in June. I thought, "Well, maybe I'm not good enough to get a scholarship," because there are all these schools where they have Ph.D.s for teachers, and so on, like Harbord and University of Toronto Schools, "but I'll see what I can do." Vaughan Road Collegiate was just ten years old, and nobody from there had ever won a science scholarship.

It was 1937 and that was the year of the coronation of King George VI, and there was a possibility that I could have gone with the Boy Scout group to that coronation, but my parents wisely decided that I should stay and take the exams. So I did. When the results were announced in September--they appeared in the newspapers, that's where you learned about them--I found that I'd gotten a scholarship for mathematics and physics. I knew I wouldn't get one for engineering because there were no scholarships for engineering at that time.

Riess: The University of Toronto was not free to the populace?

Schawlow: It was $125 a year, which doesn't seem like much money; even if you give it a factor of twenty for inflation, it would still be only $2500, which is not very much. But these were Depression days, and my father had two children. I think even with the scholarships it was a stretch, and he had to borrow money, though he didn't talk about that. So, $125 a year certainly doesn't seem like much. Before I graduated it went up to $175, but the scholarship covered that. And now I'm sure they're up in the thousands, though not like Stanford or Harvard.

Riess: You said something, back there, about not having any Ph.D.s to teach you. But you went to the top high school in Toronto, didn't you?

Schawlow: No, no, no. It was just the one that was near us. It was a good high school on the whole, but not a great high school. It was the Vaughan Road Collegiate Institute--the "collegiate institute" meant that the heads of each department had to be qualified as specialists in a subject, like in French or English or whatever, so they had certain standards. I really had wanted to go to the University of Toronto school which was affiliated with the university. And that's where a lot of

people from Model School went, but again, my parents felt they couldn't afford it, so I went to Vaughan Road. They covered the material that was described in the course, in the textbooks, but they didn't go beyond that; whereas, I think some of these other schools did give more advanced preparation. However, the exams were based on what was in the course, and I knew that thoroughly.

Riess: About the decision of which part of the University of Toronto to attend--I don't understand how the University of Toronto works.

Schawlow: They had what they called honor courses. It was specialized right from the beginning. I think my scholarship was for mathematics and physics, as I'd gotten high grades in that. I don't remember whether I had to specify that before then, maybe I did. I remember I applied to Victoria College, which happened to be affiliated with the United Church of Canada, but I didn't know that. I asked some teachers and they suggested Victoria College. You had to choose one.

Riess: This is like the British system of a university having colleges.

Schawlow: Yes. Colleges had dormitories and residences, and they had some college life in which I didn't really share because I lived at home and commuted by streetcar. In fact, I only took one course each year, I think, at the college. You had to take some sort of cultural subject that you would take in your college. But the main course was mathematics and physics; except for this one cultural subject, that's all you studied--mathematics, physics, and chemistry. And then after the second year, I think, it branched into physics and chemistry, or astronomy, or an actuarial science. Mathematics had an actuarial science specialty, and many of the top actuaries in the continent's big life insurance companies had graduated from there. We took courses in actuarial science the first and second year.

Riess: Was that in some way like statistics?

Schawlow: Well, yes, it's calculating probabilities. It's taking the life tables, for instance, life expectancies, and calculating how much something is worth based on life expectancy.

Riess: Did this have any general application that you can think of?

Schawlow: No, I don't think so. It was kind of fascinating because it was a lot of talking about what did you really mean here and

formulating the equations that I found attractive, but I felt I never really quite got the hang of it to do it easily.

Riess: Did talk to your father about it? It was sort of in his line.

Schawlow: Well, not really. This is how the insurance companies would set their rates, you know, by taking the probabilities that a person would live so long. It's a strange subject.

I took a terrible chemistry course--I may have mentioned that. This old Englishman named Kenrick taught it. He was the head of the chemistry department, but he hadn't learned anything since 1900, I think. He didn't believe in atoms. He only believed in chemicals, and he talked about a fictive constituent called "hydrogenion"--all in one word, instead of talking about hydrogen ions. Really, what chemistry I learned in high school is about all I learned.

Riess: You said you had good memories of the physics labs.

Schawlow: I enjoyed those. We had a good physics teacher for our first year. He was also about the same age as Kenrick. He graduated from Cambridge around 1905, and he'd written a number of textbooks, but had not done a lot of original research. But he worked hard at preparing problems every week and writing up solutions to these problems for us. He also supervised the lab, with some assistance. He was a very good lecturer--fairly dramatic style and a lot of fun.

We had a wonderful calculus teacher, Samuel Beatty, who later was dean of the faculty of arts and later chancellor of the university. He made things very clear and interesting. Some of the others--most of the other mathematics professors that I encountered were not so good as teachers, but then, perhaps it was because my ability was lacking. But I got through all right: in the first year, I was third in the mathematics and physics course out of about fifty students, something like that; in second year and third year, I was first. By third year, of course, we'd split off into physics, but I was top of the class before they split off.

I felt I had to work awfully hard.

Riess: You said you're not competitive. I don't understand.

Schawlow: I wasn't. But I was scared that I'd lose my scholarship if I didn't get first class honors. And I would have. That was all I really was worried about. Now, looking back, okay, I can be pleased that I was at the top of the class, but the main thing

was that I kept my scholarship. No, I didn't feel I was trying to beat out somebody particularly.

Riess: Was there an opportunity to have some individual time with any of these people you respected?

Schawlow: No, not really. We could go 'round and ask them a question if we needed to.

Riess: Were you learning a lot out of books?

Schawlow: Yes. I guess I was still reading some books about technology and science, sort of popular books about it. But my feeling around the courses at the university was that in high school, I felt I could learn everything that was taught, but in college, I knew I couldn't, so I just had to try and decide which was most important, and try and make sure I learned that well. It was really quite difficult. I felt I had to work pretty hard.

Physics in the Prewar and War Years

Riess: Charles Townes describes--I love the picture, and maybe I've elaborated on it--sitting on a rock by a stream reading about special relativity.[1]

Schawlow: [laughs] I heard that he took a physics textbook with him to the circus once. That's what his sister told me.

Riess: When were you introduced to special relativity? Do you remember struggling with it?

Schawlow: I think we had a course on it. Yes, we must have had that, probably around the third or fourth year. I found it sort of interesting, but not thrilling. I don't know. I guess I could manipulate the equations as I needed it. I've never had the occasion to use it since then, and I'm not really fluent with relativity.

Riess: When you say that, I guess I almost can't believe it because I think of science as a pyramid.

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[1]Charles Hard Townes, *A Life in Physics: Bell Telephone Laboratories and World War II; Columbia University and the Laser; MIT and Government Service; California and Research in Astrophysics*, Regional Oral History Office of The Bancroft Library, UC Berkeley, 1994.

Schawlow: Well, it's a number of pyramids, I think. Relativity does come into atomic physics, but sort of in predigested form. I mean, there are people who have applied relativity to the motion of electrons and atoms. They obtain certain results such as the atomic spin-orbit coupling depends on relativity. But I haven't designed space ships or accelerated particles to relativistic speeds, so I just really haven't had much use for it. Thermodynamics is the same way. We took a course in thermodynamics, but I've never used it. It's a fact that--actually, the old Tower of Babel is there; there are a lot of different branches of physics, and unfortunately, people who write books like *The End of Science* don't understand what we're doing, and vice versa.

Riess: You mean by selecting particle physics as the essence of physics.

Schawlow: Yes. I see how it happened all right. Atomic physics was the way to go in the twenties and it opened the door to quantum mechanics and that, of course, led to a lot of other things. But then you started looking at the fine details of the atom, like the nucleus, and that led you into nuclear physics. Then they started to get accelerators and so then they--

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Schawlow: --started getting new particles, and the whole field of particle physics began. So they felt that they were leading to an essential simplicity.

I haven't followed it closely because it just doesn't seem that they would have anything to offer me. Culturally, it's kind of interesting, but it deals with things in a very artificial sort of way, at very high energies, and you need huge machines to create them, and they only last for a trillionth of a second or something like that. What they do is they sort of follow spectroscopy and order things in patterns that are, really, in essence, based on atomic physics--although they've had to make some modifications which are fairly profound.

Riess: You say they follow spectroscopy?

Schawlow: Yes, they do, in sorting out things--angular momentum, selection rules, so on--they follow the ideas of atomic spectroscopy. Of course, it's different because these things are also strongly interacting. But it seems to be a field in itself that doesn't lead anywhere else as far as I can see.

Riess: And yet, you think it's overly identified as the calling for physics.

Schawlow: Yes. I do. I think there are people who think that we know the laws of quantum mechanics and everything's understood in principle in the atomic everyday realm. Well, it may be understood in principle, but it's certainly not understood in many respects.

Riess: The Tower of Babel image is the other extreme, sounds totally out of control and zipping off in all directions.

Schawlow: I think so. This supercollider they wanted to build--some physicists, like Phil Anderson, actually came out against it. He's a solid state theorist. I didn't do anything, one way or the other, but I think there were a lot of physicists who felt that's just not the kind of physics we know.

I understand what this man [Horgan] is talking about, his book. The theories that they have now, there are a lot of wild theories: the theories of everything--they call these string theory--that seem to require experimental facilities far beyond anything that we can ever hope to build, and that's certainly a dead end. I heard a talk that said that physics may be becoming like the cathedrals of the Middle Ages, which took centuries to build, and you can't do these problems in one generation.

Riess: One thing was interesting to me: he said science has existed as an activity for only a few hundred years, and yet people think of it as being a permanent feature of existence. But, in fact, it may not be.

Schawlow: It may not be. Of course, even existence may not be permanent. There're so many ways that people can destroy our world, it's really very upsetting. With missiles and atomic bombs, I can't think but sooner or later there'll be an accident, or a terrorist or a rogue nation will set off some of these things, and we may think it's another big country--it's horrible. When the United States and the Soviet Union were confronting each other, I could imagine that if Libya had gotten hold of an atomic bomb and set it off, we might think it was the Russians.

Riess: Okay, I waylaid you by talking about special relativity. But were you beginning to zero in on what you wanted to do in physics?

Schawlow: No. All I would really study was radio. I did a lot of reading about radio, radio technology really--not really deep science. No, what I wanted to do--well, like everybody else I

thought atomic and nuclear physics were the exciting things. After I came back, after the war, nuclear physics was what I would really have liked to have done. But Toronto was pretty run down by then; they had suffered during the Depression. All of the departments were asked to give up something to help balance the budget, and the head of the physics department gave up their research funds. It was supposed to be for one year, but they never got it back.

So there was very little money to do anything. They didn't have an accelerator. And the system of government support of science hadn't been developed yet in Canada. You had to make do with what was available. Well, the nearest thing to nuclear physics was studying the properties of atomic nuclei by details, hyperfine structures it's called, in the spectra of atoms. There was a pretty good man in that field, Malcolm Crawford. So that's what I did. It isn't what I would've most preferred, but I sort of have always taken advantage of the opportunities that present themselves. I haven't been a good planner, I just see what's available.

Riess: Please go back and talk about the war period. Was there any chance that you would have been drafted?

Schawlow: Yes, I could've been drafted by two countries. I had to register in both Canada and the United States, because I was still an American citizen but residing in Canada. But the Canadians felt I wasn't a healthy enough specimen.

Riess: You still had the asthma?

Schawlow: Well, I had a stomach upset at that time. Strangely enough, the doctor who examined me at the draft place was the same one who had been treating it. Anyway, they turned me down.

Riess: Was that upsetting?

Schawlow: That I was turned down? No, I didn't want to go.

Riess: What was your attitude? Was that a war you wanted to fight?

Schawlow: I'm not a fighter. I felt it was a just war, all right, and it would be horrible if Hitler won it, but I didn't see myself being a fighter. I sort of was willing to be on the sidelines as long as I was doing something that was helpful. What I was doing was needed and required my knowledge. Later, the Americans wanted to draft me, but by that time, I was working for this Research Enterprises Limited radar factory. They had a representative in Washington who somehow got that stopped.

Riess: Were you political during college?

Schawlow: No. I'm just amazed that--well, Canada had a liberal government, and had had one for quite a few years. I guess I felt that was sort of a good government. The word "liberal" wasn't considered as obnoxious as the Republicans seem to think it is nowadays. Well, I couldn't vote. I know we had one student who was a committed communist, and I could not understand that. We'd already seen in our newspapers articles about the show trials and concentration camps in Russia--this was no secret. I just couldn't understand how anybody could be a communist. But I wasn't active at all. I didn't have any time to do anything but study--and play with radio a bit.

Radio, Scouting, and Jazz Music

Riess: And how about your summers? Did you support yourself with jobs?

Schawlow: No, jobs were very scarce. The only time that I found a job was when a fellow student got me working for a couple of weeks in a factory that was making Christmas cards by silk screen printing. And I was helping there, most of the time cleaning Christmas cards: if they got a blob of paint you'd take a sharp knife and scrape it off. It paid twenty cents an hour.

However in one year, I believe between the third and fourth year, I was allowed to serve as a volunteer in the radio lab at the physics department.

Riess: That was during the year or in the summer?

Schawlow: In the summer.

Riess: It was a radio station?

Schawlow: No. It was mostly a teaching lab. I don't remember that we really did very much, but I could learn to use some of the test equipment.

Riess: You mentioned the Boy Scouts. Was that an important part of your life?

Schawlow: Yes, it was fairly important for a while. I'm not really an outdoor person: I went camping one year, didn't like it much, but survived it. They were nice kids in Boy Scouts. We got along. One in particular, Bill Michael, became a close friend.

I wanted to be a radio amateur, you know, but I couldn't qualify because you had to be a British subject to get a license. So I couldn't get a license; though I passed the test, I found I couldn't get it. But he had got an amateur radio station and I used to go down there sometime and help him out.

Riess: What could he do with that?

Schawlow: Well, it was Morse code. He would transmit and talk to other stations, other amateurs. Nothing terribly serious. But I thought it was very exciting to hear somebody from across the world or across the country.

Yes, shortwave radio was exciting. I mentioned that I built this two-tube radio set when I was in high school. I used to come home at noon, because it was only a few hundred yards away--sometimes the periods were staggered so I'd have a long lunch hour--and I would tune up the radio and listen, and you'd get places from all over the world coming in. Quite amazing on amateur bands. I think one day I got ten different countries. That was exciting.

Riess: Tell me what a two-tube radio is.

Schawlow: A so-called regenerative receiver which is on the verge of oscillating, one tube, they can be quite sensitive, and so you adjust them so they're not quite oscillating. The second tube was just an audio amplifier to make the sound louder.

I learned, although I'm clumsy, how to tune that thing finely. By putting my thumb and first finger on the knob and sort of balancing one against the other--you push a little bit --I could adjust it quite finely, which I had to do to get anything to work.

Riess: Did that make you want to make a better whatever-it-was?

Schawlow: Yes, it would've been nice to do that, and I did try to build this super heterodyne. As I say, I didn't get it working. This was about a five tube radio, I think, something like that. And I made some mistakes in the connections. I would've liked to have a transmitter, too, an amateur radio station, and talk to people around the world, but that wasn't to be.

The Boy Scouts--I got a lot of these proficiency badges I think they called them. I became a King's Scout, which is the highest rank, and got the gold cord, which you get if you have twenty-one badges or something like that--which is way beyond what anybody else in the troop was doing. But it was easy for

me to learn a subject and qualify for a badge. I got some weird things, even bookbinder--although my bookbinding was sort of barely passing.

Riess: But what about the Eagle Scout rank?

Schawlow: Didn't have that. King's Scout in Canada is about equivalent to the Eagle Scout in the United States, I think. That was the highest rank there.

Riess: And when were you introduced to jazz? Was it in your college years?

Schawlow: Yes. During my college years I had that radio, that super heterodyne, and I used to listen to it, and about the only thing that I found that I enjoyed was the swing music. There were a few other people I knew that knew a little bit more than I did about it. And there was a program, an afternoon swing session, that played some real jazz.

Riess: Where was it broadcast from?

Schawlow: It was from Hamilton, I think, which is about forty miles away from Toronto. Toronto, of course, didn't have very many black people. There wasn't a black district. It had tight liquor laws, so there weren't a lot of nightclubs. There were a few ballrooms where visiting bands would play, but I didn't go to those until later.

But I started listening to the radio, and liked some of the swing bands that I heard. So I went to the music library to see if I could learn something about swing music, and there weren't any books on swing. But there were a couple of books on jazz, and I read those. And books came out around that time. So then I started to explore, look for people like Louis Armstrong and Bix Beiderbecke, trying to find their records. There were a few of them.

Riess: Did you start buying them then?

Schawlow: I started buying records in August, 1939.

Riess: How do you remember August?

Schawlow: [laughs] I can almost give you the date. One friend I'd met through the Meccano club had a place out in the country, near Toronto. I went out there for a night or so to observe the meteor shower, the Perseid meteor shower, which is just about the middle of August, and on the way back I had to change buses at the corner of Bay and Bloor, and there was the Promenade

Music Center, and I went in and bought a copy of Artie Shaw's "Back Bay Shuffle," which is still a great record.

It was interesting that the records on the popular jazz labels like Bluebird and Decca were thirty-five cents. And then the war started just a few weeks later. This was in 1939. Canada got into it the beginning of September, 1939. The price immediately went up to fifty cents. Of course, they were right to do that because shellac came mostly from India, and shipping was very difficult. So they knew there was going to be a shortage of materials.

Mostly I bought records from the juke box stores. These companies, any new records that came out they put them on the juke boxes, and if they weren't getting a lot of plays they'd put them out and sell them. I think they were fifteen cents at first, later maybe a quarter. And you'd have to sift through whole piles of records, whole tables covered with piles of records, and learn to read upsidedown and sideways that way.

Riess: Were they out of their jackets?

Schawlow: They had just paper jackets where you could read the label. They didn't have fancy covers like LPs do. So I bought quite a few records that way over the next few years.

Riess: And you had a record player that was your parents?

Schawlow: Actually, at first I borrowed a windup record player from a fellow during the winter--no, my parents didn't have one--then my father bought me one. Someone, I think one of his customers, had this thing for five dollars. It was just the turntable and pickup head, which by modern standards was enormously heavy. It was amusing: when it was a synchronous motor you'd start by spinning it, and you could start it backward to play things backward. I connected that to my radio, you see, it played through it, so I played these in my bedroom.

Riess: Did you have to invent something to connect it to the radio? Or could you just go and buy a gizmo?

Schawlow: I think it took a little circuitry. I don't remember, really. It wasn't a big problem. I knew enough about how the radio worked to know where to connect it.

Riess: Well, that's a great memory, isn't it?

Schawlow: It was fun, yes. My sister was interested in jazz, too, so we shared records, she would buy some. Over the years we accumulated a number of records. I remember once one of my college classmates came over to my house and we played all the records I had. That was the last time I ever played all the records I had because I had too many to play.

Riess: How much music was on a side? How much time?

Schawlow: Three minutes, typically. Actually, I think there's a lot to be said for that. It imposes some discipline on the musicians --that was what a 78 rpm, ten-inch record would do. I think since LPs came along a lot of the more modern musicians get awfully long-winded and I think they ramble on for half an hour or so, whereas the great musicians of the swing and jazz era could say it all in a chorus or so.

Riess: Do you have now, on CDs, rerecordings of these collections?

Schawlow: I haven't everything, but I have a lot of them, yes. And I will probably build up more of those, too. Not everything I bought was good. In those days at least when you got one record that was a big event, and you'd play it over and over, really get to know it. You could even sort of pick out a particular passage because they [the grooves] were pretty spread out; you could put the needle down about the right place. Now, I really feel bad, I buy a CD, there's an hour's time on it, and I never really get to know it as well as I knew some of those old ones.

Riess: It's the first thing you've described that would really take up the kind of time that you had been giving to your studies.

Schawlow: Of course, not being a musician, I like to play music in the background while I'm working.

Riess: Oh. But you couldn't do that with three-minute music.

Schawlow: Yes, that's right. Couldn't do it very well--yes, changing the record. But the radio, when there was some jazz on, I didn't have to concentrate on it.

Riess: And you did play an instrument also, didn't you?

Schawlow: Well, during the war when my studies were interrupted, I decided I would try to learn to play clarinet. I really admired people like Artie Shaw, Benny Goodman, and Irving Fazola.

Riess: I don't know that last name--Fazola?

Schawlow: Yes. He played, at that time, with the Bob Crosby Band. And one of the things that I'm very pleased with now is that there's a company in England that's gradually reissuing all of Bob Crosby's records. Very gradually--one comes out about every six months or so per year. But I've got a lot of those. And Fazola's just as wonderful as I remembered. He had the most beautiful tone of any clarinetist, jazz or classical--I'll play you a sample if you like.

Anyway, I admired them. So I went to this teacher who offered to lend me a clarinet, to try it out. Well, I tried it, I thought it'd be nice, and I managed to buy one. Instruments were scarce then, and I bought a clarinet that was probably a mistake. It was a metal clarinet, but it was made by the Selmer Company, which is a very good company. It was a so-called full Boehm, which had extra keys so that a real musician could play A clarinet parts on it as well as the B-flat part. It made it somewhat easier.

I enjoyed trying to play it, but it became apparent that I wasn't going to be a great musician. However, I got far along enough that I could play with a few other amateurs--we had a little jazz band.

Riess: Where did you find them?

Schawlow: It's hard to remember just exactly what came first. There were a group of people that used to get together to play jazz records. They'd come around to various houses. Then Clyde Clarke had a radio program. In fact, I still see Clyde every time I go to Toronto. He has a colossal collection. He's never thrown away anything. His wife died, and his children are grown, so he has the whole house to himself--full of 78s, LPs, 45 rpms, everything. Anyway, he had this radio program, and I think it was through that that I may have met some of the other people. They put on some of these record sessions in public. I remember carrying my amplifier and stuff down to a hall for some of them.

Riess: So this doesn't have anything particularly to do with the university?

Schawlow: None at all. I never took a music course at the university.

Riess: No, but I mean that music wasn't centered at the university.

Schawlow: No, I don't think jazz would have been considered something appropriate for the university. Although in this little Delta Jazz Band that I was involved with, we had a banjo player who was by far the best the musician in the band. He was an

assistant professor of English at that time, named Priestley, F.E.L. Priestley. And his wife called him "Felp." [laughter]

Riess: So what was it? The Delta--?

Schawlow: Delta Jazz Band.[1] They were a pretty rough group. We tried to play New Orleans style, New Orleans revival. By that time records had appeared of some of the old New Orleans musicians who had never recorded before. Oh, it was wonderful stuff. We really enjoyed it. A number of us wanted to play like that. It had a beautiful swing. Always had two clarinets when I played with them, and I was the second one. We made a few recordings, but I lost them. Not commercial recording, just acetates, you know.

We actually made a recording earlier when we were called the Southern Stompers. Slightly different. We admired--by that time the books *Jazzman* and *Jazz Record Book*[2] had come out, and they greatly influenced my interest. I tried to get a lot of the records that were mentioned in them, and build up my collection.

The way the Delta Jazz Band came to an end was that--well, we weren't really very active, but when I got my Ph.D. I had a post-doctoral fellowship to go and work with Charlie Townes at Columbia. My sister was very proud, and she knew one of the university's publicity people and told him they ought to get that in the papers. He said, "Well, I don't know." But he called me up, and I told him--I could see he wasn't very interested in it--I told him that, "Well, fellowships are breaking up our Delta Jazz Band, because our banjo player is going to England on a Nuffield Fellowship and I'm going to Columbia." Boy, they really ate that up! It appeared in the national news of the Canadian Broadcasting Company.

Riess: They liked that twist.

Schawlow: Yes. I had a record of the Delta Jazz Band. I can't find it. Vanished. But I know somebody in Toronto, I think, who may have a copy of it. I'll have to pry it out of him.

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[1]For more on Toronto jazz see *Toronto Jazz, A Survey of Live Appearances and Radio Broadcasts of Dixieland Jazz Experienced in Toronto During the Period 1948-1950*, by Jack Litchfield, Harmony Printing Co., Ontario.

[2]*Jazz Record Book*, by Charles Edward Smith, with Frederick Ramsey, jr., Charles Payne Rogers and William Russell. New York, Smith & Durrell, 1942.

Riess: When tapes became popular, did you turn all of your records into tapes?

Schawlow: No, not until much later. Actually I built a tape recorder with the help of a machinist from the university--very early, about 1948 or so. It was a reel-to-reel recorder, of course. In fact, it didn't even have a capstan drive; one reel pulled the tape off the other one. Actually, I've still got some of those tapes. If I ever get time, I'm going to sort through them and see if there's anything worth listening to on them.

No, I kept buying records. Tapes--I don't like reel-to-reel. In fact, I don't even like tapes at all because you can't find anything on them. I much prefer discs from that point of view. Except they're [tapes] good for getting a lot of stuff. After the LP came out I started buying LPs in 1950. Well, 78 rpm records seemed such a nuisance after a while. They took up a lot of space, and to keep changing them was really a nuisance. So eventually, I think somewhere around 1980, I worked for several years and taped all the 78s. Then I gave them to Stanford University's Archive of Recorded Sound.

[Schawlow plays a minute of Bob Crosby band, featuring Irving Fazola]

Seeing the Possibilities in a Career in Physics

[Interview 2: August 21, 1996] ##

Riess: I want to ask you about the facilities of the University of Toronto. Would you use the library at Victoria College?

Schawlow: No, I wouldn't use the library at Victoria College. It was quite a long way away from the physics department where we spent most of our time. The physics department had its own library. And for general things the university library was not far away from the physics department. I don't think I ever used the Victoria College library.

They're having a hard time with the colleges, Victoria and the others. They're losing their function. They were modeled on the English colleges where there's a lot of tutoring going on. I don't think they ever did that, but they did require you to take one cultural course at least, and they taught most of the cultural courses in the college. I mean, they had an English department, and Greek and Roman history, and I guess other history, too. But gradually, the university has taken over those functions. Now they don't quite know what their

function really is, except that they do provide a dormitory for those who live there and they provide some social life which I didn't participate in at all because I'd just go home on the streetcar at night.

I think we had a pretty good library system. Anything I needed, we had.

Riess: And that was where you would have found the latest review and journal articles that were important to you?

Schawlow: That would be in the physics library. As an undergraduate, I didn't really need to use that very much, but I did as a graduate student.

I was also a member of the American Physical Society. I joined after I came back from World War II, but maybe even before that. We'd get the *Physical Review*, which had most of the important papers in physics, and the *Physics Abstracts*, which were then only about forty pages thick. I would read through the whole thing, at least skim through everything in all branches of physics. But now, a year's *Physics Abstracts* are, oh, about three feet long, something like that, and they don't give them away. If you're going to subscribe, I think it's something like a thousand dollars or more, so I gave up on it. But when I was a graduate student, I would read it all the time. Then if I needed to look up any of the articles, the library had a pretty good collection.

Riess: You had said last time that you took a couple of the cultural classes. What did you take?

Schawlow: In the first two years I think I took French. This was French literature--it wasn't very profound and I'd had French in high school. It wasn't too difficult either. I also had to take first year scientific German, which again wasn't difficult because I'd had German in high school. In the third and fourth years I took Greek and Roman history. I think it was Greek one year and Roman the next. Very interesting, but I didn't really put a lot into those classes. I chose courses that did not require writing essays, because I was so tired of writing meaningless essays in high school when I had nothing to say. In that way, I completed a four year course at a good university without writing a single essay, although I did write lab reports.

Riess: I wonder if you felt cheated of a certain kind of education in the humanities that could have been provided if they had systematically looked at physicists or scientists as people who were likely to be otherwise distracted.

Schawlow: No, I guess I have an unorthodox view that I think if a person is good at something, you ought to let him do it and do it well. I think it would have been a shame to make Bach or Mozart study calculus. [laughs] Well, I am no Bach or Mozart, but I think one can pick up an awful lot of cultural stuff rather more easily than you can pick up science. You can read the reviews in the New York Times and other journals, magazines. I'm not widely read in the serious literature, and I don't read modern novels, but I think you can pick that stuff up more easily than you can science. Science was a full-time occupation, really. I found it quite hard.

Riess: How about reading in philosophy?

Schawlow: I have never done any, and what I've read hasn't made any sense to me, so I'm probably wrong on that. I'm really pretty ignorant of philosophy.

Riess: At that age, or even an earlier age, what did you think about your possibilities? What sense did you have of knowing who you were? Do you feel that you knew, or were you floundering?

Schawlow: Well, no, I think at each stage I wanted to take advantage of the opportunities that I had. As I think I said before, when I started out I thought, "I can probably end up teaching high school or maybe do something involved with radio." I didn't know whether I could go on to do graduate research for a Ph.D. or something like that, it really was something that I thought was beyond me. But I didn't really worry about it--there was plenty to do.

And then, of course, when I did well at the undergraduate work I thought maybe I could do graduate work all right, and I wanted to see how far I could go. I didn't know how I could do at research, not having done it. I thought it would be nice to do some basic science, basic physics--but I didn't know whether I could until I tried it.

Riess: I think that scientists are blessed in that they often know that that's what they want to be doing, come hell or high water.

Schawlow: I knew what I'd like to do, but I had seen the realities of the Depression. I was prepared to do whatever I had to do--but I knew what I wanted to do.

Riess: To do whatever you had to do--in order that you be able to work in science? Was it like that?

Schawlow: I guess it developed that way. Initially, it was just I would do whatever I would have to do to make a living. Because as I say, in the thirties during the Depression that's just what people had to do. But yes, I think by the time I was mid-way through graduate study I felt I wanted to go someplace where there was really front-line science going on and hope that I could learn enough to perhaps participate in that.

There was a meeting of the Canadian Association of Physicists in Ottawa. This organization was formed about fifty years ago, wasn't it? No, 1945--I gave a talk at the fiftieth anniversary meeting. It was founded because a lot of physicists, people trained in physics, had done essentially engineering work during the war, and they were afraid that they would have to become registered professional engineers to continue in that sort of thing. So they formed this Canadian Association of Physicists to look after the professional concerns. Well, I joined the thing right away. I never was much interested in that, but I went to a couple of meetings and they had some physics talks as well as some talks about their worries about professional concerns.

The meeting in Ottawa had a lot of dull talks about, as I say, whether they were going to have to register or whatnot, but I.I. Rabi from Columbia, who already had a Nobel Prize, came there and talked about the work that had been done recently in their department by Willis Lamb and Polykarp Kusch, who had unearthed new information about the nature of atoms and electrons--in fact, a find for which they got a Nobel Prize shortly after.

I thought Columbia was really the most exciting place in the world, and I really wanted to go there. I applied, after I got my Ph.D., to several universities, and I think I could have had assistant professorships at several places because there weren't many fresh graduates at that time in '49, but I did get this fellowship to go to Columbia to work with Charlie Townes.

I didn't know about Charlie, and I wasn't much interested in organic chemistry--this was supposed to be for applications of microwaves to organic chemistry--but I was interested in microwaves and had been for a long time, even worked on them a little during the war. And I was interested in them even before that. So, anyway, it turned out to be a very good thing.

Riess: Yes. I guess the moral of the story was that that organization, the Canadian Association of Physicists, was perhaps a kind of watershed. It's horrible to think that if

they hadn't formed that organization and Rabi hadn't come to town--

Schawlow: I don't know, I think I would have known. I'm not sure. I think I would have because I did follow the literature quite closely then on what was going on. But it was one of the things that triggered it.

Let me say one more thing about it. I gave this talk at the fiftieth anniversary and I explained about how it had been started--I didn't really go too seriously into it. But they printed my talk and they sent me a copy of their journal, and I see they're still worrying about the same problem of the professional status of physicists, which apparently has never come up in the United States at all.

Riess: One of the things that you mentioned was that research money stopped during the war. Universities had to make a decision about research money or not.

Schawlow: That was before the war, some time during the Depression that they had given up their research money. I doubt if it was very much, but they had an annual research grant from the university. Sometime in the Depression they were asked to give it up for a year and Professor Burton--Eli Franklin Burton, who was the head of the department at that time--gave up the research grant for the year and the university didn't give it back. I know he must have been able to raise money somewhere because he had students build the first electron microscope in North America. That was a big advance and must have taken some money.

Thoughts on Emigré Physicists, and Family Support

Riess: A bit more on your early years. I wonder whether you had heroes in science. What about Einstein? Who were your mentors?

Schawlow: Of course, everybody revered Einstein--extremely brilliant. It's hard for me to remember. Now I would think probably the person I would most admire was Faraday, from a hundred years earlier almost, who did such simple experiments and discovered entirely new phenomena. But I did read a lot about physics and physicists and I did admire the ones who had done some things. The theoretical stuff of Einstein's--well, I don't think I had any illusions about being a deep theoretical physicist.

Riess: Did you father or mother understand your interests?

Schawlow: They were supportive. I never questioned them about how much they understood. I think they understood in a general way.

Riess: But if you were coming home, sitting down at the dinner table, during the high school and certainly all those college years, did you tell them about what you were doing all day?

Schawlow: Gosh, I don't know. I don't really remember. I don't think we would talk about specific physics problems. With my mother I talked about my general difficulties. But I can't remember at all.

Riess: Do you think the family was very focused on your success?

Schawlow: No, they were supportive but I don't think they were focused. Certainly they strained hard financially to get me and my sister through college. We were both there at the same time, and we both had had scholarships, but even so that was quite a burden. My father acquired some debts, I don't know how much they were. He never would discuss that with me.

Fortunately, after the war he'd bought a house. They [the owners] wanted to sell us the house we were living in for $3,000, but we didn't think it was worth it, and my father bought another one for $6,000, I think--sold it a few years later for $15,000. Got another smaller one, and I think later sold that for $23,000. That was the only thing that got him out of debt.

Riess: That's interesting. He put some work in on the house, or is this just general inflation?

Schawlow: No, just the general inflation. The inflation of houses was very fast--in fact, has been almost all the time since then.

Riess: Was there a population of emigré physicists who came to Canada like there was in the states in the late twenties and thirties?

Schawlow: There had been, but there weren't a lot around. There was a German physicist named Kohl who was a specialist in the construction of vacuum tubes. He gave a series of lectures on electronics. I listened to him twice, but it was unfortunately the same both times. Actually, he came to the Stanford area later and worked for one of the companies--I think also gave some lectures. But he wasn't right in the physics department.

Let's see--oh yes, the most famous refugee was Infeld, Leopold Infeld, who had worked with Einstein on cosmology. He

was Polish and he was professor of applied mathematics. I never worked with him. I heard some of his lectures, but I didn't have a course from him either.

Infeld--it's a sad story. He was Jewish and the Jews had been persecuted considerably even before the war in Poland, as in Russia too. But after the war he thought that the new government there, which was Communist, would improve things, and he wanted to help rebuild science in Poland. Well, the prime minister of Ontario was a Conservative named George Drew who kind of jumped on him for helping these Communists, and essentially made things unpleasant enough that Infeld left and went back to Poland.

Riess: My question is partly why there weren't more, and whether the ones who came gave Canadian science a kind of jump start?

Schawlow: I don't think they did. There weren't enough of them.

Riess: They went to Chicago and they went to Caltech.

Schawlow: Yes, Gerhard Herzberg went to Saskatchewan and then to Chicago, and finally after the war they brought him to the National Research Council. But he was never in Toronto except perhaps as a visitor. He was a preeminent molecular spectroscopist and got a Nobel Prize for that later.

I think there was some anti-Semitism in the university. I never heard it talked about, but I don't think there were any Jewish professors in the university at that time. There are now, of course, but--. And that may have been another reason why they were not so keen to take in European refugees. It's a pity, because there were certainly some great ones available for almost nothing.

Riess: You said your father chose to tell you that you were partly Jewish when you were seventeen. When you were seventeen it was 1938, two years before the onset of World War II. When you think about it now, why do you think that he chose to tell you then?

Schawlow: It's hard to tell. He had a sister who was still living in Latvia, and he was trying to see whether we could afford the money to bring her to the United States because he felt the war was coming. I think he decided in the end that we could not afford it, so I don't know what happened to her. But I think it was connected with that. I don't know, I guess he thought it was time I should know. I didn't react to it in any particular way, I think; it was a fact--nothing I could do about it one way or the other.

Riess: In science, particularly, it's a really fine heritage.

Schawlow: Well, I think also there was a Jewish tradition of supporting the son who was a scholar--usually a scholar of Hebrew and that sort of thing, the Talmud. I think they treated me that way. I spent a lot of time up in my own bedroom, studying or working on radios or something like that. They really were quite good to me, and I think they were generally supportive. There was never any question that I shouldn't go on as far as I could as a scholar.

I wouldn't say it never entered my mind that I could be a scientist, but I remember my father saying more than once that I should study German because if I wanted to do serious science, I'd have to study in Germany sometime. Well, the world didn't go that way. In fact, we had young Germans coming to work with us. Later, I gave some lectures in Germany. So the thought that I might be a scientist was not entirely out of mind [pauses] but, I think I tend to concentrate on more short-term things.

Graduate School Years--The Master's Degree

Riess: What do you mean you think you tend to concentrate on more short-term things?

Schawlow: You know, I was wondering, sort of thinking back, "Why didn't I dig deeper into the textbooks and get more advanced books on science and so on?" Well, I didn't do that. I read all around it. I mean, I was very interested in things on radio in a qualitative sort of way, not really quantitative. And I read all the papers I could find on microwaves at the time when they were classified during the war. I found that the Germans had published more than the Americans or British had. I took those out of the library.

Your mentioning libraries reminds me that our university library had a complete set of the philosophical magazine published by the Royal Society in England, and the first article in the first issue was "Mr. Cartwright's Patent Steam Engine Which Can Also Be Used as a Still." [laughter] I remember going into the library and looking around and looking through those old journals. They had a pretty good collection of old journals there. Of course, it wasn't as difficult as it is now because there are so many thousands of journals. Prices are so exorbitant. But they had a good collection of things that were published even before the university was founded.

Riess: That idea, that maybe you were thinking short-term, or whatever, I guess it's a big mantle that gets laid upon scientists.

Schawlow: I don't know anything, I think sometimes. But you learn a few things. As I'll probably say later on at the appropriate time, or maybe several times, I learned that to discover something new you never have to know everything about a subject. You just have to recognize one thing that isn't known, to look for the gaps.

I think that really came to me, though not so explicitly, when I was midway through our graduate studies. Professor Crawford had told me to build this atomic beam light source, and we built it--two other students helping, one had built the spectrograph. Then we had to decide what to do with it, and I did most of that work. I looked through the library to find out what atoms had not been studied with the kind of resolution you could get with an atomic beam light source. Finally, we did some work on silver, zinc, and magnesium.

Riess: I was going to ask you exactly that. How had you kept up with the literature to decide what to work on with your atomic beam light source?

Schawlow: Well, I fortunately knew enough German that I could read some of the old German papers--the best work had been done in Germany before the war, and in England, too. You know, one paper leads to another. You get a reference, and it leads you to some earlier paper, and you can track things. The scientific literature is highly cross-linked because--at least they have in the past felt responsibility to refer to all the relevant papers. So I did spend a good bit of time in the library at that point.

Riess: [reading][1] "In the United States [in 1927], many senior physicists watched the development of the quantum mechanical revolution with a sense of frustration. For some, the mathematics of the new formalism was simply too difficult. And all who were concerned with the philosophical implications of the new physics--particularly the middle-aged men whose thinking had been formed in a more certain world--were bothered by the seeming subjectivism of Heisenberg's approach." You went to a university where you probably were taught by a lot of "middle-aged men whose thinking had been..."

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[1]From *The Physicists, The History of a Scientific Community in Modern America*, by Daniel J. Kevles, Harvard, 1987, p. 168.

Schawlow: Yes, I think so. Almost none of our professors actually used quantum mechanics. I didn't take the formal course in quantum mechanics, but I had some introduction in atomic theory. It was really very unfortunate that I never did because there were good graduate level courses. But by that time I was working in the light lab as a--what they called a demonstrator, here we call it a teaching assistant. And I couldn't get time off to take that course, so I never had a real course in quantum mechanics.

But I had the feeling there that people there really hadn't digested quantum mechanics for the most part. Crawford had and so had Welsh, I think--but they also were used to thinking in semi-classical ways, which could do a lot of the stuff.

Riess: At the beginning of wartime, math and physics at Toronto added a specialization in radio science and technology. Was that in fact radar?

Schawlow: Well, it was aimed for radar--submarine detection, too, but I think mainly for radar. But I didn't go into that specialty. I remember asking the head of the department. He said, "Oh no, you know a lot about radio already"--which was true.

Riess: So, onto wartime. That was your first teaching experience?

Schawlow: No, I really didn't do much lecturing during the war. I think I gave one course, but mostly I was just a laboratory assistant or led discussion sections--one of the professors would teach a large class, and then the students would meet in smaller groups with some of us and we could answer questions or work out specific problems for them. And I think we helped them in the laboratory too.

Riess: This is the army and navy guys?

Schawlow: The army people were given our standard undergraduate physics course basically, which is a rather formalized, standard sort of thing. There were standard textbooks and standard topics that were covered, and it was pretty much the stuff we'd had earlier as undergraduates a few years earlier.

The navy [chuckles]--well, they tended to be submarine detection technicians, and they were told that you had to have six months at sea and you don't get seasick, that's all that was required. So some of them came in not knowing much, and we had to do as much as we could with them.

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Riess: You're well very known for your demonstrations and I wondered whether you developed some of those techniques back then?

Schawlow: No. But we did have at least one professor who gave very spectacular demonstrations in our first year physics class, mechanics. We had demonstrations in all the first couple of years physics classes, but Satterly, John Satterly, was one who really put on some pretty fancy demonstrations, especially in his liquid air lecture, in which he was following a tradition of the time when he was a student when people would go around putting on putting on liquid air lectures and demonstrations in the music halls or someplace like that, just as entertainers.

Riess: Like a magic show?

Schawlow: Yes, right. Because people were just not familiar at all with the strange properties. He did a lot of experiments, but one I remember: he'd put a loaf of bread in a great big pan about six feet across and then pour liquid oxygen on it, set fire to it, and the flames would reach up almost to the ceiling. It was spectacular.

Another one--he'd dip a piece of rubber hose into liquid air and, of course, it got very brittle and you could crack it up like that. Then he took a couple of goldfish and put them in liquid air. Then he'd smash up one of them. The other one he'd put back in the bowl, and in a little while it'd be swimming around again. I asked him once, years later, how he could do it. "Well," he said, "you just have to be careful not to damage their scales." Anyway, those were spectacular sorts of demonstrations.

Riess: It's interesting, they do connect to a tradition of magic.

Schawlow: I don't know the specifics, but I have heard about it, that there used to be vaudeville people who'd go around doing liquid air demonstrations and talks. Of course, in those old pre-television days there were the Chattauqua lectures, and a lot of others in England. People would want to know about the latest discoveries of science in a digestible way--at least the wonders of science.

Riess: Is it also the wonders of science and seances? During the turn of the century, weren't people seeking that?

Schawlow: Some were. Oliver Lodge was a very fine physicist who became a spiritualist and spent a lot of effort trying to communicate with the dead--with no great success.

Riess: Now, when you started to do your work for the MA degree, was that a time when you might have, if it had been at all possible, come to the States and begun your education here?

Schawlow: Well, yes, but I didn't know what to do during the war. I guess when I started on my M.A.--probably '41--the United States was not yet in the war, but they soon were. Well, I just didn't have enough initiative to seriously consider going somewhere else at that point.

Riess: Because you're really caught there on the fellowship issue, as you say.

Schawlow: Well, at that point--that was in '45 when that became apparent --my sister got a fellowship to go to Wisconsin to study English and she spoke to the physics department chairman and he assured her that they would gladly give me a fellowship to go there if I'd like to go. But things weren't going so badly in Toronto, and I thought--. I guess I tend to be rather cautious. In fact, I've probably missed a lot of good things by being too cautious.

Riess: Was there also a pull to stay near the family?

Schawlow: Yes, sure, I was living at home and my mother took good care of me.

Riess: Well, I meant that you would be needed to take care of them?

Schawlow: Not at that stage, no. They were still healthy at that point. They would be about fifty-five, I guess.

Riess: Would you describe what you did for your master's?

Schawlow: It was a silly thing. It had to be something applied and we, another student, Morris Rubinoff, and I were trying--this was something we did while we were doing the teaching, you know, just in odd moments--trying to develop a battery that would be activated when it was suddenly put in motion somewhere and spun. Well, we weren't told what it was for, but it was pretty apparent that it was for something to go into a shell. I learned later it was for a proximity fuse. So it had a little glass ampule and fins around the battery things.

We didn't have any priority, we didn't have a lathe or any tool equipment. We could occasionally get a little machining done, but it was pretty bad. Actually, we got to the point where we fired one of these things off in a shell, sent it to Camp Borden, which is a training camp, and the ampule didn't break--[laughs] whereas we'd dropped it on the floor and it

did. I learned later that the trick was to put a little spring in the thing, make it so that it wouldn't break on a sudden impact, and make it weak enough so that it would break on a longer impact--as in firing the shell. But we didn't think of that.

It didn't really amount to anything, but after a year or so, they said, "All right, that's enough. You can have the master's degree." The master's degree was not anything very important. As Professor Satterly once described it, "It's a bone thrown to an underpaid demonstrator."

Research Enterprises, Ltd., Wartime Research, the Bomb

Riess: During those years was there an equivalent in Canada of Bell Labs, or a research laboratory associated with a communications industry?

Schawlow: Well, there was the Canadian National Research Council, and there even was a classified research program at the university. I don't know what they did, and I suspect that it wasn't much. The National Research Council in Ottawa did some radar development, but I think the level just wasn't really very high. And of course, I didn't get invited to join this group even in the physics department. I don't know whether it was because they thought I wasn't good enough, or because I was not a Canadian citizen, or because the professors who were doing the teaching wanted me. But I knew the people who were running that, and I doubt if anything very worthwhile came from it.

Riess: The level of wartime research on the East Coast, at MIT--

Schawlow: Yes, if you went to MIT or Chicago or Columbia or Harvard very good things happened. They did move a lot of people from all over the country to these--and Los Alamos, too--to these projects. But I suspect there were a lot of universities that managed to keep going in a small way, and not doing anything very important.

Riess: Maybe since it was a British country all the research would have been sent off to England.

Schawlow: Yes, the most important work was. People there still thought of England as the mother country and looked up to it. The people at the National Research Council did develop a microwave radar with a wave-guide antenna, which is what I worked on, trying to get in final shape for installation in the last year

of the war at Research Enterprises Limited. But I think it really wasn't a very smart design. It was too sensitive to the temperature.

Riess: Was Research Enterprises Limited formed because of the war?

Schawlow: Yes, it was strictly a manufacturing company. They were set up during the war and they made some optical equipment, including some teaching lab stuff--spectroscopes. They made rather nice spectroscopes, of which we had a few at the university. But I think mainly they made radar equipment. When I did go to work there, I saw one radar setup, a portable radar that required eight trucks. [laughter] I think a couple of them were spare parts or something like that.

Riess: Did Canada get ahead of the United States in radar work?

Schawlow: I don't think so. Britain did.

Riess: I read that the magnetron--

Schawlow: That was British.

Riess: And was the magnetron given to Canada for Canadian research?

Schawlow: I think it must have been, but I don't think they did anything more than use it. They never published a set of books like the MIT Radiation Lab did to report what they had done. I guess I just never heard.

Riess: Did you feel that you were involved in the war effort?

Schawlow: Yes, but I didn't really think I was making a great contribution. I did what I was asked and I felt it was probably helpful for the war, but I didn't think it was going to make a big difference.

Riess: You were testing components.

Schawlow: Yes, basically. I worked at Research Enterprises during the summer of '43, I guess. And then in '44 I moved there after they finished courses for the army and navy at the university. When I was there for the summer we were testing transformers and capacitors. When I worked there, we were using these slotted waveguide antennas, trying to adjust them so that they would work.

Riess: Slotted waveguide?

Schawlow: Yes. It's a long piece of rectangular pipe--oh what was it?--about three inches by an inch and a half or so. And they had slots in the face that were supposed to be spaced a half a wavelength apart so that the radiation leaking through them would all be in phase in a forward direction so they'd get a good beam. These things were quite big, I think about ten feet long or so, maybe longer. We would check them in the lab and then mount them up on the roof on a turntable. Then we had a receiving station across the valley, a mile or so away, and we'd measure the pattern to see if they had a good directional pattern.

These slots--we'd measure them first; the trouble is that the effective wavelength of the waves in the waveguide depends rather critically on the dimensions of the waveguide. And they weren't all that precise and so we'd measure one, measure the wavelength in the guide, have the slots put in, and then check it. But since it depended very critically on the dimensions of the waveguide, that is the width, if the temperature changed then that would change, and they would not work so well. I think it worked, but it wasn't really a very good scheme.

Riess: That sounds very frustrating.

Schawlow: Yes, well, I don't know, I didn't really try to improve it. I just said, "Okay, we'll do what we can with what we have here."

Riess: Do you think that people, at some level, were in touch with the MIT Rad Lab?

Schawlow: Oh yes, I think so. And with the British. I think in Ottawa they were--and maybe some of the people at Research Enterprises, but I wasn't. I did have a security clearance, but it was pretty low-level stuff that I saw.

Riess: I'm trying to think about whether you would even have time to stop and think about this thing you were working on, and think in your mind about how it could be improved, or whether you were like a factory worker practically.

Schawlow: I think I was more like a factory worker. I didn't think very creatively about it, I'm sorry to say.

Riess: Well, do you think that that's you or do you think that's just the kind of nine-to-five nature of the job?

Schawlow: I think it's probably both. I think it's probably the nine-to-five nature--I wasn't responsible for it any more than doing what I was asked to do, and I did that as conscientiously as I could, but I didn't really dig deeper.

Riess: So you were doing that up until the end of the war?

Schawlow: Just about a year, yes.

Looking back, I probably should have studied physics while I was doing that, because we had to take qualifying exams for the Ph.D. on the undergraduate work and I felt it necessary to study for a year before I took those. Instead of taking them in the fall of '45, I took them the fall of '46. Of course, I started on the research before that, but nothing would have stopped me from studying for those exams while I was working. I wasn't being pushed that hard on the work. I could have worked on it at night but I didn't.

Riess: Were you still being supported by scholarships there?

Schawlow: No. During the war I was paid, not a princely sum but I was being paid for the teaching and later for the work at Research Enterprises. It was something like forty-five dollars a week, I think, for the teaching. Then when I came back there were no scholarships available, but I could get this teaching assistantship, or demonstratorship.

Riess: And then that was the year that you studied for your exams?

Schawlow: Yes, and I think I started planning on the research. I remember coming over to the university somewhere around the end of the war and talking with Professor Crawford outside the building about what I might do, and he suggested atomic beam light source. That sounded good because you could get some properties of nuclei, and what I really would have liked to do at that point was nuclear physics. There wasn't any accelerator and I couldn't get any closer to nuclear physics at Toronto than what I did.

Riess: The A-bomb, where were you?

Schawlow: Well, I really didn't follow it. I didn't really understand it awfully well. I knew that nuclear fission could produce a bomb with uranium. I think we knew even that uranium-235 was the important part. Oh, one heard rumors that there was work going on that. There even was an article in *The Saturday Evening Post* at one point, I think, that told something about it. But I didn't know where it was going on or what was being done.

And I thought that you had to slow the neutrons down by putting them in water, because the slow neutrons had a larger cross-section. So I sort of visualized this--well, they maybe would get some uranium-235 and dump it into the water, into the bay or whatever, and that would set it off. Of course, that

wasn't at all the way they did it. They did it with an implosion. After the war, the Smythe report on atomic energy for military purposes was published and so then I learned something about what actually had been done.

Riess: But that's interesting that you could've even anticipated that.

Schawlow: Well, the existence of nuclear fission was known, and as I say I think there was this article in *The Saturday Evening Post* about the possibility that one could make a superbomb that way.

Riess: Maybe they were saying that the Germans were getting this superbomb?

Schawlow: Oh yes, they were very concerned about that. That's one reason people worked so hard,to beat the Germans to that. It would have been a horrible thing if Hitler had had atomic bombs.

Riess: What do you think about the horrible nature of it anyway?

Schawlow: I think it's pretty awful, and I guess I am sort of glad I didn't work on it. So far, everything that I've worked on, even when it's the military, has been rather peaceful--for radar, detecting incoming planes and missiles. I think it's a shame that they used the thing on people, but I can understand a little bit of the military mentality because they had estimates that so many hundreds of thousands or millions of soldiers would be killed if they had to invade Japan.

Riess: Has it been an ongoing issue for you?

Schawlow: No, well, I read the *Bulletin of the Atomic Scientists*, and the Federation of Atomic Scientists, wait a minute, it was the Federation of Atomic Scientists, now it's just the Federation of American Scientists--it does very good work [publishing monographs] on trying to reduce the hazards of nuclear warfare. I think they were in the forefront of pushing to stop atmospheric testing which would pollute the atmosphere with a lot of that radioactive stuff. They're still working very hard to expose the nature of the dangers and try to get people to stop it. But I read the thing and I don't do anything about it.

I have not been a political person or an activist. I can always see both sides of the question. I think if we hadn't had atomic bombs the Russians would have been even more aggressive in Europe than they were. In fact, one of the Russians told Charlie, "If you hadn't had the bomb, we would have done some things differently," or something like that.

Riess: How about when Star Wars was--?

Schawlow: Oh, that was just nonsense. In fact, I was quoted in *Time*, out of context, as saying something to the effect that I didn't think it would work--whereas I would not have commented at that point, because I didn't know what secret work was going on. But I didn't really think there was any hope, and I was right, even though I didn't know the details.

Riess: You get called upon for quotes, probably, a lot.

Schawlow: Well, that quote was taken from some interview some months before. Somehow, they dug it out and posted it in *Time*. I was a bit embarrassed about it, but there was nothing I could do about it.

I'm really rather pacificistic in my leaning. I think war is stupid, any war is stupid, because to kill people to settle a question is really not right. But on the other hand, I do see that some people are going to be very desperate--they want something and they'll risk anything for it. I was at a meeting in Canada and met a Canadian physicist who had been involved in political affairs, and he was talking about--India had just had their first atomic explosion. I said I couldn't understand why they would want that because it would just makes them a real target.

He said, "Listen, have you ever heard of triage?" I said, "No, I haven't." "Well, this is when you divide the wounded into three groups in a battle, and you patch up those that you can get back into action first; then, the ones you can get back into action later; and the rest you just let die."

He felt that if there was a famine in the world India might be a victim of triage, and that's why they wanted to have their bomb. Well, that's his theory. Of course, they also have considerable enmity with Pakistan and China. It's a shame, the world will divide itself into groups, no matter whether they're based on race or anything else, but people will fight. Afraid I try to avoid that.

Riess: Bomb programs are big science.

Schawlow: Well, they're going to nuclear power. It has to be done. It's not really big science compared to the huge accelerators that they want to build nowadays, but it's pretty big. I, of course, held out great hopes for peaceful uses of nuclear power. The general line then was that power would be so cheap, they wouldn't even bother charging for it. They didn't realize

all the difficulties, some of which are just due to unreasoning terror, I think.

People don't know what's safe and what isn't, and they don't believe the government or the scientists--with some reason, government certainly has lied to us. But that means that they just paralyze. For instance, magnetic resonance imaging for the brain, and the body, is properly known as nuclear magnetic resonance imaging. However, they deliberately dropped the word "nuclear" because people were afraid of anything nuclear, thinking that had to with atomic bombs.

Riess: In fact, risk is something that people are studying now.

Schawlow: But you can't really ever get complete certainty in anything. It's funny, there are more people killed in automobiles than in wars, I think, but they tolerate automobiles because usually they're safe--"It won't happen to me." Of course, that's what soldiers, I gather, would tell themselves. "It won't be me."

[tape pauses]

Schawlow: I'm not sure what interest this part of the history will have for people. It wasn't until six or seven years later, actually, that we started working on the idea of a laser. I don't think people will be that much interested in what comes between, which is very different. We'll do it anyway, but I'm trying to think what Joe Public, Joe Sixpack wants. [laughs]

Graduate School Years--Atomic Beam Light Source

Riess: I'm interested in the trip you made to Purdue after the war. Was this was the first time that you'd even been out of Toronto?

Schawlow: Well, I'd been to Pembroke before to visit my aunt and her family and other relatives there. The first Canadian Association of Physicists meeting was in Montreal, and somebody had a car, another student. Several of us drove down there. The second one was in Ottawa. These were trips of at least two hundred or two hundred and fifty miles. But I don't remember having been on a train before that.

The reason I went to Purdue--what happened was that we started on this atomic beam light source, and we read the papers of Karl Wilhelm Meissner and his associates who had

built one of the first ones. I guess Minkowski and Bruck also had built one around the same time.

Riess: Minkowski?

Schawlow: Yes. And Bruck. I don't know whether it's the same Minkowski who did cosmology relativity theory. Probably not, but I haven't ever checked that.

Anyway, we had a terrible time with it. I think we had a reasonable idea of how to go about it, but we didn't know a lot. There was nobody around there who knew anything about vacuum techniques. The electron microscope people, the ones who had built it, had gone, though it was still being used. But we had an awful lot of trouble with leaks.

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Schawlow: We shouldn't really have been using brass, we should have been using stainless steel--although I don't know whether the workshop could have handled that, welded it. At any rate this thing was pretty big, between two and three feet high, and the ports, some of them were three inches in diameter. So when you evacuate, there's a lot of force from the air pressure on it.

Riess: When you evacuate?

Schawlow: Yes, you have to pump out the air.

I should explain what the thing is. The idea is that you vaporize some substance, some atoms that you want to study. The atoms will go out in all directions, but you put a baffle in between that part where the oven is--there's an oven at the bottom--a baffle that will only let those that are within a small angle go through. So you get a narrow beam. You don't get very many because you're throwing away most of the atoms, they go out in all directions, and you only take those which go through the hole.

And then up above that we would bombard them with electrons and produce light, the idea being--this is to get rid of the Doppler-broadening. That is, all atoms, if they're free, they're moving around rather quickly, so some are coming toward you and they emit a slightly higher frequency, shorter wavelength; others are going away, and since they're random, it just results in a broadening of the line which wipes out all the fine details that we want to study.

I've often described this in lectures that it's like sound: if the source goes toward you [high-pitched voice] it goes up

in pitch; if it goes away [low-pitched voice] it goes down in pitch--towards you [high-pitched voice], up in pitch; away, [low-pitched voice] down in pitch. That's the Doppler effect slightly exaggerated. [laughter]

We were trying to build this thing to cut out the Doppler-broadening and I think our design seemed reasonable. After our first year, Fred Kelly came back from the war. He'd been in meteorology, I think, during the war. He did part of it and I did part of it, but we were having so much trouble with the leaks. When we'd get a leak--we didn't have a helium leak detector which were beginning to appear, but we did have a big tank, about five feet square, and we'd fill that with water. We'd take the apparatus apart and put plates over all the openings and blow air into the thing and look for bubbles. If you'd find the place you'd have it resoldered and then put it back together again--and it'd take about a week for this and then something else would crack open!

After a year or so of that I was getting pretty desperate, and I wanted to know if we were on the right track, so I wrote to Professor Meissner at Purdue and he very kindly invited me to come and see what he was doing. He treated me very nicely. I think I didn't tell him that I was a student, but anyway he treated me nicely and showed me what he was doing. And he offered me, if I wanted to come there, he could get me a research assistantship there. But I decided at that point that we were pretty much on the right track, and so I came back. I guess it was a little later that I got kind of disgusted and I insisted that the machine shop take it all apart and solder it more tightly, more strongly, and after that it worked all right.

Riess: You financed the trip? It wasn't that Crawford sent you?

Schawlow: No, he didn't authorize it or pay for it. I just did it.

Riess: And what you were building was an atomic beam light source.

Schawlow: Yes. The thing is that you could observe the fine details of spectra with suitable equipment, which the third member of our graduate student group was building, and you could measure the nuclear spins and the isotope shifts. Now they had measured a few things, but there were a lot more elements, and that's where I had to find out what wasn't done. So I had to read all the old papers and find out what was done and what wasn't done, and pick out some other atoms that we could vaporize reasonably and yet which had something interesting to look at.

Riess: You mentioned that it had already been done in Germany. You couldn't get what you needed from Germany?

Schawlow: Well, this was after the war, of course, and the papers in Germany were pre-war stuff in the thirties and so on, so I doubt that any of them still existed.

Riess: Is it also that you don't fully understand what you're doing unless you've built your equipment?

Schawlow: [looking for papers] It does make a difference, but at least since then I've always felt, never build anything you don't have to.

Riess: Is that tongue in cheek, or do you really think that too much time is spent on fabrication?

Schawlow: Well, that saying, never build anything you don't have to, that's especially true because a huge instrument industry has grown up since World War II, and instruments have become much more complicated, and it just takes a lot of time to build them; if you're going to design and build a lot of the instruments, then you wouldn't have time to do the experiments.

[showing interviewer] This is a diagram, from Fred Kelly's thesis, of the atomic beam light source. There was an oven down here, and then this was a water-cooled tank in the middle, cooling this tube. So that defines the beam--anything that gets through here is pretty much directional. Now, compared with the atomic beams that are used in [I.I.] Rabi's lab, this is a very crude atomic beam. It gave us a collimation of perhaps one in ten, or something like that, but that would reduce the Doppler width by a factor of ten--the equivalent of reducing the temperature by a factor of a hundred. But you couldn't reduce the temperature of these vapors by that or they'd condense. So this was a way to get narrow lines.

Riess: Is what you're doing optical spectroscopy, at this point?

Schawlow: Yes, that's right. I didn't really pay too much attention to the optical equipment--well, we did work on a spectrograph. We used what's called a Fabry-Perot interferometer. Meissner lent us one of his so we could copy it, and that was very helpful.

Riess: You took the interferometer apart?

Schawlow: We took it apart and had a copy made. The thing that's tricky about it is, you use two flat quartz plates that fortunately were left over from the 1920s. You coat them with some kind of highly reflecting metal, and then you set them up so that

they're exactly parallel--they're very flat and exactly parallel to each other and this means a very fine adjustment to a fraction of a wavelength of light. The mount that Meissner showed us how to make was a way to do that, get them so they would be precisely parallel and would stay that way.

We had some fun with the coatings on these. [laughs] We were working in the ultraviolet and there was just nothing on the ultraviolet. Nothing about the techniques for reflection, little information about reflection of thin films in the ultraviolet. I remembered reading--the German ones in the thirties, Schuler and so on, said that they used the hochheim alloy, which was prepared by Dr. Hochheim of I.G. Farben. I don't know, I never heard of him after the war, but I even dreamt about it one night, that he said, "It's just aluminum--just put it on good and quick."

Riess: "'It's just aluminum--just put it on good and quick.'"?

Schawlow: Yes. Or was it "good and thick"? I'm not sure. There are various accounts of this.

[William M.] Gray was older than we were; in fact, he'd been a demonstrator when I was a freshman, he already had a master's degree, but he had gone away during the war. He was a rather timid person. He built an evaporator where you could have both plates facing the source, which is a tungsten filament. You'd put some aluminum on it and evaporate it, but when you'd start evaporating the air pressure would go up in the thing; you evacuate as best you can, but then the air pressure would go up because gas is released from it, gas dissolved in the aluminum. So he did it very slowly and carefully, taking twenty minutes or so to evaporate a film, and the films were just terrible. They had very low reflectivity.

Well, we had a visit about that time from A.G. Gaydon of Imperial College, London. He was a noted spectroscopist. We were telling him about this problem and he said, "Well, when Hilgers"--the famous optical company in England--"coats their mirrors, they just put on a little aluminum and blast it off." So then we wanted to try it real quick. But Gray was too cautious, he wanted to keep the pressure down.

However, he was married, with one child, and a second child was about to be born, so he had to take some time off to take care of the first child. And while he was doing that Kelly and I took over the evaporator and blasted things off and got much better films. We actually published a paper on that. That was the first paper I ever published. Later on, electron microscope people studied the structure of the films and saw

that they were different if they were produced fast. Basically what happens I think is that the vapor pressure of the aluminum goes up much faster than the evolution of gas, so that if you heat it good and hot, and fast, then the atoms can beat the air atoms to the substrate. Of course, ideally we should have had a super vacuum system.

Riess: Did you have any idea at that time how much you were lacking?

Schawlow: Yes, some of it, but we could do something with what we had. After we finished I did some work on silver, which turned out to be wrong, but we published it.

The silver was a very fine structure pattern, and we resolved it, but there were two isotopes and they were very close together. We tried to identify the lines with the isotopes because one was more abundant than the other. Well, so help me, when we started out the published values were fifty-three to forty-seven percent. And that we could resolve. But I think that by the time we finished someone had redetermined that it was forty-nine to fifty-one. Well, we did it as carefully as we could, and we thought we had the one that was more intense, but later people got separated isotopes and found that we were wrong on that particular point. We did put in some other ideas there, though, that were worthwhile.

Talking about evaporating metals, Kelly had to have a thesis, so we settled on magnesium and measuring the nuclear movement of magnesium. Magnesium turned out to be very, very hard to handle because every chunk of magnesium we'd get, the outgassing was just terrible, and we couldn't just blast it off with the atomic beam. We had to have a steady beam. It required, oh, about four hours exposure, something like that.

There was a professor in metallurgy who had come recently to there from a government lab--I think it was Chalk River--and he had worked on a process for refining magnesium, and he had some chunks of magnesium that had been vacuum-melted. He gave us a few pieces, and with those we were able to do the magnesium. But with any commercial magnesium we couldn't do at all.

One of the other problems that I may have mentioned in the notes that I wrote was that although this light source's electron beam gave a current of a whole ampere, it really wasn't very bright, and that is because we didn't have very many atoms when you have to filter them out and get only those going in a certain direction. I guess I didn't quite explain there that you have to have them in one direction because then you can observe them perpendicular to the direction of the

beam, neither going toward you nor away from you, so you don't have the Doppler-broadening. At least, it's much reduced.

We required, at least for the silver, exposures of about four hours at night--day or night. But if the air pressure changed, that changed the effective spacing between the plates and would blur out the pattern. The first solution, obviously, was to put the plates inside a box with quartz windows. We had to have quartz because it was working in the ultraviolet and glass doesn't transmit down there. Well, they couldn't afford to buy us a couple of quartz plates. They didn't have to be real good optical quality, but anyway, we couldn't get the quartz plates.

We called up the weather bureau and found out--. We knew that we had to hold it within a hundredth of an inch of atmospheric pressure, a hundredth of an inch of mercury during exposure. And the only time where that would ever happen is between midnight and four a.m., because otherwise the daily variation of atmospheric pressure is much more than that. So we had to start out, get everything ready, and start the exposure at midnight. We were recording the data on photographic plates, and if anything went wrong, well, it was lost, but we had to stay on to vaporize all the silver or other metal because otherwise we'd crack the crucible when it would be solidified. So I'd be coming home at 4:30, 5:00 in the morning, and there aren't very many streetcars at that time, it was kind of chilly.

It certainly was annoying not to have those few dollars to get those quartz windows, but we had to make do with what you could do.

Riess: And Kelly and Gray were in the same straits?

Schawlow: Yes.

Riess: When did you have your Hochheim dream?

Schawlow: It was about that time when we were thinking about the silver, the aluminum-coating of the plate. Now somebody, a friend, I think it was Pat Hume, another graduate student, had brought in a cartoon by George Grosz of a German Ph.D. looking very formal. So we put that up and labeled it Dr. Hochheim--but we had no idea what he looked like.

Riess: It sounds like one of the best things about all this is that you were working in a team.

Schawlow: That helped. It really helps. Gray pretty much just worked on the spectrometer, which was fairly novel, and the Fabry-Perot

interferometer. Kelly worked with me on the atomic beam source, particularly worked on the electron-gun, but we all worked on everything a little bit.

Riess: Talking things out with somebody else is useful?

Schawlow: Oh, yes, that's a big help. It really is. We didn't bother Professor Crawford much with these details, we sort of worked them out among ourselves. I remember Gray--he was sitting there one day just fuming, he really wanted to go and see Professor Crawford and say he couldn't do what Crawford had asked him to do in the way he asked him. I said, "For Pete's sake, just do it any way that works. That's all he wants." And he finally did it that way. He'd been around--he got his bachelor's degree I think in 1936, something like that. Here it was '46 or later, and he was just used to taking orders, and timid about trying something on his own.

Riess: That is an important thing, and I guess you've had a lot of experience with that with your own students. How you get to an answer--it doesn't make any difference?

Schawlow: No.

Riess: Efficiency is not a hallmark?

Schawlow: Students are very different. I've had one student who was quite good, but he could not work alone at all--and I really can't work very well alone, either. This one student, he was there for a year or so, just kind of fussing and fuming, afraid to do anything for fear of making a mistake. We had a visitor from France and they [the visitor and the student] did wonderful things in a few months. I said, "Well, just go on and do some more of that." Well, nothing happened until we had another visitor from Germany and they did some other things. I said, "Okay, you've got enough for a thesis now." Turns out this fellow has gone to Lawrence Livermore Lab--Jeffrey Paisner is the name--and he's now in charge of the planning for the giant national ignition facility. [See also Chapter V]

Riess: National ignition facility?

Schawlow: This is for thermonuclear fusion. They're trying to build it--I don't whether it's authorized by Congress yet or not. They use laser fusion, where they have very high-powered lasers aimed at a little pellet of heavy hydrogen and heat and compress it enough that you get fusion of hydrogen atoms to produce helium. If you do that, you could get a lot of power out of it ultimately. But at this point they are trying to show that they can break even, get more out than they're

putting in. They're not designing a reactor yet. That's a huge project.

Riess: It's a nice point, that some people really need someone else. They cannot create an internal dialogue about a project?

Schawlow: Yes, I think I need somebody. I really work much better with one or two people. At Bell Labs I had a technician who worked with me. I think I might've done better if I'd been working with another physicist, but that wasn't the way we worked at Bell Labs.

Riess: Before we make that leap, this first publication, did you get any kind of response to it?

Schawlow: Well, the only thing I remember is that it was just a letter to the editor of the *Journal of the Optical Society*, which was something less than a page in length. One of the assistant professors at Toronto, David Scott, had taken over the electron microscope, and he did some electron microscope studies on films that were produced fast and slow, and showed the differences in the structure. So that was some response. Otherwise, I don't know. I guess Hilgers had produced good films and probably others too, but there was nothing very much out in the open literature telling you how to do things.

[tape pauses]

Schawlow: I was never really deeply involved in radar, except in that one year.

Riess: Radar was so new?

Schawlow: Yes, oh yes. It was a great surprise to the Germans. I heard that the British let it be known that they were feeding carrots to the night fighter pilots so it would improve their night vision, whereas actually they were using radar. That was a big surprise and a great help to the British in the Battle of Britain, although the Germans by the time were also working on radar. I don't think they had it in the airplanes at that point, but I really only know from reading some popular books. I wasn't deeply involved in it.

Riess: In terms of the development of microwave work, was radar a necessary step?

Schawlow: Probably--although we had in the lab at the University of Toronto a klystron, which was quite new at that time. I think we had it before the war, or just about the beginning of the

war. This is a low-power microwave tube. They now make klystrons that are very high-powered.

This was developed at Stanford by the Varian brothers, Russell and Sigurd Varian. Sigurd was an airplane pilot and he wanted to do something to help prevent collisions of airplanes, so he thought if he had some kind of microwave thing, you could beam it. I guess he was thinking of collisions with the ground or mountains and that sort of thing. I don't think he ever actually did anything on the collision aspect of it, but he and his brother Russell had a little room in the basement of the old physics building and built the first klystron.

During the war, they and some others went to the Sperry Company and developed klystrons for military work. Mostly low-power, I think. After the war they formed Varian Associates which developed very high-power things, millions of watts, which were used, I think, both in broadcast transmitters and linear accelerators, like the Stanford Linear Accelerator Center.

Riess: Did you know the Varians?

Schawlow: No, I didn't. I never met them. They died before I came to Stanford, and I'd never been on the West Coast before 1961. I knew Chodorow and Ginzton, who had worked with them. Ginzton later became chairman of Varian Associates, but he was a professor at Stanford when I first came. [pause] That's Ed Ginzton and Marvin Chodorow.

Riess: You got your Ph.D. in 1949.

Schawlow: There's one more thing I want to say about the Ph.D. Many years later when I was in China, Shanghai, I was talking to a group of students and I couldn't resist saying, "Well, I know you're poor, you don't have all the equipment you'd like, but we were much poorer when I was a graduate student." [chuckles] We really were. I mentioned those windows we couldn't get. I burned out a ten dollar thermocouple vacuum gauge and they wouldn't buy another one and I had to take it apart and rebuild it--which I guess was good experience, but--.

I think the only research money they had was something that Professor Crawford and Professor Welsh had gotten. By working overtime teaching during the war they managed to get the university to put aside some money from their overtime pay for research, but it was very little. Fortunately, the university had been pretty good in the twenties and there were some things left over, like these very fine quartz plates that we used.

Crawford and Welsh, and Women Students

Riess: Okay, now I've read in several places, articles by you, how much you admired Malcolm Crawford. I want to be sure that he has been adequately covered. Why and wherefore?

Schawlow: Toronto was so dead in the thirties. In the twenties it had been an active center under J.C. McLennan, and one heard all sorts of stories about him. He was a real autocrat and he got a lot of results, a lot of things done, but he drove away some of the brighter people. And during the thirties it was very poor due to the Depression, and the then chairman, E.F. Burton, tried to find jobs for as many people as he could and he encouraged them to take them--and usually it was the better people who took them.

But Crawford was a very independent-minded man and he just kept on doing research. They all had heavy teaching loads, but he still put in hours late at night and did some very nice work on basic atomic physics. He told me that he had written the first paper that showed that nuclei are not like electrons, that is they're not so-called Dirac particles, the angular momentum is not simply related to the charge. He did that by showing that the hyperfine splittings of the two different isotopes of thallium were not the same. They had the same spin had different magnetic moments, whereas two electrons will always have the same magnetic moment. Of course, this is something that is well known now, but it was at that time an interesting discovery.

He would talk to us about things. I had some courses from him. He wasn't a good lecturer, he tended to write everything down on the board. It's a bad habit that I tended to acquire. [laughs] But he was clear and he would talk about the basic physics. And also when you would talk with him he would discuss and speculate about what he thought might be important in the future. He was a little man, fairly short, but very intelligent and extremely hard-working. He unfortunately died of a heart attack at the age of about fifty-five or something like that.

Riess: Besides you, did he turn out, a number of--

Schawlow: Oh, a huge number of students, particularly after the war.

Riess: In atomic physics?

Schawlow: Well, a moderate number in atomic physics. But he was also working on molecular physics. And after the war there was a flood of graduate students.

Another professor, Harry Welsh, was a slow developer. He had a very bad stutter and Burton wouldn't let him lecture. So he just was running the advanced student laboratory. However, during the war they were desperate for teachers, so he started to lecture, and he was a very good lecturer. He was slowed down by his stutter, but I think if he hadn't had the stutter he would have been too fast. But he was very clear. I took a course in molecular spectroscopy from him. He also had many students, all in molecular spectroscopy.

He later became a big wheel. I think he was head of the department during the period of rapid expansion in the fifties and pushed to get a new building, which they did, and build up the department, really rebuild it. They had several department heads after Burton, and they were flops. But Welsh took over and did a great job. He had an awful lot of students. I think Crawford had about half a dozen before me, before the war, and so on, but I think he must have had at least as many after the war in atomic physics, putting a lot of his effort into molecular stuff too.

Riess: I noticed one woman in your class picture. How far did she get?

Schawlow: Very sad story, I probably shouldn't say it. She did go to McGill. I don't know what she did during the war, but after the war she got a Ph.D. from McGill in microwave spectroscopy. And she published one paper, which was totally wrong, and then she got married to another quite distinguished astrophysicist--his wife had died. They married, and I suppose she had a good life after that. But it's a pity--this one paper was just so transparently wrong that I didn't want to refer to it. What she had done was, she had observed a series of equally-spaced lines in the microwave spectrum of ethyl alcohol, and these equally-spaced lines were quite obviously the resonances in the waveguide.

Riess: So this one time was it?

Schawlow: I don't think she did any more after that, yes. I think she probably did a reasonable experiment, but whoever was supervising her didn't catch that. I think again probably it was that McGill--they were starting up after the war and had a huge number of students, and they didn't have anybody who knew that field. When I saw her paper I was working with Charlie on the book and by that time knew something.

Riess: It was probably was unusual even to have one woman.

Schawlow: Yes, well, we had about four to start. I think one other finished--Grace Smith.

Riess: Now are we talking about graduate or undergraduate?

Schawlow: Undergraduate. Graduate years, there weren't any in our group. There were some women around the physics department who had Ph.D.s, but they were in very lowly positions or just demonstrator, which is the sort of teaching assistant. I think eventually they got to be professors, but it was a long time coming. I think it was Welsh who pushed that through.

One of them, only one of them, Elizabeth Allin, did research. Crawford got her active again and she published some papers. She knew physics well. We had her for a modern physics course in our senior year of college. But I think she had sort of given up until Crawford got her back to work on research. She's still alive, I've had a couple of letters from her, but she's over ninety now.

Riess: I was wondering whether the war years were an opportunity for more women?

Schawlow: Well, yes, the only other one was this Elizabeth Cohen, who took that picture of me. She had a Ph.D., I'm not sure in what field, and she was employed in teaching during the war. I guess she must have been around after the war because that picture was taken in 1949, I'm pretty sure. So she was probably a lecturer or something like that.

Hindsights

Riess: The picture from your undergraduate years--it's a strikingly homogeneous body of people, unlike anything you'd ever see in California. Were there any Indian students or anyone from the Continent?

Schawlow: Not undergraduate. Graduate years, we had a student from India, we had a Catholic priest from Quebec. Both of those are, well, semi-sad stories. They had to get through in a certain number of years, I think it was two or three years. So they did, with a bit of a push, but then they never did anything more scientifically. The Indian, Manual Thangaraj, was from Madras, I think, that is, southern India. He became president of Madras Christian College, so he must have had a

good teaching career. But I think he didn't do anything in research after that. A lot of them didn't.

It was not a place that attracted people internationally. It really wasn't that good. I think it became so later, but--

Riess: So that anyone who was a colonial could have gone to England, I suppose.

Schawlow: Well, that was considered the great thing. You could get these 1851 Exhibition Fellowships to go study in England, but I wasn't eligible, not being a British subject. I don't think it would've been good, anyway.

Riess: Why? Wasn't Cambridge the best thing?

Schawlow: Cambridge was. Oxford was very good, post-war years. Yes, they were pretty good places. But the particular things that were going on there--well, I could get interested in lots of things.

Riess: What you were doing in your graduate years set you up so perfectly for what you have continued to do.

Schawlow: Yes. It worked out well. Unlike Charlie Townes, I don't plan my career very well, I just kind of take advantage of what opportunities I can see.

Well, yes, it has worked out, in the end it did, but really, I went through a lot of other things. Microwave spectroscopy is quite different from optical spectroscopy, but of course, I'd been interested in microwaves so it wasn't so hard. But then I worked on superconductivity, and that had nothing whatever to do with what I had done before. And that was difficult, I wasn't well prepared for that.

Then, of course, we did fortunately get ideas about lasers and I was able to get back to optical spectroscopy and lasers. Now there I've had a very good background for it, having both radio frequency and optical work, because the radio frequency ideas carried over into lasers.

Riess: In the Nobel Prize description of you, it says your thesis made you aware of "the need for a coherent, narrow-band source of light with which to analyze the structure of atoms..."[1] So it's very tidy.

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[1]*The Nobel Prize Winners in Physics*, Salem Press, Pasadena 1989.

Schawlow: Yes. That's true. I used to wish I could just reach out and grab those atoms and make them stand still. [chuckle] But they wouldn't do that. If they're free, they're bouncing around.

Riess: Is that an image that you really had?

Schawlow: Yes, I had that. Of course, many, many years later we found ways to slow them down with light, laser cooling, which we'll come to. Since then other people rather soon learned to trap them, so they really can hold them practically still and get them very, very cold so that they have almost no motion. So they can observe them for a long time without any disturbance from the motion of the atoms.

Riess: That's neat, essential, I suppose, that sense that the atom is so physical that you can--

Schawlow: Picture grabbing it, yes. You couldn't grab it, though, because you'd have to hold it some way, and that would disturb it. They now have traps, however, which can provide minimal disturbance to the atoms. That's another one of those--

Well, my three most important papers, really, have been ones where I put in ideas, theoretical ideas, of a low caliber as far as mathematics is concerned, which were important: the one I mentioned on the properties of nuclei where you can take seriously the nuclear size correction; and the second one was, of course, the laser paper, the optical maser paper; and then the laser cooling paper. I did that with Ted Hänsch, who was then at Stanford. He's been here the last week. He's going home tomorrow. That's H-Ä-N-S-C-H. His name is Theodor, but he insists is Tay-o-door--doesn't like being called Theo or Theodore, so he calls himself Ted.

Riess: That paper was what year?

Schawlow: Nineteen seventy-five. Nothing happened about that for about six years, I think. Then Steve Chu, who was then at Bell Labs, sort of rediscovered the idea, and later realized that we had already published it. But he actually did it, and it's a rather difficult experiment. We didn't do it at the time, didn't try, because we were very much concentrating on hydrogen.

Our interest in hydrogen was because it's the simplest atom, and therefore the one that you can compare with theory most closely. Ted was and still is working on hydrogen, but hydrogen requires deep ultraviolet light and there wasn't and still isn't a suitable laser for cooling it. You could cool

atoms like sodium that emit and absorb visible light--but I guess I shouldn't get into laser cooling any more at this point.

II COLUMBIA UNIVERSITY

Carbon and Carbide Fellowship

Schawlow: I was just a young student and Canada was a backwater then, I really felt it. By the time I got toward the Ph.D. I felt maybe I could do some good science. But all the way I had never considered becoming a Canadian citizen because I felt if I was going to do science I would have to go to the United States. There's some pretty good science in Canada now, but there wasn't at that time, really. It was really not up to international standards.

Riess: Okay, I hope you will now continue the story of hearing Rabi lecture, and talk about Columbia.

Schawlow: Well, I guess there's not much more to say: I went to this meeting of the Canadian Association of Physicists in Ottawa, and most of the talks were about the right of physicists to practice as professional physicists or engineers. I thought that was pretty dull, but Rabi gave an invited talk in which he talked about the recent discoveries which led to quantum electrodynamics, and really was new physics.

I thought that was really quite exciting and that I really wanted to go to Columbia, so I wrote to him when I was finishing up and he suggested I apply for the Carbide and Carbon Chemicals post-doctoral fellowship to work on applications of microwave spectroscopy to organic chemistry, with Charles Townes. As I say, I didn't really know his work at the time. I should have, because we did have a seminar and some of Charlie Townes' papers had been discussed in there. I had an atrocious memory for names and didn't make the connection.

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Schawlow: An amusing thing, I don't know how it ever happened, but we had a neighbor a few doors away who worked for Carbide and Carbon Chemicals, and he asked me to come over to his place and sort of interviewed me. Somehow, I was being considered for this fellowship. I don't think that Carbide and Carbon Chemicals Corporation really had any say in the thing, but somehow or other he'd gotten word of it and decided he should interview me.

Riess: In fact, did you ever have to report back to them?

Schawlow: No. I was the second fellow of this type. The first one, they'd had him visit their plant in West Virginia, I think, and he gave a talk, and they found it hard to believe what he was telling them, though it was true, namely that the peak strength of the microwave absorption line in a gas doesn't depend on the concentration, on the pressure, the reason being that it broadens just as much as it--the total intensity goes up, but it broadens so that the peak intensity remains the same. And they found that hard to believe.

The history of that fellowship: it may be that Charlie has told about it, but Helmut Schulz was a chemical engineer with Carbide and Carbon Chemicals, and he had a lab accident that had damaged his eyes, he was almost blind, so they put him in a position to do some long-range planning sorts of things. He had the vague idea that one could control chemical reactions by some kind of radiation that was longer than visible light but shorter than radio waves, something in the infrared essentially. But there was no good way of generating those infrared waves. And so he looked around to see where they could put some money that might advance that.

At Columbia Charles was working on the interaction of microwaves with molecules. They also had the radiation lab there that was still working on millimeter wave magnetrons, so they were working on molecules and short wavelengths. So they decided to give the money for a post-doctoral fellowship at Columbia.

Riess: Did you meet Schultz?

Schawlow: Yes, I did. I met him several times. Nice guy. In fact, he showed up a few months ago. He's now pretty old, but he was coming out with his wife, visiting various places. He managed to drop in and we had a nice chat.

So he was the one that brought us together, and in a way this sort of thing--the fact that we were together--led eventually to the laser, which does give you a potent source of infrared. But I don't think controlling chemical reactions has really been very successful in the infrared. It's partly much too expensive because photons are expensive to generate, and most chemistry is done in batches of tons and sells at cents per pound. Laser photochemistry has been used to separate uranium isotopes which are, of course, very valuable, and while that's expensive it is cheaper than other methods of doing it. I didn't work on that.

When we thought of the idea of a laser, the only application I had in mind was this thing that Schultz had suggested, maybe control chemical reactions, because it's obvious that chemical reactions usually go faster if you heat them up and here is a very selective way of heating them up. So at Stanford I had one student working on trying to separate bromine isotopes, and we were able to get a selective initiation, but the isotopes were scrambled before the reaction was completed. What I didn't realize was that if you were going to do it you had to do it very fast, so that you complete the process before some competing collisions scramble it all up again.

After Tiffany did his thesis and left--that's William Tiffany--other students didn't seem interested in doing what was clearly chemistry, perhaps not so much physics. Also I began to worry a bit about the separation of uranium isotopes; I didn't want to do anything that would speed the day when it would be easy to make bomb materials. So I decided I just wouldn't work on isotope separation anymore, other people could do it. It's okay for labs like Livermore where they have huge facilities and some secrecy, but it's quite possible that I might have discovered a way to do it cheaply in a garage or something like that, and that would be horrible--and terrorist organizations and criminals could get atom bomb materials.

That's about the only time I ever steered clear of any subject for any ethical reason, but it was partly because students didn't want to do chemistry, and I guess I didn't either, to tell the truth.

Riess: But they hadn't focused on the ethical issues themselves.

Schawlow: I don't think so, no. But of course, bromine was quite different from uranium and had no applicability.

Riess: So you've described the intent of the fellowship--

Schawlow: In fact I was just another person in Charlie Townes' lab, where he was working mostly on organic materials. But he asked me to try and see if I could detect the spectrum of a free radical OH, that is one oxygen atom and one hydrogen atom. It's interesting that even then his real interest in it was for astronomy, because he thought there ought to be OH out in space. He thought if we could detect it, it would be an interesting probe for the conditions around stars.

I had a hard time--again, I didn't have the equipment I needed and that I knew I needed, although they had an awful lot of microwave equipment. Trouble is that you could find out pretty quickly that OH can be produced in a gas discharge. I made a spectrometer with a long tube that we could run current through to get a discharge, but then the trick was to know when we were producing any OH. The difficulty was that we could have done it very well if we'd had a good spectrograph, because the spectrum was at that time was well known--it's in the ultraviolet, it was known--but we didn't have one.

Columbia had sort of missed the boat in the twenties. It had been pretty moribund and didn't have a lot of spectroscopic equipment lying around. And Charlie didn't feel like buying one. So we tried to use a chemical test that was published that said that if you have OH, you'll produce hydrogen peroxide if you let it condense on a cold finger cooled by liquid air or something like that.

Riess: On a cold finger?

Schawlow: Finger. Yes, you take a jug of liquid air, a canister that contains liquid air, have a tube going down at the bottom which sort of looks like a finger. You let the stuff condense on that, and it's cold enough that it will condense. Well, we got lots of hydrogen peroxide, but with still no OH spectrum.

I did some other things with people in the lab. I had a student working with me, Mike Sanders, T.M. Sanders, Jr. He was quite good, but we didn't get anywhere. We used an ingenious spectrograph: instead of using electric field modulation we used a magnetic field, wrapped a coil around the long tube, and put an alternating current through it so it would produce alternating magnetic fields. We thought that was clever, but Walter Gordy at Duke had done the same thing about the same time and published before us--he used it for oxygen, which is also magnetic. Whereas normal atoms are not, or molecules; it's just the occasional one like oxygen, or the free radicals would be magnetic.

I was there two years, and after I left Sanders and another student were working there one night when something went wrong with the discharge conditions and they happened to be sitting at the right wavelength and saw the absorption lines. So this hydrogen peroxide test was just a wrong way to tell whether you had OH or not. Of course, once you have the microwave spectrum then it's a good test. But I've always regretted that I didn't have an optical spectrograph to test whether I had OH.

[tape interruption]

Charles Townes and the Microwave Spectroscopy Book

Riess: I thought Columbia was where microwaves spectroscopy was going on, but they didn't have the equipment you needed?

Schawlow: They had microwave equipment, lots of it, but they didn't have the optical equipment that I needed to go with it.

Riess: And yet, between you and Charlie, you really wrote the book eventually.

Schawlow: Yes, he asked me to stay on and help him write the book on microwave spectroscopy. So I did stay for a second year. The fellowship was only good for one year, but he got some money from the Ernest Kempton Adams Fund, I think, that Columbia had, to support me for another year. The department had suggested that I might want to be an assistant professor, but I said I just couldn't see how I could teach, do research, and write the book, so I turned that down.

Riess: It was a book that was ready to be written?

Schawlow: Well, it's funny. Charlie Townes really wrote the book. He wasn't the only one working on microwave spectroscopy, but he was one of the leaders. Walter Gordy at Duke, and M.W.P. Strandberg at MIT, and D.E. Coles at Westinghouse, those were the main ones. And there was quite a rivalry between Charlie Townes and Walter Gordy at Duke. Some book salesman came and told him that Gordy was going to write a book, and so he decided that he would write a book too. [laughs] I think everybody agrees that it was a better book because Gordy was sometimes a little slapdash.

But anyway, he asked me to help him on it and so I did, I stayed another year, which was good because that's when I met my wife, Charlie's younger sister.

Riess: Let's step back a little bit to have you describe this time. It's very fabulous for me to imagine you leaving Toronto and coming to the big city. How did you find a place to live and how helpful was Charlie? Was Charlie really in your life at the beginning, or were you just anybody?

Schawlow: He was very nice to me, invited me over to have dinner once or twice. At one time, they permitted research associates to join the faculty club. Since they didn't have anywhere else to eat, I often did eat over there. I got to know some of the professors that way, both in physics and mathematics.

Riess: And where did you live?

Schawlow: When they notified me of this fellowship, they said that I had to live in the university dormitory. I think that was not really correct for post-doctoral fellowships, but not knowing any better I took a room at a John Jay Hall, which I found rather annoying, but I didn't know any better or what else to do. I was there for about a year and a half. The thing I didn't like was the walls were rather thin, so I couldn't play my records very loud, or even what I consider a moderate volume, or I'd get complaints.

Also, in New York City and in John Jay Hall they had direct current. It was a legacy of Edison, who didn't believe in alternating current. It meant that any kind of radio equipment wouldn't work unless you got a converter, which I did. I bought a converter, a kind of a vibrator that converted DC into AC, but it really wasn't very satisfactory.

Riess: You came down from Toronto by train?

Schawlow: Yes, I did.

Riess: You packed up a wardrobe and your records. What else did you pack?

Schawlow: Did I take records with me? I guess I took some records, and of course I bought more in New York. I probably brought the record player, too. Some books.

Riess: Were there any formalities of reestablishing your citizenship?

Schawlow: Yes. There was a little bit. I went to the U.S. consulate in Toronto and presented my birth certificate and said, "Is it okay? Can I go back?" And I said, "No, I'd never voted in an election in Canada." They said, "I guess that's all right." Actually I think I could have crossed the border without bothering with any of that stuff.

While I was there I registered to vote in New York, and it was quite amusing, I was told that if you had a high school diploma you didn't have to pass the literacy test, but a college diploma only proved that you could read Latin. So I had to take a literacy test which of course was not difficult.

Riess: Did you go back to Toronto for your holidays or had you really made a break?

Schawlow: I went back for vacations, some. I did go back several times a year, and I started to go by plane. The first Christmas season, I think, that I was there, I went back by plane. And then the weather was so bad I had to come back by train, because the plane wasn't flying.

I felt very lonely at first. Although I'd never been a great lover of gardens or trees and things, I really missed the greenery. In New York, it was all concrete practically. But I got to meet people. I joined the Riverside Church and they had a young people's club that I went to, so I got to know a few people there. I got to know some of the graduate students at Columbia pretty well, too.

Riess: And did you get right onto the jazz scene?

Schawlow: I went out there occasionally, yes. I'd visited New York a couple of times when I was a graduate student. I knew where the places where, and I would go out occasionally. I couldn't go very often because it meant staying out very late at night. They used to close at three a.m. in those days. You'd come home on the subway at three o'clock and see people like milkmen going about their business. Nobody would bother you at all. It was really a nice place.

There were some stores that specialized in jazz records. It was interesting--I'd found this the first time I'd visited there, in '47, that these stores, unlike the ones in Toronto, knew exactly what every record was worth--usually priced a little bit more than what I would pay. The ones I would pay more for, they raised the price. They knew exactly what they were all worth.

Riess: You were filling in your collection?

Schawlow: Yes. And expanding--building a jazz record library.

Riess: Has that been a lot of what being interested in jazz has been for you, is to create a complete archive?

Schawlow: Yes, within certain ranges. I mean, obviously I can't get everything that's been done, but for the major artists that I really liked and admired, I try and get everything I can. I'm really missing it now: I had complete sets of Tommy Dorsey, Artie Shaw, and Bennie Goodman that were issued on Victor Blue Bird label a decade or so ago. I put most of those on mini-discs and I cannot find those mini-discs now. I'm feeling very frustrated. They're out of print.

Riess: You'll find them.

Schawlow: Maybe. Or they'll reissue them.

Riess: The Riverside Church, you had your music life, you had some friends, but the question of whether to stay on the second year: if you hadn't stayed on the second year, what was going to come up next for you?

Schawlow: Actually, the University of Toronto contacted me and another fellow, this Pat Hume that I mentioned before, who had gone to Rutgers about the same time I went to Columbia, and they asked us if we'd be interested in an assistant professorship. I asked if I could postpone it a year because I'd already promised Charlie that I would stay and help with the book. Well, that didn't work out, so he [Hume] took the job. I probably might have gone back to Toronto if there hadn't been anything else in sight.

Riess: Did you go out to Brookhaven when you were in New York?

Schawlow: No. I didn't. Charlie was there during the summer, just before I went there. In fact, he was still there over Labor Day weekend. He invited me and my predecessor Carbon and Carbide Chemicals fellow to go out there. I got a most horrible sunburn.

Riess: I've a note that Charlie was extraordinarily effective in getting the best from students and colleagues. How would you describe how he worked with you, for instance?

Schawlow: I don't know. He would make suggestions, but he didn't supervise me very closely, not on a day-to-day basis. He had weekly meetings with his graduate students and they would present some aspects of their research to be discussed there, and I think that helped to stimulate. He had a large group so they sort of supported each other in some ways; they could discuss things with each other.

For me, well he suggested various things. After I'd been there for little more than a year, and I was still stuck on

this OH experiment, he had me help some other students on other projects so I'd get some publication before I'd have to leave. I did that. I wasn't terribly interested in it, but I did what I had to do there. I really still wanted to struggle with the OH, because that was the first free radical that was found with microwave spectra.

Riess: Working on the book sounds like it could have derailed you.

Schawlow: Yes, it did some. The trouble was I was not an expert on microwave spectroscopy at all. I really wasn't. I'd been there only a little over a year when we started on it, so I sort of drafted several chapters that seemed like they were not too specialized. I did chapters on atomic spectra and diatomic molecules, and then later on pressure broadening and on millimeter wave techniques. But I had to study these up, each one, because I really wasn't an expert on microwave spectroscopy.

Riess: It sounds very uphill.

Schawlow: Yes, it was--and then it kept on and on. We didn't finish it while I was there. The next three years, I think, I would go in many Saturdays and work on the book while I was at Bell Labs. It was a distraction all right, but I felt from the beginning if we were going to write a book at all, we wanted to write a classic that would be something that everybody would respect and turn to, and I think we did. It's been very widely used and quoted. I think, frankly, Charlie's part was the more important part, because he knew the stuff and I didn't. But I did study up some and wrote some of the things.

Meeting and Marrying Aurelia Townes

Riess: Before we finish for today, and since we're being very strictly chronological, when did you meet Aurelia Townes?

Schawlow: It was in the fall of 1950. I went to Columbia in '49 and I was there for a year, and I was frankly beginning to look around a little bit to see if I could meet a nice girl, and I never took one out more than once. And then she came by my lab, he brought her around. He'd brought his older sister Mary before and I didn't pay any attention to her. They kind of looked in the door and I figured, well, his sister is probably older than me, I didn't really take a good look.

Then Frances [Mrs. Charles Townes] invited us to dinner and made sure we got to know each other and we started going out together. It wasn't very long before I proposed. She took a little while longer to decide whether she wanted to do it or not, but by January I think we were engaged.

Riess: Was she already living in New York for her music study?

Schawlow: She had been there before to study singing and music in general. She'd gotten a master's degree in music education from Teacher's College, and she'd come up this time to take more studies, mostly with a private teacher, Yves Tinayre. She did take some courses at Julliard and at the Mannes College of Music.

Riess: She was seriously pursuing this career?

Schawlow: Yes, as a singer. But it's a very hard, competitive field which she eventually gave up when we got married, pretty much. Well, she was still going in to New York to work with a pianist and an accompanist. We moved to New Jersey after a year, in September of '51, I think it was, when I started work at Bell Labs. And she was still going on the train to work with her accompanist and also take lessons from Yves Tinayre.

Riess: What were your impressions of the Townes family when you met them?

Schawlow: They were very nice to me. It was a bit overwhelming to meet all of them at once, but I guess Charlie had said some good things about me and they were quite nice to me.

Riess: South Carolina--were they terribly southern?

Schawlow: Pretty southern, but they were all very intelligent, and most of her brothers and sisters had studied in the north somewhere. I think two of them had studied at Cornell and two at Swarthmore. They were southern all right, but I wasn't particularly prejudiced against southerners, which a lot of New Yorkers were. They were something really just outside our ken in Toronto. We'd heard about southerners, and we'd heard about lynchings. I think it was Mike Sanders who said, "When you meet her father, just ask him, 'Have you seen any good lynchings lately?'"

Riess: I'm very ignorant about the south, but I love the accent. Did she have an accent?

Schawlow: She did sometimes. She could sort of turn it on and off. It'd depend on the circumstances.

Riess: Did you get married down there?

Schawlow: Yes, we did. That was the first time I met them.

My mother came along and we flew down. At that time the only plane was a propeller plane, of course, and unfortunately I think it stopped about five times. They served potato salad for lunch, and just after the second to last landing it came up. [chuckle] I remember her father said after I'd been on the ground a while, "I'm glad to see Art isn't always that color." [laughs]

Riess: It was a small wedding?

Schawlow: Her father had had a heart attack not long before that so they decided to have the wedding in their garden--they had a nice garden. It was a simple wedding. I could show you the video.

Riess: Oh! Really?

Schawlow: Well, Charlie had his movie camera and he took some pictures. I got some copies made on video lately. They didn't do a good job. Everything looks very blue, but still--we do, yes we have a little video. Not in very great detail, no sound.

Riess: And so from your side, you had your mother and your sister?

Schawlow: No, just my mother came down.

Riess: Not your father?

Schawlow: No.

Riess: They couldn't afford to?

Schawlow: I don't know why. I think maybe my parents thought he seemed rather foreign. He still had some kind of a strange accent--doesn't seem like a Russian accent or anything else, but he had an accent. I think that was it, but I don't know.

Riess: You mean that was the way he felt about himself?

Schawlow: I think probably. We just sort of didn't discuss it.

And by that time my sister was married and had at least one little child, so it would have been hard for her to come.

Riess: She was living up there?

Schawlow: Yes, in Toronto.

Theoretical Work, and Publishing on Hyperfine Structure

[Interview 3: September 4, 1996] ##

Schawlow: I've never been a real theorist, but strangely enough I think several of my best papers have been theoretical. It's a low-grade sort of theory. I don't do a mathematical calculation, I sort of look at the subject and present something differently with a minimum of mathematics.

The thing I wanted to mention particularly now--I was working on hyperfine structure of atomic spectra, and I was interested in what we could find out about atomic nuclei. So I read papers; you could read about all there was to know about the theory of nuclei in some pre-war papers. I think three of them were in *Reviews of Modern Physics*, by Hans Bethe, and each one was fairly long, but that's certainly far less than is known now. But even a simple-minded person like myself could get the general picture.

So we were measuring hyperfine structures and I looked up the theory--well, there was a formula from Goudsmit, improved by Fermi and Segré, which let you calculate the nuclear magnetic moment from the hyperfine spinnings. Now magnetic moments were beginning to be measured at that time using nuclear resonance, and so I thought it would be interesting to compare them. And I found that one had to take into account for heavy atoms the finite size of the nucleus, because the electron wasn't just being pulled in all the way, it was being pulled in until it reached the surface of the nucleus.

There were papers by Breit and Rosenthal in 1932 about hyperfine structure. And then a friend who was a student in applied mathematics, which is what they called theoretical physics in Toronto, told me about an obscure Norwegian paper which applied a method called perturbation of the boundary conditions. You couldn't use ordinary perturbation theory because going into the nucleus the electron experienced a huge change in the electric field potential, it wasn't just a small thing. However, you could imagine changing that radius slightly and perturbing it that way. So I could calculate the effects of the nuclear size, roughly at any rate, and we published this in a paper called "Electron-Nuclear Potential Fields from Hyperfine Structure" [*Physical Review*, 1949]. And this got a good bit of attention.

We also did some work on isotope shift. But it was rather lucky for me that I'd been told about this Norwegian paper by E.K. Broch, because it certainly was not a journal I would ever look at.

Riess: You were looking for different approaches? That's part of the process always in doing physics?

Schawlow: Yes, I think so. Try and do something that hasn't been done before, that's what you have to do for a thesis, and in fact to publish, too. You have to do things in a different way or do something different.

Riess: In Charles Townes's book, *Making Waves*, he seems to be making the point that since you can't know what you're going to get, you never can be too focused about what you want.[1]

Schawlow: Yes, you have to keep your eyes open and take the results of the experiment seriously, if you do get results. I've said before, probably not with you, that when you get some new results I immediately think of what it might mean. Maybe there are several hypotheses, it could be this, it could be that, and then you weed them out one by one. And probably most of them are wrong, but that's the way you make progress.

Riess: How you weed them out--this is where you think your way through it?

Schawlow: Yes, well, ideas have consequences and you might be able to do it theoretically, that this doesn't fit with something else, or you might suggest a new experiment that we should do, another test, a different test, to see whether that's right or not.

Riess: Something that I read--the idea that there's a lot of literature and old experimental work in physics that people could work on using new knowledge. It's as if one could be almost an historical physicist.

Schawlow: That's right. It's sort of like time travelling, almost, like the *Connecticut Yankee in King Arthur's Court*. And we did that. We'd go back to old issues of *Physical Review* or other journals, like the *Zeitschrift fur Physik* in the early thirties, and you'd see things they did, and how far they got, and with newer techniques you could do things that they couldn't do before. I have often thought that if I ever were short of ideas, I would just go back and look at old magazines

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[1]Charles Townes, *Making Waves*, American Institute of Physics, 1995.

twenty or thirty years ago, and see things that have been forgotten and never followed up. And there are lots of them.

I.I. Rabi pointed out in a book--there's a book about him that's well, semi-autobiographical actually, written by [John] Rigden, but with extensive interviews with Rabi. He tells about his Ph.D. thesis which was a very clever way of measuring magnetic susceptibility. He says this was never referred to by anybody, never was a single reference in the literature to this paper.

Riess: That's interesting. That reminds me of the habit practiced by physicists of making journal entries. So if you're doing your daily journal--

Schawlow: Unfortunately, I've been very lazy about that. I'd rather think than write. In fact, I was at Bell Labs for ten years and I think I filled a little over one notebook, and most of the stuff I put in the notebook was wrong. If I actually got good results, it usually was something I'd just write on a scrap of paper.

Riess: Are the notebooks considered to be in some way public property?

Schawlow: No. They can be if they're released, but otherwise not. I guess mine are still at Bell Labs, I have never asked for them. They gave me one or two pages from it, but that's about all. They gave me a *copy* of one or two pages.

The Atmosphere at Columbia, 1949

Riess: At Columbia, when you were there, there were eight future Nobel Laureates.

Schawlow: It's now eleven.

Riess: Eleven came out of that lab?

Schawlow: Well, they were around the university in one capacity or another. Counting Townes and myself there was [Polykarp] Kusch and [Willis] Lamb, and let's see, Rainwater. Val Fitch was an undergraduate.

Riess: Val Fitch?

Schawlow: Yes. He's at Princeton. I didn't meet him then, but he was there as an undergraduate student.

[Hideki] Yukawa, who got his Nobel Prize a few months after I arrived; he got it in October and I arrived in September. And let's see, there's Aage Bohr, the son of Niels Bohr.

The most recent ones--no, the second most recent ones were Mel Schwartz and Jack Steinberger and Leon Lederman. Lederman was an assistant professor at the time. Then Martin Perl got the prize just two years or so ago. He was a graduate student at the time.

I don't know if I've thought of all eleven or not, but--. Well, it was really an exciting place. And physics wasn't so diffuse as it is now. Well, they sort of concentrated. It was kind of nuclear physics, and atomic physics details were the frontier. People could still talk to each other and they did. We would meet in the afternoon for tea and discuss physics questions.

Riess: That generosity of time and sharing is unusual?

Schawlow: Yes, I think so, the fact that they could share, that they knew enough of each other's field that they could trade ideas.

And then Rabi had great enthusiasm. He was considered a tough man to do research for because he really wouldn't bother about the details of an experiment. Two students were working on a problem he proposed, and after working for a year or more they decided that it just could not be done with that sort of apparatus. When they told him, he said, "Well, I'm sorry," and they just had to find something else to do. And they did, but that's sort of the way he was.

But he had great enthusiasm. I remember he went to Japan for a couple of months, a few months after I came there, and when he came back he came around and poked his head in the door of my lab and said, "Well, what have you discovered?" Gee, the thought that I might discover anything somehow really hadn't hit me. I think maybe "finding out something," but--. It was inspiring.

Riess: So he really put his imprint on the department.

Schawlow: Yes, and he had hired a lot of people. He hired Charlie. He heard him speak, I think, at an American Physical Society meeting and he lured him away from Bell Labs.

Riess: Then he got Charlie working on the microwave spectroscopy?

Schawlow: Well, Charlie was working on microwave spectroscopy at Bell Labs. He started it. He didn't stay there because although

they were happy to have him work on it they wouldn't give him any assistants, so he had to pretty much do it by himself with occasional collaboration from other physicists there. And he had a lot of ideas and wanted to have a group, so that's why he came to Columbia, at least as far as I know. He did build up a group rather rapidly.

I had the same feeling myself when I was forty and started getting offers. I had a lot of ideas at that time, and I just couldn't do them all. I didn't even ask because Bell Labs didn't, at that time, have people working in groups. So when I came to Stanford I got a lot of graduate students and we could try a lot of different things.

Riess: Back to Rabi's comment, what is the difference between an idea and a discovery?

Schawlow: Well, I guess I think of a discovery as being something important. [laughs] Discovering, rather than finding out. I don't think it [the comment] changed what I did, but it was sort of inspiring just the same.

Publications and Timing

Riess: One of the things I'm gathering from what you're saying is that a lot of papers get written before the actual work is done.

Schawlow: Yes. I don't know whether Charlie told this story or not.

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Schawlow: Charlie didn't publish the idea of the maser before it actually worked, and the reason was that in the years after the war a lot of people were rebuilding labs or building new ones, and they did write a lot papers proposing various experiments. People joked that we should call *Physical Review* "Physical Previews."

So by 1951 when he got the idea of the maser it was sort of just not done to publish a possibility, you should go ahead and do it. That's the way he did. He wasn't secretive. He didn't formally publish it, he put it in his progress reports which were unclassified and were in some libraries.

He might not have gotten the Nobel Prize--because you have to publish for that--except that he went to Japan and he gave a talk and Koichi Shimoda wrote it down and published it. So he

got a publication there on the idea of the maser. Because about that time, Basov and Prokorov in Russia published part of the idea. They didn't have anything he didn't have. He had more, actually, but they did publish part of the idea and they shared the prize in 1964.

Riess: But the witnessed journal entry, doesn't that count?

Schawlow: No, for patent purposes that's fine. For publication credit, no, it doesn't work. You have to publish, to get a Nobel Prize at least, and I think for most other physics prizes. You have to put your name on something, and often it's hard to decide when you really are confident enough in a result that you will publish it.

There's a story about the discovery of what was it? The W-boson? The thing that Rubia got the Nobel Prize for. There was another group at CERN, that is the European Nuclear Research Center, that was working on the same project, looking for this particle, and knew just what they were looking for. Well, he met the leader of this group in the hall one night and said, "We must be cautious, be careful not to publish prematurely, because we could be wrong." And at that time he had a courier on the way to Amsterdam with a manuscript for *Physica*! [laughter] That's a little dirtier than one usually does, but that's the way high energy physics is.

Riess: I'm surprised that there's not more of it.

This is different from the Rabi and the afternoon tea party kind of physics.

Schawlow: You keep looking for new ideas and new ways to do things.

Riess: If you talk about new ideas, people might say to you, "Well that just can't be done," and you can't listen to that, can you?

Schawlow: No, you have to decide for yourself. Of course, there is the famous example of Rabi trying to talk Charlie Townes out of working on the maser. I think he argued that it wouldn't work and he should give it up, but Charlie had done the analysis himself and he felt confident, and he was right, of course. I usually haven't worried too much about that.

There was one case where a theoretical physicist talked us out of doing an experiment, but it wasn't really me, it was a post-doc working in my lab that was going to try and do something and this theorist was visiting and persuaded him that

it wasn't going to work, which was wrong. And so somebody else did it.

Riess: Do you think too much energy goes into naysaying?

Schawlow: Some people do, I usually avoid those people.

Riess: Some people do a lot of naysaying, you mean?

Schawlow: Yes, I think so.

I tend to try to believe everything, but check it out. Even crazy things, you know, if they are exciting--like cold fusion for instance. You look at it at first and see, well, what does that imply; after a while you decide that couldn't be, at least not the way they described it. But I do know people who immediately have a negative attitude. They know what they know so well that they really can't fit in new ideas. And they're not very productive.

Seminars and Group Meetings

Riess: Were you the only post-doc when you were there?

Schawlow: No, there were two others. My predecessor as the first Carbon and Carbide Fellow was Jan Loubser. He was a South African who had obtained his Ph.D. at Oxford and he was there for a few overlapping months. Then there was a Norwegian chemist named Eilif Amble. Those were the only two in Townes' group at that time.

There were a number of young people around. There was this young Bohr, who I don't think had a Ph.D. at that time because the Danish Ph.D., like some other Europeans, really requires a lot of publications. It's not just one publication, like you need for an American Ph.D. And he really knew far more than almost anybody else, and he was a great pleasure to talk with. We had some interesting discussions.

I remember once there was a seminar and John von Neumann from the Princeton Institute for Advanced Study came and talked about the theory of turbulence, which is a very difficult subject. Bohr seemed to understand it very well; I don't think anybody else did. He was running the theoretical seminars and he asked me to talk about what I had done on this theoretical work on nuclear size measurements from hyperfine structure. Well, I foolishly agreed.

When I got in there, God, there was Rabi, Yukawa I think, and of course Townes, Willis Lamb, and Norman Kroll--a whole line of theorists. Well, I knew what I knew, and I didn't know any more than that. So I said something, and if somebody asked a question I'd pause, and usually the person next to him would answer it. But afterwards, one of the other graduate students said, "Boy, you were really shaking!" [chuckle] I was.

Riess: Theoretical seminars were built into the program?

Schawlow: For the whole department, actually. They had the colloquium, which would address everybody, but they would have the topics in the frontiers of theoretical physics. And they would have small audiences; maybe thirty people or so would come to those, whereas a couple hundred might attend the colloquium.

Riess: But that wouldn't be a place where people would come and talk about ideas they were working on?

Schawlow: Yes, they would be. Or something they had just done, their new ideas.

Riess: Having come from Toronto, what did you learn about methodology when you got to Columbia that was different?

Schawlow: Well, let's see. It wasn't really qualitatively different. I was able to work longer hours because I lived right near there and had no family.

My lab was next to the molecular beam lab. In fact, right next door was Alan Berman, who later became chief of research for the Naval Research Lab. We would often--most of them would start work at noon and work until midnight quite regularly, so I kind of got into working those hours too. I'd work long hours, do a lot of chatting.

It was amusing--when I went to Bell Labs I noticed a big contrast. It was 8:15 to 5:15, and there wasn't much fooling around. There was a pause for afternoon tea for the small solid state physics group, but otherwise people were working hard all the time. You can do things in different ways. I was impressed by the graduate seminars, the group meetings--

Riess: The theoretical seminars?

Schawlow: Well, those too.

Riess: At Columbia?

Schawlow: At Columbia. They had group meetings where different students would discuss what they were doing, or some particular aspect that they'd been asked to look into, some new development. We did that at Toronto too, so it wasn't really all that different. I think the level was higher, there were perhaps better students.

Looking for OH

Riess: Were you at all tempted to go off in other directions in that new arena?

Schawlow: Not really. There wasn't much opportunity. Unfortunately I got tangled up in a difficult experiment which I never did finish. Charlie had me, in the last six months or so, work with some others so I'd get some publications done. I was working on trying to find the spectrum of the OH radical, that is, a fragment of a molecule. He was interested in looking for it for astronomy, and indeed the OH radical was found much later on and in astronomy it's quite important in studying nebulae. I was a little out of my depth there, but it's interesting, that was where he was interested in at that time, and that's why he put me on it.

Riess: I cannot imagine the patience involved in working on something where it might take years before you see the thing you're looking for.

Schawlow: There's a lot of drudgery in experimental physics. You have to get pumps and electronic equipment and everything working. You try and fix one thing and then another, and well--. So we didn't work as consistently as people at Bell Labs did. We'd spend a good bit of time chatting with other students. That was interesting, you learned some things that way, but--.

Riess: The results Mike Sanders got were a fluke? [See pp. 75-76]

Schawlow: Yes. Whatever it was happened to the discharge, I guess maybe you had to adjust the pressure of the water vapor.

Well, we tried all kinds of things to try and get the result. We tried looking at the chlorine dioxide, which is another radical, but that spectrum was extremely complicated, we couldn't make anything out of it. We saw lots of lines, but not the OH line. It's frustrating, it certainly is. But--I don't know, you keep trying to get ideas to try this, try that.

I don't remember all the things we tried, but they didn't work--[laughs] at least they didn't get the desired result.

As I say, we spent some time looking at chlorine dioxide, trying to get the quadropole coupling of chlorine isotopes. So that took some time, and we sort of had results in that we saw a lot of lines, but we didn't really because we couldn't decipher them, there were so many lines.

Riess: Were you counting the lines or did you have equipment that could do it for you?

Schawlow: We had a scanner. There was a dial on the power supply knob that was connected to a clock motor and slowly turned the thing. And you had a chart recorder that showed the intensity of the signal. You should get a change when you go through the right wavelength, the right frequency for that particular absorption.

There was one amusing incident. Another graduate student worked with us briefly when he was just starting out, named Wilton Hardy, and we came back one day and he had chart paper all around the room. He had hundreds of lines. But it turned out that what happened was that the clutch on the motor was slipping and was just drifting back and forth across the same line.

Riess: Do you have to be taking notes when you're doing this kind of work?

Schawlow: No. Not really.

Riess: In order to know what you've eliminated?

Schawlow: No, I think we just know what doesn't work. You prepared--it took a lot of time to try each thing. You didn't just go in there and do something. You might work for weeks to get ready for this particular variant of the experiment.

Riess: When you were talking about your own dexterity--

Schawlow: I haven't got any. [laughs]

Riess: --you told how you were able to tune the one-tube radio.

Schawlow: That's one thing I've learned. I'll show you here how I do it. [moves over to hi-fi equipment] I'd put my thumb and finger here and I'd push them against each other and make the fine adjustments.

Riess: But dexterity is not needed for setting up these experiments.

Schawlow: We were looking for something on the chart recorder that was reproducible--

Riess: It can't be dependent on--

Schawlow: It wasn't dependent on dexterity, not at all.

I might mention here that Gerhard Dieke, who was the head of the physics department at Johns Hopkins University around 1960, told me that R.W. Wood, who was his older colleague and was a very famous man for his beautiful experiments, was so clumsy in the laboratory that he had to design these things cleverly enough so even he could run them. [laughter]

Riess: Have you other stories of the Columbia years?

Schawlow: One story. They allowed me to join the faculty club there, and to eat lunch there, and since I didn't have any other place to eat I did eat there very often. At that time they didn't have a lot of post-docs, there were only one or two others in the department, so they didn't mind me sitting there with the professors, and that was very interesting, to hear some of these discussions.

[laughs] I may have recounted in the introduction to Charlie's oral history, about the time they were discussing this magazine article about the top young scientists.[1]

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[1]"Another occasion, when I was at lunch in the Columbia Faculty Club with Charles and a few of his colleagues, the discussion turned to two articles which had just appeared in *Fortune* magazine. The science editor, Francis Bello, had picked ten outstanding scientists under age forty in universities, and ten in industry, and had tried to draw conclusions about what they had in common. One thing he noted was that they were all oldest sons or only sons. Charles remarked that it didn't seem right for him, for he had an older brother and two older sisters. Thereupon Rabi squelched him by saying, 'You didn't make the list, did you?' There can't have been many lists since then of the outstanding scientists of the twentieth century that failed to include Charles Townes." [From Arthur L. Schawlow's introduction to Charles Hard Townes, *A Life in Physics*, ibid.]

The Subject of Equipment

Riess: Now, Columbia had a radiation lab group in the physics department. Were you a member of that?

Schawlow: Yes. It had been a microwave lab during the war and they had developed what was known as the rising sun magnetron there. And they still had a group working on magnetrons, trying to get shorter wave lengths--millimeter waves. That was one of the things that attracted Dr. Schulz of the Carbide and Carbon Chemical Company to Columbia. That was two floors; the tenth and eleventh floor were radiation lab. And they had an administrative office that handled contracts and things like that. They had a workshop and an electronic shop too.

I was on the tenth floor most of the time, that's where my lab was. But I would go up there, I guess to order something or to get something made in the machine shop. There was a lot of equipment around there, a lot of microwave equipment. That was different from Toronto. There was all sorts of--waveguide and klystron tubes which were war surplus stuff that Charlie had acquired, and others I guess there. Willis Lamb also had experiments in the radiation lab.

Riess: Sounds like it's important to associate yourself with wherever there's money so you can get equipment.

Schawlow: I suppose so. There were strange things there--as I say, we really lacked a spectrograph which I needed, optical spectrograph. And when I came to Bell Labs it was sort of a shock again, because although they had huge amounts of equipment, they didn't have what I needed, they didn't have equipment for what I was going to do. Whereas at Columbia, I'd gone in there and it was suggested that I work on microwave experiments to detect OH, and they had a lot of wave guide stuff.

They didn't have anything for what I was going to do at Bell, and they also had this very strange regulation that you couldn't buy any new capital equipment unless you could junk some old equipment. And of course being new, I didn't have any old equipment to junk. I had to hope somebody else in the department did. The excuse for that was Bell Labs was set up as a nonprofit corporation, and if they increased their capital by acquiring more equipment, then that was the equivalent to making a profit.

Now after I'd been there about five years--well, to show how bad it was, you couldn't even buy an oscilloscope. You

could buy a thousand dollars worth of platinum and throw it away tomorrow, but not a three hundred dollar oscilloscope. I felt quite frustrated by that. After I'd been there about five years they suddenly realized that they could buy new equipment if they say it's not for general use but for a particular experiment. All of a sudden the purse opened and you saw big Varian magnets sprouting all over the place and all sorts of big equipment. I saw that my productivity shot up and so did everybody else's. Management realized that. We hadn't realized how much time we were spending working around the limitations of equipment.

Riess: That's very interesting.

You've mentioned Schulz and we've talked about him before, but this reference to his memo in August 1945 about induced resonance, is that like he was having an idea of a maser?

Schawlow: No, he didn't have any idea of a maser. It was not induced resonance. I think it was that you could control chemical reactions somehow by using some radiation longer than visible radiation. Photochemistry is well known, it goes on every day in camera film, when light falls a chemical reaction takes place. And there lots of other reactions--bleaching, for instance--.

But his idea was that you might be able to control chemical reactions if you had some radiation in between microwaves and visible, so that was why he supported the Columbia Radiation Lab. He knew they had the magnetron work, so they were producing shorter wavelengths, and he knew that Charles Townes was working on interaction between microwaves and molecules. But he didn't have any ideas as to how to go about that. As a matter of fact, the way things have worked out, the possibility of chemical reactions was in our minds when we were working on the idea of a laser. Although it wasn't really an important motivation, it was something we hadn't forgotten.

Infrared has not been useful for chemical reactions as far as I can find out. You irradiate them but then they tend to quench too rapidly, they don't hold the excitation long enough to undergo a reaction. Also, the radiation is masked by the thermal radiation which is present all the time and is in the infrared even at room temperature. So that hasn't really worked out.

Riess: Charlie is still interested in the infrared.

Schawlow: Oh, yes. He's interested in the infrared for astronomy, but I don't think he's ever done anything on photochemistry, not as

far as I know. We did for a while, and we'll come to that, at Stanford, but we used visible light and it wasn't very successful. There are a lot of things I didn't know.

Nepotism Issues Motivates a Job Search

Schawlow: Columbia had very strong anti-nepotism rules at that time. And if I was going to marry Charlie's sister, then that would be nepotism. There was never any possibility of staying on at Columbia in a permanent way at all, or even probably not at all. The second year, 1950-51--the Carbide and Carbon fellowship was only good for one year, and Ned Nethercot came in from Michigan and took over that.

Charlie wanted me to stay on to help him write this book on microwave spectroscopy and he found some money from the Ernest Kempton Adams fund at Columbia University to pay my salary, same as I'd been getting on the fellowship, I think. But it meant that it was a busy time. I was still trying to get somewhere with this research, and he did have me work with other students so I would have a variety of experiences and get some publications.

It was a pleasant time. As I think I said last time, I had started looking around, thinking that I might like to find a wife. And then, of course, Aurelia came around and I was in the mood. Well, she was very attractive. She was pretty, but the main thing about it was she was intelligent and, well, the kind of person I'd like to live with. That was really the most important thing for me. I never had bothered with girls at all before that. I just felt I couldn't afford to, either the money or the time. As I say, I did look around a bit, but I didn't see anybody I really wanted before that.

So we began seeing each other in the fall of 1950, and I think in January we became engaged and married in May.

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Schawlow: It was a busy year. I did get a letter from Professor Henry Ireton, who was sort of the administrative director of the physics department at Toronto, asking if I'd be interested--no, I'd gotten that before that year started--whether I'd be interested in going back there as an assistant professor, but I'd already promised Charlie that I would stay so I didn't get that job.

Then when I started looking for jobs in the spring, there weren't many. Whereas in '49 there'd been a great scarcity of people, in '51 there didn't seem to be academic jobs. There was the complication that Aurelia had come to New York to study singing with a teacher, Yves Tinayre, and she didn't want to leave the New York area, so that limited things. I think I wrote to a couple of universities but I didn't find anything.

Then Bell Labs had a very good, efficient recruiting scheme. Sidney Millman, who had been one of Rabi's students, I think, was then department head at Bell Labs, and was recruiting. He'd come and talk with the professors, ask who might be a good prospect. Somehow he suggested me.

I still wanted to do some real physics, so I was really rather afraid that I'd just get into some kind of routine engineering development. But what they offered me was to work with John Bardeen, to do experiments. Bardeen had already invented the transistor, or had been co-inventor, and had published a lot of theoretical papers. He was a theorist and he was beginning to get interested in superconductivity, so they wanted somebody to do experiments on it, and they hired me for that, even though I had no background in low temperatures or solid state physics at all.

Now maybe that's what scared him away, but by the time I arrived in September, he had gone, decided to go to the University of Illinois. So there I was. And they didn't tell me not to work on superconductivity, but I had to learn about it and try and find something to do--which was rather difficult.

Riess: You were married in May?

Schawlow: Yes, that's right. May 19th.

Riess: And you were down in Washington with Charlie at the same time?

Schawlow: Yes, earlier, the end of April. So it was just a couple of weeks before I was to get married. Two or three weeks. Maybe my memory isn't so good at those things. I honestly do not remember the incident that he's so fond of telling about how we shared a room. [laughter] It's plausible, because indeed I had been used to sleeping late, working this noon to midnight shift. So it's possible that he woke up early and went outside.

I don't remember sharing a room with him, but I can't really deny it--but I just can't confirm it either. I never heard about that until 1959, just before the first Quantum

Electronics Conference. He held a party in New York for the various people coming to that conference including the two Russians, Basov and Prokorov. That's when I first heard that story about the park bench.

Riess: But you did witness his notes?

Schawlow: Yes, I did witness his notes. I'd forgotten that too, so I'm a real forgetter. [laughter] I remembered him telling me about the idea. What I didn't remember particularly was signing his notebook.

Riess: Do you remember the electricity in the air?

Schawlow: It was an interesting idea, I thought, yes. You couldn't be sure it would work, or how difficult it would be to make it work, or how useful it would be. But if I hadn't been going to Bell Labs, I would've liked to work on that. That would have been a good project.

Riess: Well that's really what I was thinking about. I never have heard of nepotism that has to do with the wife being--

Schawlow: --the relative. Well, Charlie is a very upright person, very correct, and I think he pointed that out to me.

At the time I didn't feel I was good enough for Columbia, to tell the truth. I didn't have as strong a theoretical grounding as I should have had, so I wasn't too concerned about that. I think now that I probably could have stayed on there, otherwise.

When I was talking about staying the second year, Polykarp Kusch was the chairman, and he came around and asked me if I'd like to be an assistant professor. That would be in 1950, before the 1950-51 academic year started. And I had to say that I really couldn't see how I could manage to do research and write on the book and still do some teaching. Well, he did offer me that, if I wanted it, and he might have offered me something in 1951 too, but--.

More on Writing the Microwave Book

Riess: We will get you to Bell Labs, but this business of writing that book--.

Schawlow: Ooh, horrible job.

Riess: You tell me why it's a horrible job.

Schawlow: To begin with, I didn't know anything much about microwave spectroscopy. I'd only worked on it really for a year. Well, we divided up different chapters, and I had to draft some of the chapters and Charlie drafted the others. All the more complicated things he did, and the things I did he revised pretty heavily. But it was a lot of learning. I had to read a lot of papers and try and boil it down into understandable prose. We were writing about basic principles, and also reporting on what had been done.

I felt when we started on it that if I was going to do it at all, I really want it to be a classic. And I think it is. It's still in print and has been referred to many, many times. So I was willing to dig fairly deeply in the stuff. There was a review of the basic theory: you had to do molecular theory and microwave theory, and I think we even had a chapter on atomic physics to bring the thing up to the start. Then a lot of the details--Charlie did a lot of detailed stuff on the interaction of microwaves with molecules, and that can get very complicated, particularly if you get asymmetric top molecules that have little symmetry.

Of course, my attitude about molecular spectroscopy up until then was given by a definition that I've repeated many times: that a diatomic molecule is one with one atom too many. [laughs] It can get very complicated, and here they were dealing with atoms that actually had even more than two atoms. So I had to read a lot of theory and try and put it in understandable form.

After I think about eleven chapters or so on the spectra--and they would be illustrated by reports of experiments that had measured certain things there--. The bibliography had a thousand and one references. Actually, it was a little over that, but we put in some "a's" and "b's" so we'd come out a thousand and one.

Then after that there were several chapters on microwave techniques and one on millimeter waves--I remember I drafted that one. Of course, millimeter waves were not very advanced at that time. They were beginning to be used, but I remember I started the thing off saying something about their difficulties but techniques nevertheless are now available. And there was a misprint in the book, and it came out that "techniques are not available." The editor from the publisher said they expect about one error per page on a technical book like that, and we had the manuscript read by several graduate students and still there was about one error per page.

Riess: Maybe they were just predisposed to thinking they were not available.

Schawlow: Well, anyway, that's the kind of thing where Spellchecker won't help.

I once put a speech synthesizer in my computer that would read text that you have in there. And I caught a mistake there. I think I had and "of" instead of an "or." I haven't done it since then, but it seemed like a useful idea.

Riess: Did you have any trouble with the writing? Do you struggle to write in general?

Schawlow: Yes, I do struggle. I used to think I could write pretty fast, but I couldn't. And when I do a draft or something I tend to cross about every second or third word and keep struggling with it. Usually, though, when I'm through the first draft, I'm through, because I've done my revisions as I go along. I guess now I'm probably doing a little more revising, but generally the first draft is a struggle.

Riess: For the spectroscopy book, did you write it by hand?

Schawlow: Yes. It was all by hand. I did have a typewriter at the time, but I think Charlie's secretary did the typing, I don't remember typing any of that. Of course, there were a lot of mathematical symbols.

I remember one thing is that when I was a student the people teaching electricity and magnetism used the centimeter, gram, second system of units--cgs, those were the basic units. But the engineers foisted on us the mks system, which is meter, kilogram, second. Let's see, now, we wrote the chapter on microwave techniques first I think in mks. Then we decided that physicists were more comfortable with cgs. And then we were persuaded to go back again and translate it back, which was a certain amount of bother. [laughs] I think we ended up with the mks system. The engineers were the ones who pushed the mks, but the physicists had given up on that--mostly, not entirely.

Riess: Who were you writing the book for?

Schawlow: McGraw Hill.

Riess: For the engineers or the physicists?

Schawlow: Well, physicists and chemists mostly. The mks system had been accepted by national standards bodies, and we were told it was

supposed to be the system of the future. We wanted to be understandable, so that's the way we ended up.

Riess: Well, all things considered, 1951 was a big year. You decided to stay and work on the book, not to go back to Toronto, for instance. And you are very identified with the book?

Schawlow: Well, clearly Charlie is the senior author on that, and he's gotten at least one award for it, but yes, it helped me a lot, helped my reputation quite a bit, because it is a formidable book even though I didn't write it all. But for me personally it was a waste of time, because I haven't worked on microwave spectroscopy since then. I practically never looked at the thing again but it was good advertising.

Riess: You were working on that when you were at Bell Labs.

Schawlow: Yes, it went on and on. I would go in practically every Saturday to Charlie's office to work on it. I guess I worked on it some at night too. I finished it I think in '54. It takes some months to actually get it into print, but it came out around June of '55, May or June.

Riess: Not an ideal way to start out being married. That's living much more like a graduate student.

Schawlow: Yes, it wasn't an ideal way to start a career at Bell Labs either. It was distracting. I could've done better. You know, we managed. Aurelia was understanding. I didn't do much writing at night, actually. It was mostly just Saturdays I would spend on it.

Oh, we had tickets for chamber music concerts in New York, and quite a few Saturdays I would go into New York in the morning, and come back home at around noon or so, and then go into New York for the concert in the evening. I remember being rather sleepy during chamber music concerts.

Riess: Did you pursue your interest in jazz or did marriage end that?

Schawlow: Well, kind of dampened it a bit. I didn't really pursue it seriously at that time. There was a hiatus for a few years. I picked it up again when we came west and I had more room to store records.

III BELL LABS YEARS

Experiments on Superconducting Phenomena

Riess: Let's talk about your work in superconductivity at Bell Labs.

Schawlow: Well, I did some cute things. They didn't solve the problem of superconductivity. Bardeen, Cooper, and Schrieffer did.

Riess: Were you the point man on superconductivity at Bell Labs?

Schawlow: Superconducting phenomena. I should explain that Bell Labs seemed to want to cover a lot of fields. They said that the purpose of research there was so that they would be informed about the latest developments that might affect their technology, and they felt the best way to do that was to have people working in the different fields. It wouldn't do any good to have somebody sitting in the library and reading stuff because that would be a year old. But they wanted people to go to meetings and discuss on an equal level with others in the field so that they would really know what was going on in the frontier.

I was the only one working on superconducting phenomena. Bernd Matthias was working on superconductive materials. He was sort of an alchemist. He mixed together all sorts of things, worked very intuitively, and he did find some new superconducting materials, and ferro electric materials too.

Hal Lewis did some work on the theory of superconducting phenomena. I had the feeling about superconductivity that it was such a perfect thing that it was awfully hard to get a handle on it. I mean, the electrical resistance is really zero, not just nearly zero. Magnetic fields just don't penetrate. Well, we did do experiments where it had been found that magnetic fields do penetrate a small distance sometimes,

something like a few hundred to a few thousand angstrom units, an angstrom being 10^{-8} centimeters. Oh, I'm showing my age: it's 10^{-10} meters. In fact, you're not supposed to use those units any more. You're supposed to use nanometers, a nanometer being ten angstrom.

Anyway, that's a small distance. But one of the experiments I did was to measure the penetration depth by winding a coil very closely around a rod of superconducting tin. And you can get very pure tin, it was surprising. You could buy it from a company called Vulcan Detinning. It turns out that people do recover the tin from tin cans. And there's not much on a tin can, so they have to have extremely selective processes which take the tin and nothing else. So you can get tin that's extremely pure, and so we got some and made up a rod of it. We wrapped a wire coil very closely around it. In fact we wrapped it around the inside of a glass tube. I used to kid people, say that I did it by a simple application of centrifugal force, but in fact we had it wound first.

Bell Labs facilities were very useful there. We got niobium wire enameled, coated with Formvar, which is something you couldn't buy on the market but they prepared it for us. Then they wound it on a mandrel, on a rod, and it was just the right diameter. Then it would be cemented together with this Formvar. Or rather, they'd put some kind of cement on it and slip it inside a glass tube and then remove the mandrel. Then you put the tin rod in place of where the mandrel had been. It was a close fit.

What we did was to measure the inductance of a coil which depended on the amount of magnetic flux inside it. Well, magnetic flux depends on the magnetic field and on how much space there is. And the volume was small, just the space between the rod and the coil, and whatever penetration distance there was. So what we could do was measure the change in the penetration depth as it cooled it through the superconducting transition. We did that by making this coil part of a radio frequency oscillator, and then the frequency of the oscillator would change, and we had a crude frequency counter which were just becoming available, and we could measure the change in frequency.

It turned out that--. This was not the first thing I did, it was more like 1957 I think. But we found a departure from the predicted dependence on temperature. The penetration depth fell out more rapidly as we cooled it. These were small effects, but we were able to measure that the penetration depth changed more rapidly than the simple theories had predicted. And it turned out that this was also a prediction of the

Bardeen, Cooper, Schrieffer theory which came out about the same time. Bardeen asked me to present these results at a conference that he was organizing. And so that was useful. It helped to confirm the BCS theory, but it didn't really open any doors.

We did another experiment earlier where we used penetration through a thin film, and there I really missed something. What we did was have a thin film coated on the inside of a glass tube, and then put a tiny little coil inside it to pick up what signal could get through--we'd have another coil outside with an audio frequency signal and pick up the signal and measure how much it was.

We got some results which were reasonably consistent with other measurements of penetration depth, but we noticed that there were spikes sometimes coming through suddenly. Hal Lewis suggested that maybe it was flux quantization, which would have been a great thing to discover. But we didn't take it seriously. We thought it was associated with defects in the film because you got it some places, not others. But that's exactly what we should have expected would permit flux quanta to penetrate through. So we could have discovered quantization of magnetic flux in superconductors, but missed it.

Riess: How big a deal would that have been? Is that something somebody discovered later and got a big Nobel Prize for?

Schawlow: Well, unfortunately not a Nobel Prize. My colleague at Stanford, William Fairbank, discovered it about the same time that I came, in a different way. He should have had a Nobel Prize for it, but it was discovered independently in Germany by R. Doll and M. Näbauer, and then Näbauer died a few months later. So I think maybe that's why they didn't give a Nobel Prize for that. I think he should have had it, I nominated him for it.

The reason why it was interesting was because the flux quantum was only half of the value that people had predicted. The value was hc/2e. That is, it depended on two electron charges--h, Plancks constant, divided by 2 times the electron charge. Now the reason that is important is because the Bardeen, Cooper, Schrieffer theory depended on pairing of electrons. So pairs of electrons get coupled together and provide the superconducting current.

That could have been an important thing. Now, wait a minute, the BCS theory was already there, not from when we did our experiments but when Fairbank and Deaver--who was a student, Bascom Deaver--discovered the flux quantization.

Still the fact that it was hc/2e was a useful new piece of information. It surprised a lot of people.

Riess: When you were working on this, who would you report to? How was it set up at Bell Labs?

Schawlow: There was a department head, a man named Stanley Morgan. He was a chemist. He was a good administrator. He had been joint head of the solid state physics group with Bill [William] Shockley, but Shockley had been an impossible person to get along with. They had split the group, and Shockley had a group on transistors, and the rest of solid state physics was under Morgan. Although he was a Ph.D. physical chemist, he didn't really try to tell us what to do.

I remember that one of the technicians there, quite a good man named Ernie Corenzwit, went to Stan Morgan one day and asked him when should he ask about what to do. And he said Morgan told him, "If you know what to do, do it. If you don't, ask." I thought that was a good philosophy. In fact, when we came to Bell Labs they had indoctrination sessions. One of the men there told us, "The first thing you've got to learn is there are no oracles. You'll have to think things out for yourself." So they didn't interfere much with that.

Earlier I had done some nice work on the interface between superconducting and normal regions. Although it was a cute idea, I don't think it was really important. When a magnetic field penetrates into a slab of superconductor, if the field is perpendicular to the slab, then it comes in in regions. And some regions are normal where the magnetic field has penetrated and destroyed the superconductivity. And the regions in between them remain superconducting. This is known as the intermediate state. There's been a lot of speculation about that and you could measure the surface energy of these boundaries.

Well, what we did was to sprinkle niobium powder on the tin plate and photograph it. Niobium powder is superconducting and it's pushed out of magnetic fields, unlike iron filings which would be pulled into a magnetic field. But the niobium powder is pushed out, so you saw a nice pattern of the lines of spaces on the thing.

And we got several papers on that. We did it first, I think, with polycrystals and then we did it with single crystal. Then we were very surprised because the magnetic field penetrated faster in the direction that we thought was the wrong one because the higher the conductivity, the more

dampening it would be, it would slow down the motion. But it came in the direction where the conductivity would be higher.

This worried us a great deal. In fact, Bob Schrieffer was a graduate student, one of the people who did this, later got a Nobel Prize for the theory of superconductivity. He spent the summer there. And Hendrik Gorter from Holland was there, and nobody could explain it. But then I did a measurement of the magneto-resistance of this very pure tin and I found out that by the time you reach the field of a couple hundred gauss, which breaks down the superconductivity, the magnetoresistance has crossed over, and the direction which was high before became low.

Your picture it like this: look at my hand here. If I put a magnetic field perpendicular to that, then the field would penetrate in there and I would see these lines. If you have a square plate, you couldn't tell which way it was going to come, it would come in from all the edges, but it comes faster in the direction where the resistance across here is higher because that doesn't dampen as much.

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Schawlow: As I say, then we did this experiment to measure the magnetoresistance, which is hard to because these things are extremely good conductors even in the normal region--this was very pure tin, I think it was something like a hundred thousand times lower resistance than at room temperature. So again, I did an experiment where I wrapped a coil around the thing and measure the inductance, and that gave me the resistance. I'm pretty good at putting coils around things. [laughter]

Riess: You talked about Bernd Matthias and other people. Was this a team that was working together and constantly talking?

Schawlow: No, no, absolutely not. Matthias was strictly--he was a very intense sort of person, and he was extremely original. In Bell Labs I gather he used to wear no socks a lot of the time, was considered one of the wild men around there. When he would rehearse a talk--. They would get you to rehearse your talks at Bell Labs, which was very good thing. It's one reason Bell Labs people had a reputation for giving good talks. But he would be outrageous, he would say horrible things, and then when the actual date came he would give a good talk. But he just liked to put people on.

For a time we shared a laboratory with him. Now he wouldn't do anything as far as the experimental equipment was concerned. He had collaborated with John Hulm, who was at

Westinghouse, and Hulm had given him a design for a cryostat where he could test his samples. All he would do was--he might have Corenzwit make up some samples, using electric arc melting usually. Then he would bring them in and lower them one by one into this cryostat, and check whether there was superconducting or not. That was in the same room that I had my apparatus, but it was a simple rule that when he was there, I wasn't.

Research, Resources

Riess: Bell Labs didn't work in teams?

Schawlow: They didn't work in teams, not in the basic research.

The one thing I didn't realize for a long time--. I really didn't make use of the resources. The way things happened at Bell Labs was Mr. A gets an idea; he goes to Mr. B who makes a sample for him; and takes it to Mr. C and D who make measurements; and then to Theorist E; and they come out with a paper with five names on it. And they say, "Oh, Bell Labs has put a big team on this," whereas generally by that time, they're not even speaking to each other.

I didn't realize that there were a lot of lonely people, like myself, sitting around, and they have equipment for something or other. If somebody can think of what could be done on that equipment, they can drop what they're doing and help you. It was very good. Both the experimentalists and the theorists too would help each other when you asked them. But I was too shy to ask them most of the time. The last few years I did begin to work with others.

Riess: How did you get from one problem to the next? How did you know that something was done?

Schawlow: You read. To begin, there's a book by David Shoenberg, from Cambridge, on superconductivity. I read through that and looked for ideas, and that's where I got the idea of measuring penetration depths. Then, well, I talked a little bit with Lewis, even Matthias very occasionally would talk. In fact he had the idea of looking at the intermediate state by sprinkling iron powder on the thing, and the iron would be pulled into the regions where the magnetic field had penetrated. I persuaded him that it was better to use the superconducting powder that would stay away from the regions where there was a magnetic field. Now I'm not so sure that really was better, but it worked.

Riess: I'll give you a break and read some of the Bell Labs philosophy on doing basic research: "...[research] deals constantly with uncertainty, except that there is ever present the certainty that important new things remain to be discovered..."[1]

Schawlow: And there were not very many people in research, maybe a couple hundred people in the whole place out of about seven thousand altogether.

Riess: "...[research] must assure the flow of invention and new science that will enable future technologies to be developed. And it must see the ways this invention and new science can be exploited by Bell System."

Schawlow: Yes, well, maybe not right away. A lot of their research was more closely tuned to communication needs.

I used to worry sometimes because I couldn't see why what I was doing was going to help the Bell System at all. But there were people doing communication research. Oh, for example, the satellite communication, they pioneered that, first with reflecting balloon satellites. Oh, they developed the travelling wave tube there and things like that, which were research but they were more directed toward the needs. But ours was just basic physics, not all kinds of physics, but physics of materials and things that were related to the kinds of things they did.

But the inventions that came out of our department, I think--there were few, and they didn't expect many from the basic research. And they were very expensive. Things like transistors and lasers took millions of dollars to develop because they were so far out before they could get any use from them.

Riess: Where else was similar research going on?

Schawlow: There was work at Columbia, and at Rutgers too. I guess we knew what they were doing, what they published, but it wasn't very close to what we were doing. About the only people doing things close to what I was doing were a couple guys in England and in Russia. Maybe I hadn't chosen very well, but it felt rather isolated.

We did do one useful thing for Bell Labs which maybe paid for our salaries during that time. I think it was Dudley Buck

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[1] xv, *A History of Engineering and Science in the Bell System: Physical Sciences (1925-1980)*, AT&T Bell Laboratories, 1983.

at MIT who invented a superconducting switch that you could make switching systems or--called a cryotron. You could in principle make computers or even telephone switching networks from that.

So they had a meeting. IBM put a lot of people on it, I don't know, maybe a dozen, or twenty. They tended to throw a lot of people at problems. They did that on ferro-electric memories before that, and then they sort of worked for a couple of years and gave it up. I'm told that Watson said, "There's so much money to be made in computers that we can't afford to overlook anything." And that was true in those days.

So they had a meeting to ask should we at Bell Labs get into cryotrons. Lewis and I went there and pretty easily persuaded them that they shouldn't. And indeed nothing did come of it at that time. There were two reasons: one is that it had to be in liquid helium and, well, it was primitive and not very fast either at that time; the other thing is that while you might use it for computer calculations, it wasn't suitable for the telephone system where you're switching. You need a lot of input and output, so you had to have a lot of wires coming in and out of the low temperature region which is very hard to do, because they conduct heat. So anyway, we did tell that it wasn't worth getting into and I think saved them a lot of money.

Riess: Just because one part of the word is the same, superconductors and semiconductors are not.

Schawlow: They're worlds apart. They're both solids, but--. And I didn't work on semiconductors.

Shockley would have liked me to work for him I think, but Charlie had warned me and so I didn't go to work for him. Charlie said, "He's nice, but if he thinks you're a rival, he can be pretty hard." I don't know whether you know his history here in California but he started the Shockley Semiconductor Lab which was financed by Beckman. And he hired some very good people, but then they went off and started other companies: first the Fairchild Semiconductor Company and then a lot of others, National Semiconductor and maybe Intel. He was so hard to get along with--he knew good people, he had high standards--

Riess: He inspired an industry.

Schawlow: Oh yes, he did, he was very important.

Riess: Inspired it by people wanting to get as far as possible from him.

There's a whole list of things that were developed while you were at Bell Labs. High temperature superconductivity was one of the things, though somewhat earlier, between 1951 and 1955.

Schawlow: Oh, goodness. Well, high temperature is relative. Matthias and [T. H.] Geballe did work on some materials and I think they had for a while the highest temperature superconducting alloy. I think it was niobium germanium. It had transition temperature of around twenty degrees Kelvin, so you could actually run it in liquid hydrogen rather than liquid helium. But that's nothing like the high temperature superconductors that were discovered in the 1980s. They go up in temperatures over a hundred degrees Kelvin. They can be run in liquid air which makes a big difference.

Let me be fair with them there. This material--was it niobium germanium or niobium tin? one of these fairly high temperature superconductors--could also resist magnetic fields better than others, so that you could wind a magnet from it. Now if you wind a magnet coil from superconducting wire and put a current through it it produces a magnetic field, but when that's strong enough, it destroys the superconductivity. These could resist that, so these wires are still used for superconducting magnets which are used quite widely in magnetic resonance imaging devices.

So the high temperature wires, they were important--even though I wouldn't call it as high a temperature now. For those days it was high, and those are still the best for the magnets.

Murray Hill and the Work Day

Riess: Let's just get you situated a bit now. Murray Hill is where you were?

Schawlow: Yes. It was a little town, almost nothing there except this huge Bell Labs laboratory at that time. I think now it's been built up quite a bit around there. It was out in the country pretty much, west of Summit, New Jersey. The nearest town was New Providence. There was a Murray Hill post office, I think, which was the largest second-class post office in the country or something like that because of all the Bell Labs system.

Riess: I want to make sure that we really get an idea what is was like to work for the Bell Labs, what the virtues and the drawbacks were, and how you could ever be induced to leave.

Schawlow: It wasn't hard.

Riess: Charles Townes spent time trying to get them to do things that they were so slow to think about doing.

Schawlow: I was shyer, probably. I didn't really particularly try to get them to do things. I could have and should have. Like, for instance, when we had any ideas for lasers, I should have tried to get them to give me some people to help me try and build one. They didn't have anybody, and that's the way it was, so I didn't try to build one. I just sort of assumed it wasn't possible.

I worked conscientiously, but I didn't work much at nights, only very rarely went in at night or on weekends. I spent a lot of time with my wife and then family. It was sort of like a job. I mean, it wasn't as consuming as it has been at the university. Of course, the university, you have teaching and administration, all added on.

Riess: It sounds like they set it up to make it just like a job, if you've got to be in the parking lot at eight--

Schawlow: Eight-fifteen, yes, at the beginning. Yes, I think so. It was an industrial company, really. They changed that. About the last couple of years I was there, maybe the last three years or so, they decided they were going to make more spread in the salaries and they would have formal evaluations of people. They would divide them into octiles, the best eighth and so on. Well I don't know how my rating was, but I don't think it was very high because I was working alone on superconductivity, and no great invention had come out of that.

At one point Hal Lewis and I asked the boss if we should write down some ideas. We could think of inventions, like switches and so on. He said, "Well, does it have to work in liquid helium?" I said, "Yes, I guess so." And he said, "Well then don't bother." So we didn't bother.

As I say, I think they really didn't think very highly of me because they made me the department safety representative, and that's usually a kind of drudgery job that they give to somebody who isn't doing anything else much. About the only thing I did was that I had to write an accident report when one of the theoretical physicists stabbed himself with a pencil--a sharp pencil. I pointed out that theorists should be instructed on the uses of pencils [laughter].

Riess: Another thing you did was teach while you were there. You taught a class on solid state physics.

Schawlow: Yes, that's sort of ridiculous, but they asked me to do it and I did it. I learned solid state physics as I went along. It was kind of fun, but it was work. I had to go to New York for those [lectures], three days a week I think it was.

Riess: You mean you were teaching in New York?

Schawlow: The new engineers who were coming in. They still had a big laboratory in New York. That was their headquarters for a long time. People would come from Murray Hill, maybe even from Holmdel, which was mostly military engineering. I don't know, we didn't get to know who the students were very well.

Riess: You also built an audio frequency parametric amplifier.

Schawlow: Oh yes, just for the heck of it.

Riess: How did that fit in?

Schawlow: Well, after the maser came along, some people realized that--I think it was Harry Suhl who realized you could make what we now call a parametric amplifier. (I think Rudy Kompfner gave it the name.) It's one where you change the parameters of a circuit, namely the inductance or capacity. If you do that at twice the resonant frequency by the circuit, then you can make it oscillate and you can make it amplify.

I tried to learn, get my thoughts straightened out, you know, how did this compare with masers, which I wasn't working on, but I was interested in them. It was rather simple: you could find in the stockroom toroidal coils, that is, with a doughnut-like iron core. You'd find that in the stockroom and then you put that in the circuit board with a capacitor, and you'd get a resonant circuit. Then I would change the inductance of that coil by wrapping another coil around it--these are very high permeability cores, and because of that they're easily saturated, you could saturate them on every half cycle, or so, and so you could change their inductance. We did that.

It was just kind of fun to make that. I think I wrote an internal report on it, but I didn't publish anything on it. An interesting sidelight is that Suhl had invented this parametric amplifier that used a microwave ferrite. He didn't build it; he was a theorist, a very formal theorist, but he could invent things with formal mathematics.

Then he realized the generality of this concept, and they made an application for a patent in his name, but the patent

office came back and said, "You can't have that patent because Bell Labs already has a patent"--I think it was Jacobsen, I'm not sure, issued in the 1930s. It had just been forgotten. I think they didn't realize that the parametric amplifiers were low noise amplifiers and didn't realize the importance of that.

This man was still around, this guy whatever his name was, though he was in a different department. We never met him.

Riess: Then in 1957 you and Charles Townes got back together again and start doing things. That sounds like the place where we should begin next time.

Madison, and Home Life

Riess: Before we finish today, would you describe life during the Bell Labs period? You and Aurelia lived in Madison?

Schawlow: We lived for the first five years of our marriage in Morristown, New Jersey, and then we bought a lot in Madison and had a house built there in 1956. We were very lucky in a way: there was a section of Madison, a very nice section adjacent to Drew University, which had been partly developed in the 1930s and then people ran out of money. So some lots were left in among the houses. We were able to get one from an old couple who had finally decided they were never going to be able to build there. It was covered with dogwood trees, lovely, a very pretty area--Woodcliff Drive in Madison.

Riess: And you got an architect?

Schawlow: Well, we got a set of plans from a magazine, you know these housing magazines sell plans. Then we hired an architect to modify it for the particular lot, adapt it to the lot. Then we had to get bids and they were all high. But then this black man came along, Reverend Sanders I think. Anyway, he was a minister part-time and builder. He hadn't built a house, actually, but he was a carpenter. He didn't do a bad job on the thing.

We said we wanted to be able to put in air conditioning later on, and the architect hired a heating consultant to design the ducts for that. The builder got a heating and air conditioning man who took one look at those plans and said, "Those ducts won't go in those beams. They're too big." So he said, "Leave it to me, I'll do it right." So it was sort of architect-designed. It was a nice house, we liked it. We made

one mistake. We didn't bother to have a garage. We didn't really need it, but I think when we were selling it it probably was a defect.

When we came to sell it in 1961--we were moving away--we had quite a hard time. We tried to sell it ourselves but we didn't succeed. Finally got a real estate agent who sold it shortly after we left. But one of the big problems was that at that time they were talking of building a third New York airport in the so-called Great Swamp, which is near there and we would have been right on the flight path. So that depressed housing values at that point.

Riess: What did you like to do? Did you do outdoorsy things at all?

Schawlow: Not very much. We liked to go to concerts and shows, things like that.

It was rather unfortunate, in a way, that Aurelia had got this job at the Baptist church in Morristown as choir director and organist. A wonderful man was the minister, Mr. Barbour. She had written to several churches, and one day when she was feeling particularly depressed he showed up at our apartment and offered her the job, and that really made things look up.

She was a very good choir director. She had directed a choir, choruses. She'd taught music at a college in Georgia, Piedmont College, even put on a concert with Percy Grainger, the composer. He came there and they played his music--I guess he played some too. But what was unfortunate was that she didn't know how to play the organ, although she played piano, so she had to take organ lessons, and for the first few months they had a substitute organist.

She had a good choir there. But that meant every Sunday we had to be at home, we couldn't go away for weekends, which was a limitation. And I had to work during the week. When we had vacations, we'd usually drive up to Toronto or down to South Carolina to visit folks there.

I guess our common interests were mainly cultural, and also in the church, too. We got active in the church. I was on the board of trustees and even on the deacons--which was clearly ridiculous. It was a very liberal church, and although it was a Baptist church, you didn't have to be baptized. And I hadn't been, and as an adult I didn't feel like doing it. But they still wanted me on it.

Riess: That's why you're saying it was clearly ridiculous.

Schawlow: Yes. And there were young people's groups. We got some friends there. It was a nice time and nice people.

Riess: Did you have your children by then?

Schawlow: We had trouble having children. Aurelia had to have--what is that test where they put carbon dioxide into the fallopian tubes? Apparently pretty painful. But after that we had children. Our first year or two, we thought better not to--the advice you usually get is don't have them too soon. But then afterwards we were trying and not getting anywhere. So finally we took that test and then Artie was born in 1956, Helen in '57, and then Edith in '59. They were all born during those years.

Riess: But for the first year she was in the apartment and depressed and happy to be offered the job. Why depressed?

Schawlow: That was just shortly after we came to Morristown. We went to a garden apartment complex there and they had thin walls. She wanted to practice, and there was a woman I think downstairs who absolutely would not allow her to practice anytime. Aurelia tried to arrange a time when she could do it and just wouldn't. We fortunately found another place which was the second floor of a house on the other side of Morristown with a nice old lady, a retired kindergarten teacher who was slightly deaf and didn't care how much music we made as long as we didn't do a lot of drinking--which we didn't do.

I guess working on her career was more--and singing. She was still taking singing lessons until after Artie was born, I know for a while after that, and was going into New York to work with an accompanist. But that's a very tough business to try. She had a beautiful mezzo soprano voice--I have some recordings of her--but she never got any opportunities really to be a singer. It's just a tough business. William Warfield was also studying with Yves Tinayre at that time, and he's had a successful career.

Stan Morgan and the Solid State Group

[Interview 4: September 12, 1996] ##

Schawlow: When I went to Bell Labs I went into the so-called solid state physics group, which was headed by Stan Morgan, who had been a physical chemist. He was a very nice person, very easygoing, quiet sort of person, but very capable. He didn't tell us what

to do, which was difficult of course. I didn't know what we were supposed to do, particularly since I was working on superconductivity and had to find out something to do. The group was a remarkable group and I used to wonder at that time how many of them would be famous ten or twenty years from then. Really, all who stayed active in physics did achieve big reputations.

They included Walter Brattain, who had already been co-inventor of the transistor and did get a Nobel Prize soon after. I remember the day when he got the prize the telephone company very quickly managed to intercept his calls--somebody would answer them for him. But he did come to our afternoon tea, which was held every day. I remember him looking at the newspaper and commenting adversely on some of the things it was saying.

One of the things I remember about Brattain is really worth mentioning. He told us his father was a prospector and was working up in the mountains still--he must have been fairly old by that time. But he was several miles from the nearest store of any kind, and they [the store] had a telephone, and the only way to reach him was to send something to him care of this place. Well, Walter somehow didn't want to make it too public, so he sent a telegram saying: "Transistor men win Nobel Prize." When it reached his father it said: "Your sister won the Nobel Prize." [laughter]

That group included Phil Anderson, a theorist who later got a Nobel Prize; Conyers Herring and Gregory Wannier, both very distinguished theorists. And they were people who were willing to talk to you if you had any questions. Conyers has been at Stanford for some years since he retired from Bell Labs, still active. A very encyclopedic theorist, he knows everything, practically, and has made many important advances.

Also Bernd Matthias, who was kind of an alchemist, I think. He kept inventing new compounds for superconductivity or ferroelectrics. There were a few others who dropped out. John Galt, who had done distinguished work, went into management and later was one of the top people at the Sandia Corporation which was then being managed by Bell Labs.

Riess: Were you all more or less the same age?

Schawlow: No. Well, I don't think there was anybody much over forty. Well, Brattain was. They were all fairly young. Some of the theorists I think were older--Wannier and Herring. Anderson was young. He's younger than I am.

Riess: You said Wannier?

Schawlow: Yes. He later became a professor at the University of Oregon. [laughs] He was Swiss, and they begged him to come back to the University of Geneva, which he did for a year, but then he came back. He said he couldn't stand the food, it was too rich.

Riess: It sounds like one of the real pluses of working at Bell Labs is that notion of a group.

Schawlow: But they didn't work on the same problems. You could discuss anything and--

Riess: What defined a group then?

Schawlow: Well, they were in the same department. Yes.

There were personal matters involved. For instance, Brattain had worked on semiconductors and co-invented the transistor but he couldn't stand Shockley, and that's why he was with Morgan rather than Shockley. There are many people who couldn't stand Shockley I think, though Shockley was brilliant.

Morgan was the head for about five years or so. He then became head of the chemistry department, which was another step up. They changed the title. Ours used to be known as a subdepartment, and then there was the department, the physics department which was headed by [S.] Millman, who was also another easygoing guy but very capable. He had been one of Rabi's group at Columbia before, and he was the one who recruited me for Bell Labs. Then there was the general department which was under Addison White.

Later they inflated the titles so that the subdepartments became departments and the departments became laboratories, I think. So the department head became a laboratory director. I forget where it went from there. I remember joking at the time that they should inflate all the titles so that the staff members like myself should be called research executives and the technicians would be associate research executives. [chuckles] But I don't think they adopted it. Actually, we were known only officially as members of the technical staff, but I've always put in my biography that I was a research physicist, which really was what I was but the title was just "member of the technical staff," like all the engineers and so on.

We also had Richard Bozorth, who was older but had a very distinguished career in magnetic materials. I remember before

I came there, I read an article in *Reviews of Modern Physics* reviewing magnetic materials and it apparently was also being published in Encyclopedia Britannica, the same article, and it was beautifully written--and it was by Bozorth.

The custom then was that each experimentalist had a technician working with him. I had Jerry Caruso for a while, but he didn't like what I was doing so he switched to another department. I had to find another one and then George Devlin came along. Now, he had an unusual background. He was quite young. He had never--I guess he had finished high school, but he certainly had no college. But he had been a champion model airplane builder, and I thought that shows he's pretty good at building things.

He turned out to be very, very smart--but totally nonmathematical. I tried to get him to take college courses and go ahead, but he just couldn't manage math, not even arithmetic. But he could think intuitively about things, extremely well, and he noticed things that I didn't notice about the experiments, so he was really indispensable. He joined me perhaps around 1953 or so and was with me until the end.

Riess: Were these people like Caruso and Devlin freefloating at Bell Labs?

Schawlow: No, no, they were assigned to a particular scientist or engineer.

Riess: What had Devlin been doing before?

Schawlow: Well, he was pretty young; maybe he hadn't been doing anything. I don't know. But he really did a good job.

Riess: Did he understand the experiment?

Schawlow: Yes, he could understand experiments very well--a good understanding of physics, but in a nonmathematical way, which actually suited me pretty well.

There were others in the group, like Ernie Corenzwit who worked for Matthias, and was very good at fabricating the materials that Matthias wanted made. Matthias, Herring, Anderson, Brattain, and myself all became members of the National Academy of Sciences. Wannier never made it, which was really regrettable. He was on the ballot quite often, but when he moved to Oregon he was sort of out of sight, and somehow never got enough votes.

Riess: Is one elected by the entire Academy?

Schawlow: Eventually, yes, but it's a very elaborate procedure, where the individual sections, like physics, they even have subgroups that try and pick out nominees and the section votes on it. The top ones in that go on to the class committee which includes geology and astronomy, and mathematics I think.

I'm sure they must have a lot of fighting in those committees because they have to rank order them, and then when they get on the ballot, you have to vote for a certain number in every class. People in other classes don't really know anything about the candidates say in the physics class, people in biology or something like that. So they tend to vote for the ones that are picked out by that class as being the top candidates. When you can get through these several filters, you may get elected.

Riess: Would you say Stan Morgan particularly brought you along as a group? Or is it just happenstance that all these splendid people were together?

Schawlow: We were hired by various people.

Bell Labs had a very extensive recruiting system then. They would have a contact at each of the major universities who would know the professors and would go there every year and ask, "Who are the good people coming out this year?" Millman was from Columbia, he went to Columbia, and I guess Townes, maybe others told him about me and so he brought me over. I was interviewed by Ad White and by a number--you go around and talk to a number of people there, and finally they decide they want you.

It was a very thorough recruiting. They [Bell Labs] had people at Berkeley. I was recruiting at Toronto for some years when I was at Bell Labs. You recruit not only for your own department, but for others that are not too distant.

Riess: You introduce this by saying that Stan Morgan really didn't tell you what to work on and that was a problem, yet somewhere along the way in the hiring and the recruiting they must give you a pretty clear sense of what they want you to do there.

Schawlow: They had claimed that the purpose of the research department was so that they would be in touch with all the relevant technical and scientific fields, so that if anything that they should know about came along, then they would know about it. They felt the best way to keep informed was to have people actually doing research in these different fields; the

alternative might be having somebody sit in the library, but they would be a year out of date at least. If you're in that field, and you talk with the other leaders, you can really know what's going on.

I was hired because John Bardeen wanted somebody to work on superconductivity, but as I think I may have already said, by the time I got there he was gone. But I wanted to try and work on superconductivity. I didn't see anything else around that I particularly wanted to do, so I did that.

Working up to the Laser

Riess: Okay, about this "not seeing anything else around that you particularly wanted to do," you and Charlie had a close relationship, a family relationship and everything, and the maser was under development at Columbia.

Schawlow: Well, as I told you, I am one of the most anti-competitive people you ever met. I wouldn't think of competing, especially with Charlie, who was very good.

I did do a little work on nuclear quadropole resonance when I first came there. I heard about it and it looked so easy--and it turned out to be--that I did some work on that, wrote a couple of papers on it. [laughs] I remember I did some work on resonances in the ultrahigh frequency region, that is couple hundred megahertz. I had found one resonance in a bromine compound and I couldn't find the other one. I thought I knew where it should be because we knew something about where the bromine resonances were in sodium bromate.

By scaling from chlorine, which is a somewhat similar atom, I thought I had the higher frequency isotope, and I kept looking for the lower frequency resonance--I think the one I found was somewhere like 180 megahertz--and I couldn't find it. Then I thought, "Well maybe it's the other way around," it's up around 215 or so. But there was a television station there. I found that the television station was only off the air from midnight to six a.m., or something like that. So one of the very few times that I came in at night, I came in and looked and found the resonance I was looking for.

The apparatus I used was extremely simple and primitive-looking. I remember I had a visit from Professor Gutowsky from the University of Illinois. He took a look at this and said, "Well, I've never had much luck with simple apparatuses,"

something like that. Or "primitive," I forget what he called it. Well, it was pretty crude, but I was just sort of exploring.

I had some reason to do it because these were moderately sharp resonances and they might perhaps have been used for frequency standards. But I measured the temperature dependence of them--they're quite sensitive to temperature, and they weren't really awfully sharp, so they were not suitable. I mean, I did explore them enough to find that and also get a little data of interest to the physical chemists although I really didn't understand it very much. It gives some information about chemical bonding. but not much.

Riess: You said that to have thought much more about the maser would have been competitive?

Schawlow: Well, at that time it was only the ammonia maser, the other kinds hadn't been invented yet. By the time they were, there were a lot of people in the field, including a group at Bell Labs. In fact, they came around and asked me if I'd like to work on masers, maybe about 1956 or '57, and I said no. I just really couldn't see getting into that.

Riess: But at the same time weren't you and Charlie talking about the potential for an optical maser?

Schawlow: No, we didn't talk about that at all until the fall of '57--I think it was October. By that time he was consulting at Bell Labs and we had lunch together and decided to cooperate. I had begun thinking about trying to find ways to make infrared masers; I hadn't gotten very far but I was thinking about it. Then Charlie came and said he'd been thinking about it too.

See, the original idea of the maser was to get wavelengths shorter than you could produce by radio tubes, but it had not succeeded in that. It had other uses: an atomic clock and sensitive amplifier for radio astronomy and radar and so on. The interesting question was: could you extend it farther?

Well, my thoughts were to just take the next small step, go into the far infrared, closer to the microwaves. But Charlie pointed out that in fact it might not be any harder to go to the visible or near visible region. That appealed to me because there was really at that time very little information about spectra in the far infrared, and the spectra are the raw materials that you have to use. So we agreed to think about it.

We had to, first of all, see whether you could get enough excited atoms at one time. A maser or laser requires that

you have more atoms in the excited state than in some lower atomic state or molecular state, and this doesn't ordinarily happen. In fact, in thermal equilibrium at any temperature whatever, no matter how high, there are always more in the lower states than the upper states.

But he [Townes] had shown in the ammonia case that he could do it, he could find a way. Well, in the case of a microwave maser, the relaxation is very slow: that means the molecules when they are excited don't radiate very fast; they'll stay excited so you can accumulate enough for the purpose. Whereas in the optical region they usually emit their radiation in a millionth of a second or less. But it turns out that that doesn't matter too much. Still, we had to get some specific examples and try and calculate how many that we might need.

I started to look into the alkali metals: sodium, potassium, rubidium--because they have the simplest spectra. In a way, that may have been a mistake--well, those were the things we could get information about, but some of the more complicated ones are more useful.

Riess: Was this an issue of getting materials from Bell Labs?

Schawlow: No, no, this was purely theoretical. What we did was to go to the library and search. Although the spectra were pretty well known, widely published, what was not so widely available was the transition probabilities, or lifetimes of excited states. They're closely related because if the atom is going to be stimulated, it needs to have a certain coupling to the electromagnetic field. And that same coupling is what causes the radiative decay. You can't think of the spontaneous emission as really being stimulated emission, stimulated by the vacuum fluctuations. In the microwave region, it would be the thermal radiation around; in the optical region, it's the fluctuations in the electromagnetic fields of the vacuum. It has no average field, but it does fluctuate.

It turned out, as a matter of fact--Charlie had the equation and I turned it this way and that to try to see what it implied, the maser equation--it turned out that it didn't really matter what the lifetime was because if the lifetime was short, you didn't need very many because they were more strongly coupled to electromagnetic fields. If the lifetime was long, you had to have more. So the number didn't matter.

What did matter was the efficiency, what fractions of these atoms would be stimulated to emit in the particular decay channel that you wanted, at the particular wavelength or transition that you wanted. If they were all going to go off

at some other wavelength, then that made it inefficient. So that was something that we realized, that it was more important to have good quantum efficiency.

The second thing: we had to know the absorption strength to know how much light we would need to excite the atoms. We didn't think of anything at that time except exciting them by light from another kind of a lamp. A method of optical pumping was known in connection with Kastler's work using light to excite atoms to an excited state from which they decay to a particular chosen level of a ground electronic state. But we were in thinking of optical pumping in a different sense, as using light to get atoms into the upper state so we could get the maser action. One of the advantages of the alkalis was that you could get lamps of the same material and they would have the right wavelength for pumping.

Now, of the alkalis, I concentrated on potassium, which was wrong for some reasons. The reason I did it was a very foolish one. I mentioned how hard it was to get equipment when I first came, but the one thing I did get was a wavelength spectrometer, which is a visible spectroscope: you look through the thing in the visible range. I got that for measuring the thickness of thin films. I had that around, but it only worked in the visible region. Potassium had the interesting property that the first and second absorption lines in the spectrum were both in the visible, one in the deep red and one in the blue--whereas all the others, at least one of the lines was out of the visible spectrum, either in infrared or the ultraviolet.

The reason it turned out to be bad was that potassium is very reactive chemically. After we'd finished our paper, Charlie put two students and a visiting scientist on trying to make a potassium laser, and they didn't have much luck because the slightest trace of oxygen in the device would quench the fluorescence. But we could work it out and that's what we used in the publication. You could see that with reasonable lamps, you could get enough excited atoms to get stimulated emission--get enough gain so that with reasonable mirrors you could get reflection.

It's funny, at first we thought of the thing really as like a maser with a box resonator. It's curious that we had "L" was the width of the box, and "D" was the length--I think that's the way it was in the paper too--whereas obviously "L" is the right thing to use for the length and "D" for the diameter. But that was something we inherited from the maser. I forget whether we turned that around before we finished the paper or not. I don't think so.

As I say, I spent a lot of time looking in the Landolt-Börnstein Tables for transition probabilities. These are monumental tables published in Germany, many volumes. There wasn't much information about transition strengths, but there was enough for these simple atoms.

Riess: Thank you for going through that. I know you've written papers that go over this in a very clear way.

Mode Selection

Riess: What I think is interesting at this point is to get an understanding of why Bell Labs called Charlie back to consult, what their motivation was. Did they think he would come back and work on this with you? Was that the intention? And during that time, where did you meet? Did you meet there, or was this all happening on the phone? What were the circumstances.

Schawlow: They called him back because Nicholas Bloembergen had invented the solid state maser, which was obviously a very sensitive amplifier for microwaves, and was tunable. The first one was built at Bell Labs. Bell Labs very quickly got a license, I guess even before the patent was issued, and [H.E.D.] Scovil, [G.] Feher, and [H.] Seidel built the first tunable, solid state maser.

I think they wanted Charlie to help with the progress of the maser program. They did not think at all about optical masers or lasers. This was something just off the books. He came to Bell Labs from time to time to see the maser people, and we would talk in my lab.

##

Riess: How did the two of you work together?

Schawlow: He gave me, I think, some notes that he had made. He had originally proposed thallium and I decided that wasn't going to work because the upper state would empty faster than the lower state so that you wouldn't be able to get an inversion. Well, I'm not sure I was clever enough to find an alternative way to use it, but he accepted my arguments at the time, so that's when I switched to looking at the alkalis, potassium in particular.

We would talk for maybe half an hour or so and I'd tell him what I'd been doing. One illustration of that is that we did

discuss the question of mode selection. He had thought that you'd use some sort of a box with reflecting walls that would be much bigger than the wavelength. For the maser, you could have a box that was comparable in size with the wavelengths so that the wave would only fit in one way, and that would mean that you would get one pure wave stimulated. On the other hand, in the optical region, the wavelength is 30,000 times shorter and if you could make a box that small, you wouldn't have any room to put any atoms in it. (Actually, it's been done since then.)

So we thought, from the beginning, of something of convenient dimensions--centimeters or more. Martin Peter, a Swiss scientist who had gotten a Ph.D. at MIT with Strandberg, had worked some on mode selection there, and he kept urging me that we had to find a way to select one particular mode of oscillation. Well, Charlie felt that even though we couldn't do that, that somehow a few modes would probably have higher gain or lower losses than the others, and might stand out; it might be jumping from one mode to another, but it would be enough different from an ordinary lamp that you could see it.

Well, under Peter's urging I was thinking about it. [laughs] My sister claims it was while I was shaving, but I thought of using two small mirrors far apart. This is like the Fabry-Perot interferometer that I'd used as a graduate student, but not really like it, because those plates were big and close together and these would be just two tiny little plates at the end of a pencil-like column of active media. I thought, "Oh boy, the wave has to go"--simplemindedly--"if the wave's going to get from one mirror to the other it has to go straight along the axis, otherwise it'll go off and be lost." That's why I estimated that you could get the radiation down to an angle of a few degrees. The wavelength would be selected by the atoms; they would only support a small range of wavelength.

I told that to Charlie--I think the next day I happened to see him--and he said, "It's better than that, because the wave is going to bounce back and forth many times, and therefore we'll get really good selection."

Riess: So that was an exciting moment.

Schawlow: It was, yes. I think at that point we felt that we had it, and the only thing left was to write it up. We could have tried to build one, but I didn't have any equipment, and I had only the one technician, and I didn't think of asking for help which maybe I could have had, I don't know. But it just seemed impossible to build one for me.

Riess: And Charlie couldn't have gotten Columbia organized?

Schawlow: He did, actually. Somewhere around February or March of '58 we made the decision that we should write it up. But instead of trying to build one--well, I think we both agreed it was important to write it up first because of what had happened with the maser. I think I've told you about that already, that he had the maser idea, and in those days it was not considered proper to write a proposal of what you were going to do but rather to do it. That almost cost him a Nobel Prize, except by accident it was published. So we decided to publish it rather than try and build it.

Riess: The sense of excitement--I want to know what it was like.

Schawlow: It was exciting to have the ideas that fitted together--couldn't be sure, though, that we hadn't overlooked something. When we presented a draft of the paper for review, some of the theorists including Clogston gave us a hard time because they'd never heard of such a resonator. It was a very strange one with open sides. They said, "How do you know what the modes will be in that kind of a resonator?" Well, I didn't know. All I had was this simple-minded view that if the wave was going to go from here to there, it has to go straight back and forth.

Charlie did put in a little stuff about how much diffraction would spread it. In fact, diffraction is really what makes it work--that is, the spreading of a wave around an obstacle. You see, you start out with one atom, say, and it emits some radiation, and it'll be a circular wave, though, spreading out, and some of it travels along the direction of the axis and gets reflected and stimulates other atoms to go the same way. The light will spread out as it goes back and forth until it fills the space within the mirrors. This is by the process of diffraction.

[laughs] I gave a talk at a conference in 1961 in England, an optics conference. Professor Hopkins there said, "I don't understand how the wave fills the resonator." I tried to explain but he said he still didn't understand! We didn't have a rigorous theory for this kind of resonator. Not long after that was developed by Gardner Fox, and Tingye Li. They did a numerical calculation of the waves between two such resonant mirrors in the resonator and came out with a pretty good description of it.

It's interesting, several people said, "Why don't you use spherical mirrors"--there is a spherical Fabry-Perot--"because the losses would be less." Indeed, George Series suggested

that in 1959. But in fact, you depend on the losses. The ratio of the losses for the different modes is the same, but the absolute value of the losses is larger for the flat mirrors. And if you're working with a solid material, you have pretty large gain and you need fairly large mirror loss to exceed the loss from scattering in the material. So the losses at the mirrors have to be big enough so that only the highest gain mode survives, whereas in a gas laser they do use spherical, or sometime one flat and one spherical mirror, which have lower losses, because they have very much lower gain.

Riess: You mean spherical or do you mean concave?

Schawlow: Concave, yes, parts of a spherical surface. One kind uses a flat mirror and then the other end is a spherical, concave mirror whose center of curvature is at the other mirror. Well, people worked out the losses; in certain spacing between the mirrors the losses are large.

By about 1963, that's after I left Bell Labs, I was beginning to get annoyed by these people saying you needed to have very low loss mirrors and to use concave mirrors for everything. So I actually suggested that you might make mirrors that are curved the other way, that are divergent, for high gain materials--and indeed, people do that for high power now. I didn't bother to patent it or anything, but I did mention it and it's been developed; the theory and all that's been worked out particularly by A. [Anthony] Siegman.

Riess: You were just saying it to make the point.

Schawlow: Yes.

Riess: Your sister said you got the idea shaving. Did you?

Schawlow: I suppose I must have told her that, but I don't remember. I really don't remember it, any more than I remembered rooming with Charlie in Washington!

About the Patent--The Smell of Success

Riess: We've talked before about your not having filled many notebooks, but you did write down your ideas in February 1958.

Schawlow: Unfortunately that was just before I had thought of the two flat mirrors. I was thinking about it. I thought of things where you might use diffraction gradings on the walls that

would reflect different wavelengths differently, at different angles, which later have been used by other people to tune lasers. But I didn't do anything more with it. I think I did describe the potassium system.

I'm not sure exactly what was in those notes because I have only one page of it which the Bell Labs people copied and sent to me. But I don't have the rest of it.

Riess: You put these ideas down--you knew you had a big one here?

Schawlow: Well, yes, I thought it might be something good, that we were kind of getting there. I think by that time I decided that the potassium system could be made to work, so that I had something to write about, and I did put down our thoughts on mode selection.

Then I had it witnessed by Sol Miller, who was one of Charlie Townes' former students, who had a lab next door, I think it was. They had a system at Bell Labs where they would keep people separate departmentally, but they would mix them geographically, deliberately, so you'd get to know people in other areas of the company without having any responsibility to work for them. So I told him about it, he read it, and witnessed it. That was Friday. And then I was rather horrified on Monday to learn that he had gone to IBM.

Riess: How extraordinary! And he didn't tell you.

Schawlow: He didn't tell me, no. I think it was that close, yes. But I don't think it did any harm.

Riess: You also say it seemed best to publish without waiting for experimental verification. But you had to circulate the manuscript for technical comments, and also to the patent department.

Schawlow: Yes. And the patent department didn't want to do anything about it. But Charlie sort of insisted. They said, "Well, this is a maser, just a different wavelength," you know. They didn't realize the importance. And I think really the patent wasn't nearly as good as it could have been if they or we had thought it was important.

I had never patented anything before, but Charlie had and persuaded them to file for a patent. We helped them somewhat, but we didn't put down all the ideas we considered obvious.

Riess: Something Charlie points out in his oral history is that when he had been working at Columbia he was used to talking about

everything he was doing with everyone around him, but that when he was working with you at Bell Labs he didn't talk openly about what he was doing.

Schawlow: Well, I did. I pretty much talked with anybody that wanted to, certainly anybody at Bell Labs. Ali Javan was recruited by Bell Labs about that time. He had been a student with Charlie and then a post-doc. He came out for an interview and I told him about it, and he did come to Bell Labs, but he might not have. I didn't really try to be confidential at all. I don't remember whether I told anybody outside of Bell Labs. I wasn't trying to be particularly confidential. I didn't know whether it was going to work or not.

Riess: Something else you said in a paper was, "Being at Bell Labs, I had been pretty thoroughly indoctrinated to believe that anything that you can do in a gas could be done in a solid, and can be done better in a solid. Al Clogston, my boss at Bell"--he was boss within that Stan Morgan structure?

Schawlow: No, he replaced Morgan when Morgan became head of the chemistry department. Actually he wasn't the immediate one. Ken McKay came first. I don't know when Morgan left. It must have been after only a few years there, and then Ken McKay. He came in and then Clogston. Clogston was very supportive.

Riess: You say he, "encouraged me to, if I wished, drop superconductivity entirely and begin studies of possible optical maser materials."

Schawlow: Yes.

Riess: Then you say parenthetically, "Though no one suggested putting together a group to build an optical maser."

Schawlow: That's right.

Riess: "Anything like that I would have to do myself."

Schawlow: Yes. That's right. Well, I just didn't know how hard it was going to be. I didn't realize how easy it would be. [laughs] I was very close and I just didn't realize it.

Riess: This is really a Joe Six-Pack question for you: did you smell success with this? "We can get this thing patented and we can really make out like crazy?"

Schawlow: No. No, I thought it could be important if it worked. I wasn't absolutely sure that we hadn't overlooked something. We'd been as careful as we could, but I don't know, I'm timid I

guess. Of course, I didn't know what it was going to be like. I thought it might just give microwatts of power at some near-infrared wavelength or something like that. And that wouldn't be terribly useful.

Looking at Materials--Ruby

Schawlow: I think I did mention in my articles that I started to look at materials. In fact, in the paper, I mention that some solid state materials have an advantage because they have broad bands to absorb the radiation and still emit it in narrow lines. I thought you had to have narrow lines because our equation said that the gain was inversely proportional to the line width, so the wider the line, the less gain you'd get for a given number of excited atoms. I really had a fixed idea that you had to have narrow lines, which turned out to be wrong later in some cases. But to get started, that's what we needed.

I knew nothing about solid state spectra and I always like a chance to learn something new, but the only one I knew about at all was ruby, which is chromium in aluminum oxide. We knew about it a little bit because ruby was by that time being used for microwave masers and it was one of the best materials for them. So you could find people that had drawers full of ruby crystals. One thinks of ruby as a very expensive gem, but artificial rubies are not expensive at all. They are made in large quantities. They were used for watch bearings in large numbers and I don't know what all else.

Riess: But they have the same properties?

Schawlow: Yes, in fact they are better than the natural ones for optical things, because natural ones are never very large or pure or unstrained. In fact, ruby always does have some strains in it, it's hard to grow it without strains. But ruby does have a broad absorption band in the middle of the visible so that a broad band lamp could pump it, like a flashlamp, a photographic flashlamp. It does have a sharp line in the red, or a pair of sharp lines called the R-lines.

So I thought, well, I'll look into ruby and see what I can learn about it, try and find out how the line width depends on temperature--for instance, would it get sharp at low temperatures?

Riess: This was while you were still there at Bell Labs?

Schawlow: Yes, yes, we did a lot of work the last two years there. We also looked at chromium in a couple of other materials: magnesium oxide, which is a simple crystal, like rock salt structure, but it couldn't be grown easily--well, it was grown for other purposes in some electric furnace, I forget where, not at Bell Labs. And also we looked at gallium oxide. Gallium is related to aluminum in the periodic table. There's aluminum, gallium, indium.

They had a marvelous crystal grower at Bell Labs, Joe Remeika. (Oh, I was going to tell you a story about him.) He grew some crystals of gallium oxide with various concentrations of chromium in them. We knew that there were other lines in the spectrum, and nobody had any idea what they were--fluorescent lines to the red of these strong R-lines. I guessed that they might be from coupling of vibrations in the crystal to the emitting atoms.

However, Remeika grew crystals of gallium oxide with different chromium concentrations. George Devlin noticed that the strength of these other lines relative to the R-line was different in different crystals. In fact, the more the concentration, the stronger these lines were. He pointed it out to me and I immediately realized that the lines had to be due to pairs of chromium ions that happened to lie close together, because the higher the concentration, the greater the chance of having pairs of ions. So we spent a good bit of time, both there and again at Stanford, studying these pair lines and trying to find out which pairs--the crystal is only moderately complicated, but I think there are a number of nearest neighbor pairs. There's a pair there right along the symmetry axis, and various pairs at different angles that had different distances.

In fact later at Stanford we put stresses on the crystal in different directions to see which lines shifted most with a particular direction. But before I leave Remeika, I must tell you an amusing story. This was earlier. There was a time when people thought that ferroelectric crystals would be useful for computer memories. Now ferroelectric doesn't mean it has any iron in it, but it has an electric susceptibility that resembles the magnetic susceptibility of a ferromagnetic material. However, people had trouble with these things not being good insulators, they would act as semiconductors rather than insulators and were too lossy because the currents would flow through them. Remeika found that he could grow good crystals if he did them in an iron pan, so they got a little bit of iron in them which acted as acceptors to cancel out the donors in the material. They called this Project Ironpan. [laughs] They kept it secret for a while.

Walter Mertz, who was another physicist in the group who later went back to Switzerland and became head of the RCA lab there, was working on these crystals. He gave a talk at a meeting of the American Physical Society. [R.M.] Bozorth was the chairman at this, but he didn't know about this particular project, so after Mertz's talk he asked him innocently, "Can you tell us what was the difference in these crystals that were so much better than the ones people had?" And of course he couldn't tell them, which was embarrassing. [chuckles]

Riess: When you left Bell Labs, were you able to bring George Devlin to Stanford?

Schawlow: No, I offered him to come, but he didn't want to come. He went instead to stay to work with another of Charlie Townes' former students, Stan Geschwind. And he stayed with him for twenty or twenty-five years. Then he retired early and took a job at the NEC Laboratory in Princeton, so he had a good pension, and also probably a good salary.

Riess: What is NEC?

Schawlow: NEC is Nippon Electric Corporation, universally known as NEC. That laboratory is headed up by still another one of Charlie Townes' students, Joe Giordmaine, who had been at Bell Labs too.

Riess: Were you at Bell Labs long enough to get a pension?

Schawlow: No, not one cent. In those days it didn't vest at all. I was there for ten years, but I would have had to stay until retirement to get anything. I think they've changed that. It did leave me a little annoyed but I knew that was the rule.

Where were we?

Riess: You were working on the ruby.

Schawlow: We found out that these other lines were caused by the interaction of chromium ion pairs. And I realized that these pairs would have several levels, not just the one ground state, but they would have several levels near the ground within a few hundred reciprocal centimeters.

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Schawlow: We found the same thing in ruby. The lines had been known in ruby for fifty years, but nobody had any idea what they were about and we were the first to discover that they were caused by chromium ion pairs.

We also realized that the lower levels of these particular pairs of atoms would be split by maybe several hundred wave numbers. (A wave number is equivalent to roughly a degree absolute.) So if we could cool it down to low temperatures we could empty the upper ones among this group of lower ground levels. Instead of having just one ground level, we'd have an array of a few of them, and then we could cool it and get the empty lower state.

I thought, "Boy, that's what we need." We may not be able to put very many atoms in the excited state and if we have an empty lower state, then we'll get gain immediately. I actually tried that, very sloppily, with what I had around. I still had an old Dewar from the superconductivity days.

Riess: An old what?

Schawlow: Dewar, D-E-W-A-R. That is a vacuum flask for getting low temperatures by insulating liquid helium. I had a of dark ruby polished; the ends were polished flat and parallel as near as I could get. (I still have the order for that.) I cooled that down in this thing. But then I didn't buy a big flashlamp, which I should have but I didn't. I just had a stroboscope sort of thing, I think a General Radio Strobotac, which is only about twenty-five watt seconds--not a very powerful flash at all. I tried that and nothing happened so I just put it aside.

Riess: This story is painful in many respects which are obvious to you, too, that the materials that you keep--.

Schawlow: I was stupid.

Riess: No, no. No! It's almost like you were programmed from those early days of Toronto not to expect to be able to get hold of what you needed if you wanted it.

Schawlow: I think that's right, yes. I think that's right. I just sort of learned to make do with what I have. I did not have an aggressive training. I think other students who came in had been in labs where they had money, particularly in the years like the late fifties and sixties when the government was putting a lot of money into research and people got anything they wanted. Yes, I think that's true.

Riess: But then on the other hand maybe it's given rise to more ingenious solutions.

Schawlow: Yes.

Well, but then I really put my foot in it. I gave a talk at the first Quantum Electronics Conference which was held in 1959 after we'd published our first paper. I mentioned about the ruby pairs and said that they would be good for an optical maser, but that the R-line was not suitable for maser action because it went to the ground state.

Well, [Ted] Maiman next year proved me wrong on that, and one of the reasons I said that was partly because I didn't think quantitatively, but there were no less than three measurements of the quantum efficiency of atoms when they're excited to the upper level of the R-line, and they all were between one and three percent. I don't know how they were so far wrong, but if it had been that low then it wouldn't have worked. In fact, we did some experiments which really indicated that it was much higher than that but we didn't make a direct measurement.

The experiments we did were on radiation trapping in ruby. That was really almost the most fun experiment I ever did. I had known from work in Toronto--I'd heard Crawford talk about it--that if you have a lot of atoms, then when one emits another one may absorb it, so the light has a hard time getting out, and the apparent lifetime will be longer. I forget the exact course of the thinking: I think that sapphire, which has only a very tiny trace of chromium in it, did give a lifetime of something like three milliseconds. The papers exist, we can check those things. Whereas the ruby was considerably longer than that, I think as much as twelve milliseconds or something like that. So I thought maybe it might be trapping. With the collaboration of Darwin Wood of the chemistry department--he had a diamond saw and cut us a thin slice of ruby, and we measured the lifetime. It was somewhat shorter, but not as short as the really dilute material.

So then he ground it up for us--this was all done in a day or so--and it got shorter still, but still not as short. Finally we took some of this black stuff that's like plasticine, it's called Apiezon Q, which is used for vacuum work, and we embedded the grains in the surface of this black stuff so that one grain could not see the other. And we finally got the lifetime as short as you got for really dilute material.

Now that should have shown us that the quantum efficiency was pretty high, because these things were able to catch it and re-emit it.

Riess: Why are you saying that was fun? What made it fun?

Schawlow: Oh, well we did it so quickly. You try one thing, you get some results; you have an idea and try that, try the next one. It was really fun, I really liked that. That's what I consider fun, when you start getting some results and that suggests something else, and you can try that out. Usually though you have to do a lot of preparation to try another thing. In this case we were able to do it right away. So, we did this work on radiation trapping and that really did show the lifetime was longer.

Ted Maiman's Work, and Publication

Schawlow: Maiman, I think, made his own measurements on the fluorescence efficiency, did a quantitative job, and realized that he could actually excite enough atoms to invert the population. And he did so.

Riess: And he built the first one.

Schawlow: He built the first one that worked, though there are some funny stories about that too.

Riess: Go ahead.

Schawlow: Well, there was a very sad story about this publication. He had a paper in *Physical Review Letters*, published I think in May of 1960, in which he did some excitation of ruby. What was it he measured? I thought he was working on optical pumping of the ground state of ruby and didn't pay much attention to it, although it was sent to me for refereeing and I said it was okay--except I made him put in something what concentration of ruby he was using, how much chromium.

But then in June or early July, he got his laser working, and he sent another letter to *Physical Review Letters* and it was rejected. Now *Physical Review Letters* had published an editorial saying there'd been too many maser papers and they weren't going to print any more maser papers. And he, I guess, called it an optical maser and they rejected it. He thought they'd done it because they wouldn't take any more maser papers.

In fact, a few years later I talked with Simon Pasternak, who was one of the coeditors of *Physical Review Letters*, and he told me that they hadn't bothered to referee it. They felt it was a case of serial publication, whereas they wanted people to finish a project and write up a full report rather than

dribbling it out in little bits and pieces. Since he'd just had a paper published on exciting ruby, they didn't bother to have anybody referee it.

Well, Maiman didn't know. He thought it was because it was masers and he didn't ask for another referee, which is the normal thing. Instead, he submitted it to the *Journal of Applied Physics* and they said they would publish it. But Hughes was quite excited about it and they had a high-powered publicity agent they hired for the thing, and this guy sent around preprints of Maiman's article for *Journal of Applied Physics* to various trade journals. One of them, *British Communications and Electronics*, published it, without permission. They did it quite quickly, I think in August.

They'd had a press conference in July, that was when they announced it. I had a preprint of it--so did a number of other people. This press conference got a lot of attention. However, once this *British Communications and Electronics* had published it, the *Journal of Applied Physics* said they couldn't publish it because it had already been published. So then he finally sent a slightly abridged version to *Nature*, and they published it I think around September.

In the original article he said only that he used a crystal of centimeter dimensions. And I think he made some remark that because of the reflections from the side walls it wouldn't produce a beam--I don't know exactly. So we thought, well, we'll get smart. At that point several people at Bell Labs quickly got into the thing and set up big flashlamps.

Riess: Under you?

Schawlow: No.

Riess: But you were still associated with it?

Schawlow: Yes. They were friendly. There were two groups. One was with Bob Collins, Robert J. Collins, and Don Nelson. They were in the same building and we talked quite a lot. In fact I had talked with Collins earlier about potassium lamps, how much light you could expect, and that sort of thing. So they built up a ruby laser.

Maybe I ought to go back a minute and put in something I forgot about. In this first Quantum Electronics Conference paper I wrote up that the structure of a solid state optical maser would be especially simple: just a rod with the ends polished flat and parallel and coated to reflect light, and the sides left open to admit pumping radiation. Well, when I saw

the picture of Maiman in the newspaper with a little rod of ruby, it was exactly what I had in mind.

Anyway, the people at Bell Labs thought they would check the predicted properties. I couldn't resist joining in some of that with Collins and Nelson. I had a good spectrograph so we could measure the line width and found that it was sharper--the stimulated fluorescence was narrower, as predicted.

Riess: Say that again.

Schawlow: The emission bandwidth of light emitted by the laser was in a narrower band than the spontaneous emission of the ruby by itself. That is, at lower powers it would have would have emitted over a certain broad wavelength, but the only part that was stimulated would be at the center of the emission line where the gain was highest. So we verified that.

I had a good oscilloscope. It's amusing. I think I told you that after I'd been there about five or six years they loosened the purse strings for apparatus. People could buy almost anything they wanted. I didn't buy anything very extravagant, but when we got into this laser materials I decided to buy the best oscilloscope I could find on the market, the most expensive one. This was a dual beam oscilloscope from Tektronix, so it could display two traces at the same time.

Well, one of the things we did was look at the time development of the laser pulse. George Devlin asked, "Is there any sign of hysteresis?" That is, a thing having friction being slow to start up and slow to stop. We thought, let's look at the details of this line. The dual beam oscilloscope turned out to be exactly the right thing because we could spread out one of the traces, so that the whole scan was only about half a millisecond or so, which was the length of a laser pulse. You could see there were spikes, that is, it was not going all at once but in narrow spikes. So we were the ones to discover that. It's particularly so for the ruby, not for all other lasers.

Riess: So does this mean then that you put pen to paper?

Schawlow: Well, in a short time. But then this other group, Garrett and Kaiser--Geoffrey Garrett and Wolfgang Kaiser--was working in the other building. They were somewhat more competitive. We didn't know exactly what they were doing.

It was really bothering me. One night I couldn't sleep. I was wondering, now does this thing really produce a narrow

beam? We couldn't see it in our early ones because we had this bright flashlamp which put a tremendous amount of light, lit up the whole room--we really hadn't boxed it in. The next morning I came in and insisted that we've got to look to see if we have a beam. What we did was just use a camera to photograph the spot that it was producing, and indeed it was a narrow beam.[1] And I thought we should get a narrow beam because we had a rod that--I think I had suggested we have it rough-ground on the side so that you wouldn't get a lot of reflection from the sides.

However, a few days later the group of Garrett and Kaiser, who were working also with Walter Bond--he was basically a

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[1]

Riess: [from several pages later in the interview] Just a footnote to something you said before. I couldn't visualize how you could only test the focus beam by actually taking a photograph of it.

Schawlow: Well, it was silly, we didn't really have to, but we had a camera which I'd bought for work on superconductivity, looking at the powder patterns, a Speed Graphic camera, and you could put Polaroid film in it. The reason we needed the camera was because the whole room lit up. As I say, we just hadn't boxed in the flashlamp. You'd put this thing up close, and you'd put a shield around so to shield the camera without shielding all the rest of the room, and put it up close to the rod or maybe a few inches away, and then see whether you got a spot or not.

One of the things we did find there is that emission was occurring from many separate filaments in the rod. You know, one of the reasons why I didn't know whether it was going to work was that this ruby was really a very poor optical material. It's a very wavy structure in the thing, almost like Coke-bottle glass somebody said, so was it possible that the wave could go from one mirror to the other without being terribly distorted? Well, apparently what happened was that there were a number of little small paths that the light could go through from one mirror to the end of the other and get reflected back. So the thing actually lased in a large number of small filaments.

And you could see that by photographing the end of the rod. The obvious way, and we didn't always do the obvious things, would have been to just build a box around the whole thing. Even a cardboard box would have done to cut out the stray light, but we were in such a hurry to try everything that we didn't stop to do that.

crystallographer, but he was polishing the crystals for them, and in fact he found good ways of polishing ruby crystals, a very wonderful person, he contributed a lot to the techniques at Bell Labs. Anyway, it was Bond, Garrett, and Kaiser, though Garrett and Kaiser were doing the experiments. They had a rod that was not ground on the sides, it was just polished, but they boxed it in and they could see the spot on the ceiling. It was a small spot. It turned out that polishing the sides didn't matter.

By about that time Maiman had also discovered that his thing was producing a beam. He didn't publish it right away, but Mary Warga, who was the executive secretary of the Optical Society of America, very much on the ball with the early laser stuff, she got him to give an invited paper at the fall meeting of the Optical Society. The deadline for abstracts was the end of July and in that he said he had a beam, so he must have had it by about then. But it wasn't in print until October [1960].

So we had these various properties and we finally agreed that we would set a deadline and we would pool everything we had and write a letter to *Physical Review Letters* as of a certain date--I think it was in September. We did an experiment with Collins and Nelson to show that the beam was coherent; we got diffraction from a single slit. We were going to do a double slit, but we ran out of time. So we published that. We were careful not to use the word maser in the article, but it was published without any problem in the fall of 1960.

Riess: You published with Bond, Garrett, and Kaiser?

Schawlow: And Collins and Nelson. They had a press conference about that time, Bell Labs did. There was a good bit of jealousy there. They didn't want me to come first in the program, they had me come somewhere in the middle to explain how the thing worked. But they didn't fool anybody, the newspapers knew who had started all this, but the jealousy was there all right.

I had promised to give a talk at the Northeast Electronics Conference, and I had to send around an abstract for clearance. Garrett and Kaiser objected and said something about shouldn't all the authors of this paper be included or something like that. I was really very upset, and I complained to Clogston. He said, "Leave this to me, I'll handle it." But I said, "If necessary, I'll just talk about the things before our experiments." Anyway, I felt that they were jealous. Also, there was increased secrecy, people doing things and not telling you.

One thing of course I noticed was that all of a sudden I had more people talking to me than I had time to talk with, whereas before on superconductivity I was really all alone and nobody cared about what I was doing.

Riess: This business about hysteresis--did George Devlin get credit?

Schawlow: I'd have to look that up. I did put him on a number of papers. I don't think so, no, because that was included in a paper where they already had six authors. I hoped we thanked him, but I'm not sure.

One other thing Devlin did. When I was looking at the spectrum of ruby I was studying how the line was dependent on temperature, and it didn't get nearly as narrow as you would expect to have at low temperatures. You'd think it should be very narrow because the lifetime was milliseconds so it should be a line width of kilocycles. But it wasn't, it was much wider than that. So I was looking at the thing with high resolution and Devlin was helping make the scans.

He noticed a little bump on the side of the thing, and he insisted that that's real. I thought oh, it was just noise, you know. He said, "That's real." Of course, again I realized immediately that could be an isotope effect because there are several chromium isotopes. And indeed it was. Remeika made us samples of the separated chromium isotopes. Devlin was wonderful. He didn't really know the theory of the thing, but he had open eyes and he'd see things.

Riess: To track some of these publication dates then--

Schawlow: Do you have my bibliography? If you don't, I should give you a copy.

Riess: The article for *Physical Review* that you and Charlie did came out in December of 1958.

Schawlow: That's right. It was published very quickly. We submitted it in late July or early August of 1958.

Riess: When you publish are there letters in response or is that not the kind of situation?

Schawlow: Not usually. People can complain if there are mistakes in it, like you didn't give credit to somebody, something like that. But there was no response after that. The attitude of most people was they didn't think it would work and gave various reasons for it. But a few people believed in it, started out

to try and make optical masers with various materials. And by the end of 1960 there were I think five different lasers.

Riess: The Quantum Electronics conference [September 1959] must have been an exciting event in and of itself.

Schawlow: Probably. Most of it was on microwave masers. I was so busy and so slow that I didn't go the first two days of it. I just stayed back at Bell Labs writing the paper because I didn't have time to do it before then. So I only went to the last day of the thing. And there weren't many people working on optical masers yet, but they sure were after that.

Riess: Did Maiman pick up on it from being in the audience or from a later publication?

Schawlow: He had been in the audience, but also there was a conference that Peter Franken sponsored on optical pumping at Ann Arbor. He called me up just a few days before the meeting and wanted me to preside at a session and give a talk. Well, there was no time to get official clearance for a talk--that was in '59 too, I think Maiman was there. But I did tell a little bit about these pair lines which were in the course of publication. I did suggest just that they'd be suitable for various kinds of masers without being specific.

Riess: I'll be interested in the bibliography.

Schawlow: If you want to take a moment's recess, I think I can start the computer printing it out.

Pressure Results in Exhaustion

Riess: It's clear that 1960 was a big year in your life.

Schawlow: It really was. We had a lot of results. It's one of the reasons why I didn't try to build a laser myself--I should have --but I was finding so many interesting things in these experiments.

Riess: You said something about thinking about the ruby lines and you couldn't get to sleep.

Schawlow: No, the thing I couldn't get to sleep about was I wanted to know whether the laser produced a beam or not.

Riess: Okay, right. But it made me wonder how good you are about leaving it behind when you come home?

Schawlow: Pretty much in those days. I didn't really work at home very much.

Riess: Did you go in on weekends?

Schawlow: No. Things have changed at Bell Labs. There wasn't much pressure in those days. I think we felt that everybody was more or less equal. We didn't know what people were making. Later on they made a point of introducing what they called "octiles," where they were dividing everybody into categories and said they were going to adjust salaries accordingly. What I heard from others who were at Bell Labs later was that then the pressure sort of grew, and people were working night and day there. It wasn't that way when I was there. They did work hard during the day, but that was it.

You couldn't help thinking about things sometimes. The particular time when I was sleepless was when the three of us--Collins, Nelson, and myself--knew that there were a lot of things to try out. They did bring their laser down to my lab because I had the spectroscope and oscilloscope and so on. So we'd argue about what to do next, and it was at that point that I sort of felt that I just had to take over and check to see if there was a beam or not. And there was.

Riess: Was Charlie still involved?

Schawlow: Not very much.

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Riess: Was he still consulting at Bell Labs?

Schawlow: Let me think. I'm trying to get the schedule of things. In 1959 he took a position in Washington with the Institute for Defense Analyses [IDA]. After that, he couldn't consult. Columbia later asked me to come as a visiting associate professor during the academic year 1959-60 to help his students where I could and to teach some classes.

Riess: Students who were working on maser experiments?

Schawlow: Yes, that's right.

Well, that was a horrible time for me. That was I guess in the winter of 1960 before any lasers had operated. It was horrible because I couldn't leave the stuff at Bell Labs.

Devlin was still working, but he did flounder when I wasn't there. Things were not getting done. I would go in [to New York City], I don't know, three or four times a week, and I would come home every night and that was a long trip.

I got sick. I got cold after cold, ended up with a fever around the end. I was supposed to give a talk in June, or July, at a meeting, and I had to cancel at the last minute because I had a fever of 103 or so. At this point the doctor finally gave me an antibiotic that took care of it. It was just sheer exhaustion, and I've learned since then that if I get overtired, I get sick.

Riess: How did Aurelia respond to that?

Schawlow: She was very good, but it must have been very hard for her because she had the three children by then. Of course, when I was home I'd do what I could. We did a lot of things with the family when I was home, and we were involved with the church, the nice young people's group that we were in.

Riess: By "respond," I would expect a sort of outrage.

Schawlow: No, she didn't push me to do anything. Then I think when I started getting offers in 1961--I was approached by a number of different universities--.

Riess: Let's get back to the chronology. You had a Columbia spring semester, and that ended when summer came along?

Schawlow: Yes.

Riess: And then summer and fall you were back in the groove at Bell?

Schawlow: Yes, I guess so. Now, what was I doing? Oh, I was still working on some of these solid state materials.

We had a visitor from Japan, Satoru Sugano, a brilliant man and a wonderful person. He had already published papers on the theory of ruby before he came. I think he must have come in '59. It was before we published our work on the pair spectrum. So I think he probably was surprised when he saw that, but we did work on stressing ions and crystals, and he did some theoretical work on that. I was surprised when I went to an American Physical Society meeting and found he had been collaborating with two other people at Bell Labs too. He was just very productive.

I just had a fax from him two days ago. He retired--the Japanese style is they retire at sixty and usually take another

job for five years as a dean or something like that at another place. And he did that. But he retired the second time a year or so ago. He's building a house in the mountains in central Japan, some town whose name he gave but which means nothing to me. But he also apparently inherited some money and is using that to set up a foundation to put on conferences in the fields he's been interested in.

Publishing with Bell Labs--The Clad Rod Laser

Riess: You've talked about publications. What kind of support did Bell Labs give you? Did they do the typing?

Schawlow: Yes. They did do the typing. How did they do that? I forget whether there was a departmental secretary that did it or not. There must have been. They did have a typing pool, maybe that's where it was done. I don't remember.

I was in a carpool around that time and there was a lady there who worked in the editorial branch who was supposed to straighten out the language of engineers in their reports. So I asked her to try and see what she could do with one of mine. She said I didn't need her help.

Riess: No, I would say not. I think your papers, the ones I've read, are clear.

Schawlow: Well, once I get the ideas clear, it's not hard to say it. That's the hard part.

Riess: Was there a "publish or perish" feeling at Bell Labs?

Schawlow: No, not really, although I felt that I obviously had to produce something.

I think I was probably in danger of perishing before I got into this [maser work]. I don't think that I was very highly rated at Bell Labs at all. I think I mentioned that they made me the department safety representative and also asked me to supervise a technician who was running the helium liquefier which turned out to be a terrible headache. I worked hard to get him classified to a higher rating, which he really didn't deserve, but I finally managed to pull it off. But he didn't get enough of a raise, so he then filed a grievance with the union I think.

Riess: Well, good that you got on to this.

Schawlow: Yes. It's good that I got onto the optical stuff. The laser was obviously something important. They realized that right away.

Riess: So the clad rod laser?

Schawlow: That was something we did probably in the time you're talking about, probably the end of 1960.

Riess: What does that mean, clad rod?

Schawlow: A clad rod. It meant that the ruby rod was the core of a larger rod whose outside was clear sapphire. Now I'd heard that the Union Carbide people were making some of their crystals in a doughnut form, or like this, [draws] a disk. They would drop sapphire or ruby grains on the edge and melt them with a torch. And as this thing rotated [demonstrating on lid of a sugar bowl] it would grow radially like that.

So I realized that they could grow one that had ruby at the core and then sapphire outside that. I also realized that if you look at the way the light goes in there, it's bent towards the axis; no matter what angle it comes in from the side it's refracted, because there's a high refractive index in ruby. It bends more towards the center, so that all the light over a big angle would come through this central core. Thus you get more efficient pumping and so get lower pumping power required.

Riess: So essentially it's a sapphire-clad ruby rod.

Schawlow: Yes, that's right. Ruby-clad with a sapphire outside. We got a patent on that, I think. But they were hard to make and they were more strained than the pure ruby rod because the sapphire has a slightly different crystal structure spacing than the ruby, so it didn't quite fit and that strained the material.

An interesting thing was pointed out by Joe Giordmaine--he explained why it was the early ruby rods produced a good beam, even though they could get reflection off the sides. The reason was this business of the focusing of the light as it came in, so that it was more intense at the center than at the outside. So as you'd reach the threshold for laser oscillation along the axis of the rod, and not at the outside, and it would be absorbing at the outside. That's why light that went out to the side and was reflected wouldn't get amplified much.

I think that may have been what inspired me to think that the clad rod, that here was the ruby and it was being focused, but some of it was being absorbed and therefore wasn't useful

in the outer regions. So the idea was to have the same focusing effect without the absorption.

Riess: What were the virtues of the clad rod laser?

Schawlow: Lower pumping power. More efficiency--you collect the light more efficiently. It was fun thinking about it. It was the sort of thing, again, that people didn't believe at first.

Riess: You like that, don't you?

Schawlow: I do, yes. I think I've said before, if you have something that some people can't believe and say it's wrong, and others say it's obvious, then I feel I have something good.

Time to Leave Bell Labs

Riess: Fall of 1960. I wonder what was going on that convinced you that it was time to leave?

Schawlow: I had a whole lot of different experiments going on that I was trying to do, a whole lot of ideas. I just couldn't do all the things I had in mind to do, so I felt it would be good to have students to work with me. That was the main reason, I think, intellectually. Also, I was getting annoyed at the jealousy that was apparent among some of the people at Bell.

Riess: Did you approach Bell with what you wanted?

Schawlow: No.

Riess: You just knew that within the structure it wouldn't happen.

Schawlow: Yes. Charlie ran into that too earlier. They encouraged his work on microwave spectroscopy, but they wouldn't give him another person to work on it. So I just kind of assumed that was all one could do.

Now Ali Javan, who had the proposal for the helium neon [He-Ne laser], which was the first gas laser, did manage to get two others to work with him on it. Two very good people. So maybe some things could have been done, but I think that was the main reason. And then the question of Artie came up too: New Jersey was a terrible place for illness in those days.

Riess: I want Artie not to be just brought in sideways. Maybe the next time we could start out by talking about that.

Schawlow: Yes. It was a consideration, and in fact one of the reasons why I went to Stanford rather than somewhere else.

Riess: How did you go about making yourself known, that you were available?

Schawlow: Didn't have to. People would call me. I didn't apply for anything.

Riess: What were the most appealing choices? Did you go to meetings and talk to people, or did you already know all the universities?

Schawlow: Indiana University invited me and I went and talked with them. It was attractive. Although it wasn't a great university, it was a good place and I think it would've been good. Professor Mitchell was the chairman, and he had worked earlier on resonance radiation and co-authored a book on it back in the thirties from which we got a lot of information.

The University of Toronto came after me. That was, of course, attractive in some ways, but Aurelia didn't want to go there. She'd been to Toronto several times, and people there--. Well, she was a Southerner and Southerners, you know, you go to New York or somewhere they kind of twit you about the lynchings there and things back there. And you go to Toronto and they sort of come at you about the things that the United States government is doing that they don't like. So she felt that she just wouldn't be comfortable there, although if I'd really wanted to go, I think she would have gone anywhere I wanted. Certainly she didn't like the idea of going as far as California, but there were good reasons to go.

I did get approached by Johns Hopkins and also Columbia, but I think that was after I'd accepted Stanford.

Riess: Would you have to have brought your own money?

Schawlow: No, I really didn't know what the things were going on there [at Stanford], but I sort of assumed there would be money like there was at Bell Labs and I went ahead and ordered stuff. And in fact the Microwave Laboratory at Stanford was well-funded. I knew I'd have to apply for some money of my own, but they set me up and get me equipment for things, some fairly expensive stuff.

Riess: Did you talk about money with the other places? Indiana?

Schawlow: No, I didn't discuss what money they could provide. I know I was used to--working at Bell Labs, well, sort of "money comes."

Riess: And money was coming for science then?

Schawlow: Yes. Actually it was beginning to decline slightly. Apparently in the late fifties it had really been awash with money and you could just get anything for any purpose. By then it was beginning to get a little tighter, but it wasn't bad. I got support from NASA first.

Riess: What did it feel like to be out of Bell Labs?

Schawlow: Well, nervous. I managed to get a one year leave of absence in case I wanted to come back, but I didn't really think I was going to. But you know, there were things I'd worry about--like I had to teach, and I didn't know whether I could do a conscientious job of teaching and still have any time for research. Well, I guess I feel I've never had enough time for either of them. But I could do an adequate job, I think. But I could have been a better teacher if I hadn't had other distractions.

The most productive time for experimental physicists is between ages thirty-five and forty, and those were good years for me. I was thirty-five in 1956 and forty in 1961, full of ideas and able to get a lot of them tried out, and some of them were working and other people were working on things I'd started. Of course I was trying to follow everything that was going on in connection with lasers, which has long since become impossible. So it was an exciting time.

Certainly when I heard about the announcement of Maiman's first laser, I was really excited because then I began to realize how important it was, because he'd got just a short pulse but peak power of kilowatts. And I'd been thinking of milliwatts. So this was much bigger than I had thought of.

When this picture of Maiman appeared in the newspapers, he was holding a flashlamp, a pretty big flashlamp--obviously a General Electric FT 524, because there weren't many flashlamps on the market at that time. He didn't say what he used and he didn't mention the rod, and the rod was obviously a few centimeters long, maybe five centimeters and about five millimeters in diameter, something like that. Just, as I say, what I'd been thinking of.

But in fact, that wasn't what he used. He used a smaller lamp and a short, stubby crystal. I think it was this focusing effect that made his produce a beam. Well, the question of why he showed a different one [in the picture], one of his colleagues told me the reason was that when the photographer came to take the picture all of the lamps he'd actually used

were broken. [laughs] He himself has testified, I think in a patent suit, that the photographer thought this was better looking, but I don't know.

Riess: What was the patent suit?

Schawlow: I don't know. He did get a narrow patent on ruby lasers, but the basic patent was ours, which was issued ridiculously early in 1960, March of 1960 and of course expired in '77.

Riess: You had no control over that.

Schawlow: No.

We saw that picture and we recognized what it was, so we bought some FT 524 lamps. The advantage of that was that I was able to put a small vacuum jacket inside a glass finger, a Dewar vacuum flask, so I could cool my dark ruby rod to low temperatures and still get a powerful blast from it. That was one thing that made it easy both to check that the lines got sharper as you cooled the crystal--even the laser lines did--and you could run the dark ruby pair line laser at liquid nitrogen even.

Riess: We are looking at this picture from the IEEE article, July 1976.

Schawlow: Well, that one we used--it's not easy to see in this copy, but we used a straight flash lamp and a reflector, an elliptic cylinder reflector. But in our earliest experiments we used the same sort of a Dewar that you see there, with a fairly narrow finger but it had to be big enough to contain a vacuum jacket. And we just put it down inside the flashlamp.

Riess: This is the drawing of the Dewar.

Schawlow: Yes. It shows it inside the cylindrical metal housing. I think they cropped that a little bit so that you can't see where the flashlamp is in the drawing, but the flashlamp would be off to one side in the cylinder. There's the cylinder--the lamp would be over here somewhere and the light would be reflected from the inside of that cylindrical mirror.

Riess: You do realize how simple this whole thing looks?

Schawlow: That's the way I am. If I had known it was that easy--. I just couldn't think that anything that simple would work.

Riess: Isn't it extraordinary? I don't know whether this is like a general principle of physics

Schawlow: No, it's just the way I work. I just don't have the mind to do complicated things.

Back to the first laser: now, a laser doesn't work until you get above the threshold where you have enough gain from excited atoms to overcome losses. Well, we had a very poor ruby rod, and we had a power supply, and a big lamp that was rated at 4,000 watt-seconds--that was the most you were supposed to put in it--or 4,000 joules. But it didn't lase at that. So I thought, "Well, what have we got to lose"--we turned up the power and at 4200 joules it started to lase. That same thing happened again once with one of my graduate students, but that's later.

Riess: What do you learn from that?

Schawlow: You learn that there's a threshold and you have to get over that threshold. It doesn't come up gradually. Well, there is a buildup close to it, but it's a sudden thing. If you're below the threshold, it isn't lasing; if you're above it, it is.

Riess: A couple of stories you have told of going from doing things the way you're supposed to, slowly and meticulously, to blasting off, like when you were trying to--

Schawlow: Get those mirrors.

Riess: Yes, right.

Schawlow: Sometimes you have to be rough.

Riess: Ah! That's what I wanted--a quotable end line: "Sometimes you have to be rough."

National Inventors Hall of Fame, 1996

[Interview 5: October 30, 1996] ##

Schawlow: [Talking about recent trip to Akron, Ohio, to be inducted into the National Inventors Hall of Fame] By Thursday morning I had bad chest pains, and that turned out to be pleurisy. I had to sit up all that night because I couldn't find any position

where I could lie down. But then, as you can see in that newspaper story, Dr. Forrest Bird treated me.[1]

Riess: And who is he?

Schawlow: He's a member of the National Inventors Hall of Fame for inventing various respirators. He had one shipped in and he gave me a treatment and managed to get my lungs straightened out. Apparently pleurisy only lasts a few days anyway, but he got me breathing again pretty quickly.

[indicating the respirator] The thing is worth $3600. Dr. Bird gave it to me, and that was because he is very grateful for inventing the laser, because his wife just had an operation for endometriosis with a laser. It's a pulsed respirator; it puts out pulses of air up to five times a second and about forty pounds per square inch. This is supposed to loosen up stuff in your lungs and so on.

Riess: Quite a story, and quite a coincidence.

Now, what is this videotape that you've given to me?

Schawlow: That's quite a story too. Back in 1965 or 1966--the California Academy of Sciences had been sponsoring a program called "Science In Action" on educational tv, and this was near the end of their run. They had an independent producer, and they decided that they would do one on "the scientist," and they somehow picked me as the scientist. They came down to my lab and to my house, filmed me with my daughters. In that I talk a little about how I felt about physics and things that you are going to discuss today.

One of my former post-docs called and urgently wanted a copy of that film. I could not find that videotape, and I'd sent the film to Cleveland to use in the material for the National Inventors Hall of Fame induction ceremony, and it hadn't come back. So the day before yesterday I called them in Cleveland. They said, "Oh, we sent that ten days ago, on October 10." They sent it to the university. They checked and found who had signed for it. Well, I asked the secretary. She hadn't seen it. Turned out it was down in the mail room, they had just left the box down there.

I got it the day before yesterday and yesterday I made a copy and sent it to him. And I thought, well, maybe you would

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[1]See *Akron Beacon Journal*, September 23, 1996.

be interested in that too. It's all about me and I do talk about how I felt about things in physics.

Riess: You were the representative scientist.

Schawlow: Yes, just the only one they did. Instead of talking about some particular discovery, they just talk about one scientist and see what he does, see what he's like, that sort of thing, which is a wonderful thing to have.

The people in Cleveland had made a good VHS copy from the film, much better than I've been able to get made, so I made a duplicate of the thing.

Riess: Thank you. Is that something you're able to do with your equipment here, you can make duplicates?

Schawlow: Yes, well, I have two recorders so I can just take one from the other room and hook it up here.

Riess: When you were in the hospital and surrounded by all the electronic monitors and gadgets, did you have some curiosity all that?

Schawlow: I was pretty sick. Well, I admired some of the gadgets but I didn't really get into how they worked or anything like that in detail.

Laser Action in Ruby--*Physical Review Letters*, Feb. 1, 1961

Riess: Last time we had gotten to the point of your coming to Stanford, but I realize you haven't told about your 1961 publication on laser action in ruby. That's the article that was published at the same time as an article by [I.] Wieder and [L.R.] Sarles.

Schawlow: Okay. I had been rather simplistic in my approach to things. I had not really done any quantitative calculations, I just sort of went by instinct. I used Charlie's maser equation as a guide, but still--I saw that to get gain you had to have more atoms in the excited state than in the lower state.

One substance that kind of fascinated me was ruby. I didn't know anything about solids but I had a feeling that well, it was sort of the Bell Labs culture, that anything you can do in a gas you can do better in a solid. Ruby was a crystal. There were samples around because they were using it

for microwave masers, and so I thought I'd take a look at the spectrum of ruby.

Trouble was that the atoms are all in the ground state when you start--and although there's a good broad band that you can pump into with the green region, and then the ions all populate a level that fluoresces to the ground state and produce a red line--actually, there are two very close together. But the trouble is that the atoms there are all in the ground state and you'd have to excite more than half of them before you'd get any gain. That didn't seem to me a practical sort of thing.

But as we studied the spectrum of ruby we noticed that there were a lot of other lines there that were not accounted for by the theory. Fortunately we found out that they were due to pairs of chromium ions, because their proportion relative to the single ion lines got stronger as you made it more concentrated. George Devlin noticed that, actually in some crystals of gallium oxide with chromium, which is closely related to the aluminum oxide with chromium which is ruby.

So we saw these lines were due to pairs and they were split by fairly large amounts by the exchange interaction between these chromium ion pairs which, in concentrated chromium oxide where it's all chromium and no aluminum, makes it anti-ferromagnetic. That is, the spins of adjacent neighbors are paired anti-parallel. Well, this meant that here was a system that did have lines that were spread out over a substantial region--and that meant that the energy levels were also split by several hundred wave numbers, which is equivalent to several hundred degrees temperature.

So it occurred to me that by cooling that stuff to a low temperature--I didn't know how low you'd have to go--then you could empty some of these lower levels, and then you would have a much lower threshold. And all you had to do was get some atoms excited and you'd get gain. How much gain you'd need was hard to predict because the optical quality of these rubies is very poor. Just like, somebody said, Coke-bottle glass--they don't put Coke in bottles any more, I don't think, but anyway, it was not optical glass.

I talked about that at the first quantum electronics conference. We published the results. I foolishly said that the R-line, which is the main line in rubies, was not suitable for optical maser action because you have to empty the ground state, but these ones would work.

Well, I tried it very crudely. I got a rod polished and silvered, but I only had a twenty-five joule flashlamp.

Actually it was a Strobotac for measuring rotational speeds, for motors or something like that. And that wasn't nearly enough, and nothing happened, so I just put it aside, which was foolish because Maiman then came along and showed that he could get more than half of the atoms excited and get laser action in ruby. That was the first laser. Then I helped Bob Collins and Don Nelson get their first ruby laser working: they copied more or less after what Maiman had published. We got that going around the beginning of July or so of 1960. We then set out to measure some of its properties, showing directionality and so on. We prepared our work, and that of Bond, Garrett, and Kaiser for a joint publication. We didn't use the word maser because we thought that *Physical Review Letters* had a ban on more maser articles, which they really weren't applying to optical masers.

Then I remember asking the boss, "Should I try the dark ruby?" He said, "You owe it to yourself." That was Al Clogston. I got a big flash lamp and the same ruby rod. In the article about it I thanked Walter Bond for polishing the ends. It really was just one end that had cracked, the other end was still the original. And it did work. I got it working in November of 1960. In planning to publish these results I decided, "Well, if *Physical Review Letters* doesn't want articles on optical masers, I'm just going to send it to *Physical Review*." That is not as prestigious, but it's a very respectable journal.

Our paper arrived on a day that they had a big snow storm, and a paper by [Irwin] Wieder and [Lynn R.] Sarles's also arrived on the same day. They reported that they had observed stimulated emission in dark ruby. I don't think they quite understood what the difference between stimulated emission and an optical maser was--I mean with the mirrors being essential. The editors felt that they had to treat them in the same way, and so our paper ended up in *Physical Review Letters* which we hadn't requested.

Riess: What do you think the politics behind all that was?

Schawlow: Politics? The editors are great people. Sam Goudsmit was a great scientist, and should have had a Nobel Prize. And Simon Pasternak was the associate editor. These are great physicists and they knew what they were doing, though they did make a mistake on Maiman's original paper. By that time they realized they had made that mistake and didn't want to make another. So ours appeared in *Physical Review Letters* at the same time as Wieder and Sarles's paper.

Riess: I guess I shouldn't have said politics. When I see the attention given to timing on all this I think, "Well, how is this important? This seems petty, this concern." And yet it's not at all, is it?

Schawlow: No, science is cumulative. It puts another building block, another brick, in the wall, so it's hard to tell. I think a lot of the stuff that gets into *Physical Review Letters* is not all that important, but they try to give things of general interest. It keeps getting fatter and fatter. Now it comes out every week.

Riess: But that requires that you read so much more, it seems like there's not that much gain.

Schawlow: Well, there of course are huge numbers of papers published in a lot of journals. But which one do people actually look at? I think *Physical Review Letters* is one that a lot of people do look at, even if they don't ever look at anything else.

Riess: Were Sam Goudsmit and Si Pasternak doing science as well as editing?

Schawlow: I think by that time Goudsmit was semi-retired. I heard him talk about it. He got famous back in 1924. He and Uhlenbeck realized that the fine structure in atomic spectra could be accounted for if you assumed that the electron had a spin. Well, the concept of electron spin has been extremely important ever since then.

This was a theoretical paper, but Sam--he really was an experimentalist at heart and he somehow got labelled as a theorist. He was at University of Michigan before the war and after the war he went to Brookhaven National Laboratory. He wanted to do experiments there, but they didn't want him to. He did one. He built a new kind of mass spectrograph, I think it was. But then they needed an editor for *Physical Review* and *Physical Review Letters*, and he took the job which is certainly a great service to the physics community.

It grew very rapidly. In fact when I first joined the American Physical Society and for quite a few years afterwards, there was just a letters section in *Physical Review*; and then later they decided to publish *Physical Review Letters* as a separate journal. Now the *Physical Review* has grown so huge that hardly any individuals subscribe to it anymore. Libraries have to. The cost is very high, hundreds and hundreds of dollars.

I used to subscribe to it, kept it up as long as I could, but it just got to be such a monstrous thing that I couldn't be bothered with it. So I gradually cut down to two sections, then one section, finally gave it up entirely. So anything published in *Physical Review* I just don't see unless somebody tells me about it.

Riess: And the sections are very specific?

Schawlow: There are five sections. There's one, I think it's atomic physics--atomic, molecular, and general physics. I forget what the others are: solid state, condensed matter, and nuclear. Particle physics. I think there's a theoretical one too.

Riess: In editing the letters, is there a lot of back and forth with the authors to be really clear about what they're writing?

Schawlow: Sometimes. Or sometimes they reject them. Sometimes the authors fight back and manage to persuade the editors to print their stuff after all. I know at least one case where the paper was rejected by several people, including me, as being not important enough to put in *Physical Review Letters*. But the author was a very determined guy and he got it in.

Inventing Stuff

Riess: Well, that all may be a footnote but it's interesting because publication and patent are both much more important than I ever would have thought in science.

Schawlow: I have little use for patents because I had nothing much but trouble from them. Of course I didn't get any money from the laser patent. Bell Labs had given me a dollar for all patent rights when I joined the company. But they did support me for seven years before I filed any patent applications.

However, just recently I was inducted into this National Inventors Hall of Fame, which is strictly based on patents. If I hadn't had that patent, I wouldn't have that. And now, in fact on Friday I have to go to San Jose to get the Ronald H. Brown American Innovator Award which comes from the Patent Office department of the Department of Commerce. This is a new award that they started last year. Again, just because I had that patent.

Riess: And you're the first recipient.

Schawlow: No, this is the second year. They are giving out seven this year. It was given in Washington on the fifteenth, but I was far too sick to go then. But the Commissioner of Patents, who is a Deputy Secretary of Commerce, or Assistant Secretary I guess, is giving a talk to the Patent Law Association in San Jose on Friday and asked me to go there and they'll present this thing to me.

Riess: Is Charles Townes a member of the National Inventors Hall of Fame?

Schawlow: Oh yes. He was in years and years ago.

Riess: But aren't you identified as an inventor more than he is?

Schawlow: No, oh heavens no. He invented the maser and co-invented the laser.

Riess: You have such an inventive turn of mind.

Schawlow: Certainly not more than Charlie, who is really a very great scientist. But as I told the people in Akron, we experimental physicists are always inventing stuff. We have to invent the apparatus that will do the measurements we want to do, but often there are things that are not worth patenting. I gave an example, that in 1975 Ted Hänsch and I published an article showing that it would be possible to cool atoms down to very, very low temperature--free atoms--by using laser light.

Well, we didn't do it at the time because we were interested in hydrogen and there still isn't a suitable laser for cooling it. I didn't even think to mention it in my Nobel lecture. But in the eighties a number of people, including particularly Steve Chu who's now with us at Stanford, but was then at Bell Labs, showed that this would work and you could get down to a fraction of a degree absolute. Then things were fortunate. It turns out there are other mechanisms that we hadn't thought of that make it even better than we thought. And now they get down to micro Kelvins.

Since then it's become possible to use these very slow cold atoms--they're still free, but they're not moving very fast--they can measure the acceleration of gravity more precisely than any other way, and that might be useful for prospecting. It's still too big an apparatus to take out in the field.

Also, they can make an atomic gyroscope, which is probably better than any other. So these are inventions that may be worth patenting, though they're probably twenty years away from being useful. We saw that it was so far away from being useful

for anything that there was no point in applying for a patent. If we had it would have expired by now. But it was an invention.

Riess: Yes. In order to apply for a patent you have to publish.

Schawlow: You don't have to publish it in a paper, but you have to have--in fact, one of the things I don't like about patents is that it's quite secret until the patent is issued, but you have to give them a description that will convince them that it will work, convince the patent examiner.

Ours was what they call a constructive reduction to practice: that is, we described in detail how you would do it, and so they issued the patent. More normally, they would like to have a working model that shows that the principles of the invention actually work.

Science Writers, Informing the Public

Riess: Interesting. In the sequence of things, there was a story of you being asked to talk to the *New York Times*. You were being asked for comment about Mirek Stevenson.

Schawlow: Oh yes. Stevenson had called me up the night before. Stevenson was a student of Townes's and had gone to work at IBM. It's quite an interesting story--I don't know whether I wrote that up before. He was very much interested in business; even as a graduate student he was making a lot of money in the stock market. He later started an investment fund. I don't know whether that's still going or not. At the time he had taken this job with IBM and was working with Peter Sorokin, who was a very brilliant experimental physicist and was a student of Bloembergen's at Harvard.

I guess they heard--I'm not sure if it was before Maiman did his stuff or after. I think it's probably before Maiman published his attainment of laser action. But Stevenson felt they should do this in a businesslike way and buy everything possible, don't take time to build it yourself. So he searched and found the biggest flash lamp on the market and he also found that they could buy the crystals that they wanted from a crystal growing company. So they quickly got laser action in divalent samarium and trivalent uranium.

I think it was Stevenson, one of them called me up the night before it was officially announced. And I did find out

that it was trivalent uranium and divalent samarium. So when the reporter called and asked me what I thought of, I said that it was good stuff and told him what it was so they got the story straight.

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Riess: That must be a challenge, the public need to know, and dealing with science writers and how to get things clear with them. Has science writing improved over the years?

Schawlow: Well, there have always been some good ones. I think Lawrence of the *New York Times* was very good, very careful. The science writers were not bad. I think it's the regular reporters that have to deal with a story that really get things garbled sometimes. Even that has improved, I think. But I sort of came to the conclusion that whenever I saw some story in the newspaper about which I knew the facts, there was always something wrong with it.

I really have to admire how science writers can jump from physics to biology to astronomy and everything. Of course, they tend to always want to fit it into a pattern. With lasers it's either a death ray or a cure for cancer or both. That's indeed the way it turned out, no matter what you told them, pretty much.

Riess: You mean it's sort of the human interest.

Schawlow: Well, yes, that's what they want. And these were old ideas. Of course, the death ray idea is much older than actual lasers. Buck Rogers in the 1930s comics strips and H.G. Wells' *War of the Worlds*--the martians had a sword of heat. Even back to Archimedes supposedly burning the sails of enemy ships with reflected sunlight. All these things. So this is an old idea. And as soon there were any lasers, that's what they jumped on, although the lasers that we had then were very primitive.

I remember calculating that if you could deliver one joule, that's one watt second of energy, once a second, you could completely vaporize a two hundred pound man--but he'd have to stand there for two years.

Riess: Now that's an image! [chuckles]

Schawlow: I didn't think much of them as weapons, and in fact they're still not really usable as weapons. They've got some giant lasers that will fry things, but what they really want to do is melt missiles at five thousand miles away, and that takes an

awful lot of power. It could be countered by putting a little more shielding or more decoys.

Riess: When the newspaper calls, do you view it as an opportunity to clarify things or do you greet it with dread?

Schawlow: I don't greet it with dread. I try to give them the story as I see it, and I don't worry too much about how it comes out.

Post-Laser Atmosphere at Bell Labs

Riess: Another event. What you refer to as "the first public demonstration of an operating laser" was at the Nerem electronics meeting.[1]

Schawlow: The Nerem meeting occurred in the fall of 1960. By that time we had a laser, a big clumsy thing.

As soon as lasers came on the scene, the atmosphere changed at Bell Labs. First of all, there were a lot of people who wanted to talk to me whereas before in superconductivity I was pretty much all alone. But also there was some jealousy and secrecy. People weren't telling things that they were doing, even within the laboratory.

I had been asked months before to give this talk at the Nerem meeting and I had agreed. But I had to circulate an abstract for approval. One of the other people at Bell Labs that had been involved in that combined paper about the properties of lasers said that all the authors should be consulted on this thing. Well, I got rather angry. This was something I had done myself and I could talk about stuff that they had published and give them some credit, but still the original thing was mine.

I really was quite angry and I complained to Clogston. He said, "Let me take care of this." I heard no more about it.

They'd had a press conference from Bell Labs before that, and again I had this feeling of jealousy. In fact, they arranged it so that I wasn't the first speaker. There were all six authors of that paper on the properties of lasers and I think they put me in third or fourth place there. I was

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[1] p. 143 "From Maser to Laser," Arthur L. Schawlow, in *Impact of Basic Research on Technology*, Plenum Press, 1973.

supposed to explain the principles of the thing and they tried to deemphasize me. Well, the newspaper wasn't fooled. [chuckle] But that's why I really began to think of leaving Bell Labs.

Riess: Was Bell Labs trying to recast the invention in terms of being a kind of communications breakthrough?

Schawlow: They did want to emphasize that all right. And these people, they'd all done something. But they wanted to feel their part was just as important as anybody else's--which it wasn't. Mine had come first, and the whole thing wouldn't have been thought of if we hadn't done what we did.

Riess: It's hard, as I sit here, to imagine you getting very angry.

Schawlow: [laughs] I don't very often. But that really annoyed me. I said, "Well, if they want, I'll just talk about the theory and not about any of these results."

Riess: In fact, you did have contacts with the newspapers and you could have gone public and really embarrassed them, I suppose.

Schawlow: Well, I'm not that kind.

Gordon Gould, and the Competitive Drive

Schawlow: I always feel that it's better not to attack others but just to say my piece. Now of course, one thing that I really hate to mention is Gordon Gould. He's been a real thorn in the flesh. He got elected to this Inventors Hall of Fame years ago, but his forte was patents. Patent lawyers control this thing pretty much.

He was a graduate student of Kusch's. He'd never finished his Ph.D. He was older than I am, a year older. But somehow he got wind of what we were doing and he started writing stuff in his notebook. Oh, some months after our patent was filed, he filed a patent application--nearly a year after. There was interference, and fortunately both the patent office and later the courts decided that he hadn't shown conception of the ideas in sufficient detail to be acceptable. Also he hadn't shown diligence in reduction to practice. But his lawyers filed--

Riess: Diligence in reduction--

Schawlow: --to practice, yes.

Riess: What is that expression?

Schawlow: Well, it means either writing a detailed description that a person "skilled in the art" could duplicate, or actually making one.

He went to work for TRG, and I think his agreement was that anything he had done before then was his, but anything after that was theirs. But he kept on adding to his notes for his own personal patent application. To give you an idea of how dirty they were, two things: I can't say how much was his and how much was his lawyers and backers, but the company that was sponsoring his patent stuff, after TRG, a company called Refac, I think, and then later Patlex, they got into trouble because they were doing insider trading when they heard that his patent was going to be issued. They bought some of their own stock.

But worse than that, the patent office decided that he hadn't shown conception of the idea and also hadn't shown diligence in reducing to practice. Then they took it to court--at that time it was being sponsored by Control Data, which had bought TRG, and they had plenty of money for good lawyers. But the court--I think there were three judges, and they ruled unanimously that he had not shown conception of the idea. Two of the judges ruled that he hadn't shown diligence in reducing to practice. The other one said, "Well, since he hadn't shown the conception, we don't need to rule on that."

Then they put out press releases that this had only been rejected on the narrow grounds of insufficient diligence in reducing to practice, which was just a plain lie.

Later, to give you an idea of what they did that was really rotten, they went after a lot of little companies. They were very smart at managing. They bought off Bell Labs and General Motors, I think, who could have put up a real fight, by giving them a cheap license.

Riess: You mean Control Data did?

Schawlow: No, that was later. It was Patlex or Refac. It was after the Control Data time. I think Control Data just gave up on it after that point.

They went after a little company in the San Francisco area and this guy was too poor to hire a good lawyers to defend it. But in court, the lawyer for Gould's side got up and said, "You wonder why this great inventor hasn't received the recognition he deserves. Well, his professor," meaning Charles Townes, "had witnessed his notebook of an idea for optically pumped maser and then later put it into his own papers."

This was really a disgraceful lie. Because, first of all, Charlie Townes had this particular--it was just for an optically pumped maser, not a microwave maser. And Charlie had this idea in his notebook several months before that. And this had come out in earlier patent litigation, so it was public record and yet they lied and made it sound as if he had stolen some of Gould's ideas. And you know, Charlie is the most honorable man you ever met. But that's the kind of dirty playing that they did.

Then they scrambled around and looked at things that maybe we hadn't quite specifically mentioned--we didn't really try to think of things around. They threw out his patent application, but the court forced them to reinstate it. They had accused him of lack of candor, meaning he'd said different things in different cases.

He got a patent finally on maser amplifiers, said that we had only shown an oscillator. Well, you can't make an oscillator without having an amplifier. An amplifier provides the gain and then you have some kind of feedback. So we certainly had amplifiers. But then they tried to collect royalties on *any* laser. They said, "Well, it has an amplifier and we have a patent on the amplifier."

Riess: It an extraordinary story because it's so singular. You don't hear stories of this kind of greed and duplicity.

Schawlow: Well, this outfit had apparently done something similar in ultrasonic testing.

Anyway, he did get a patent on gas lasers and I don't know just what he had that based on. They collected a lot of royalties on that.

Riess: But do you put it to the company, Patlex, or is it Gould's hysterical approach?

Schawlow: Hysterical is not the word. But yes, I think he had a lot to do with it. Although the lawyers, at one point, boasted that Gould had invented the laser but they had invented the patent. And his patents were issued many years after. Of course, that was good because by that time there was a lot of business to collect from. But he just maneuvered and made it not worthwhile for anybody to fight it even though it really could have been fought.

Riess: No, hysterical is not the word.

Schawlow: Devious.

Riess: Yes. Science is relatively free of that sort of thing.

Schawlow: This wasn't science. We're talking about money and inventions and technology. I noticed a big difference as soon as this thing got to be something that somebody might make money on. Oh, I hate to put this stuff in print.

Riess: I would have brought Gould up because you write that TRG invited you to give a talk there. You say, "...we exchanged ideas about work on spectroscopy of rare earth ions of the sort that might be useful for optical masers."[1]

Schawlow: Yes. Then they were still fairly open.

Also, in 1959 there was a conference that Peter Franken sponsored on optical pumping at Ann Arbor. He called me up just a week before and wanted me to come preside at a session and give a talk. I didn't have time to get clearance from Bell Labs, so I spoke rather obliquely.

Again, Gould was there, and he said, "We have six different kinds of materials and a number of different structures, but unfortunately this is all classified. I can't talk about most of it." [laughter]

For the Shawanga Lodge conference in September 1959 Charlie said, "Well, let's not fight in front of the Russians. Try and say something nice about Gould." I did mention his idea of using a scatterer instead of a mirror, which is okay but not very important.

Riess: Was this the time that the Russians that shared the prize with Townes were here?

Schawlow: Yes, they came to this conference in '59, the first quantum electronics conference. That's the first time I met them.

But at this one in Ann Arbor I did tell about the ion pairs and said they'd be useful for various kinds of masers without indicating that I meant optical masers. Maiman was there and I think he got some ideas from that. But I couldn't resist--after Gould gave his talk, I said, "Well, your laser really is more of an oscillator than an amplifier, so we should change the "a" to an "o" in your entry into the optical maser race. [laughter]

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[1]*ibid*, p. 134

Oh, I'd never seen anybody like that and I hope I don't. But certainly in science that doesn't happen. Particularly high energy physics where there are rather unique sharply defined problems, there's some very dirty work goes on trying to get publication before the other guy does. They have to make sure enough that they have the results, but not wait too long or other people will do it.

This book, *Nobel Dreams*, about Carlo Rubbia, who did get a Nobel Prize--when they were working on this thing that got them the Nobel Prize, there was another group at CERN working on the same thing. He met the leader of this other group and said, "Well, we must be careful not to publish prematurely. Make sure we really have a result." Meanwhile he had a courier taking the manuscript to *Physics Letters* in Amsterdam. But I've never had anything like that.

Riess: It's one thing if it has to do with real greed and money, I suppose that's not okay. But if it has to do with academic competitiveness, you're all in an academic world where it's kind of dog eat dog?

Schawlow: Well, not for me, fortunately. But I think in high energy physics it is that way. And in his case he got a Nobel Prize and the other guy didn't. And that's very valuable, even apart from the money involved. He gets all sorts of prestige.

In my world it's not that way. I have always said, "If anybody wants to do anything that I'm thinking of, okay. I have a lot more ideas that people don't think are worth following up." As I've told you before, I'm really not a competitive person at all. I'll go out of my way to avoid competition. So it's a different world, different people.

Riess: Well, then the university is different from Bell Labs.

Schawlow: Bell Labs usually was not that way. It was only when people smelled something that was really important that they began to fight for it. Mostly they were very open and friendly.

In fact, what it took me a long while to realize at Bell Labs is that they wanted to cover a lot of different topics, to keep an eye--the purpose of their research, they claimed, was just so that they would have a good view of all the frontiers of any science that was related to their technology. So they'd have just one or two people working on each area, so there were a lot of lonely people around there. If you wanted some help on something, you'd go to them, and they could drop what they were doing and help you. It took me a long time to find that,

about five years, but certainly that's one way that Bell Labs works so well.

Typically, a person gets an idea: he goes to crystal-grower A, gets some crystals; goes to somebody who has equipment, B and C; and then takes the results to theorist D. And you come up with a paper with a lot of names on it. They think, "Oh, Bell Labs has put a big group on that," whereas by that time they're probably not even speaking to each other. So it was a very good environment that way; it didn't seem competitive at all.

They tried to make it more competitive. About the time I was leaving they started clearly rating people in octiles, giving bigger raises. When I was there people worked hard, but they didn't work long. There was not a lot of this all-night stuff which I gather there was in later years.

As you'll see in that movie [video], I really felt--there were times I really desperately wanted to get the answers to things, really wanted to know. But I had to learn to be patient. When you have to work through students--and some of them are awfully slow--you try and help them, but it just didn't happen very fast.

Riess: So you become more teacher than physicist.

Schawlow: Yes. Well, more than hands-on. I didn't do very many experiments myself, at all, which is probably a good thing because I am quite clumsy.

IV THE EARLY YEARS AT STANFORD, AND FAMILY

Riess: You were at Stanford in September 1961. In the negotiations for the Stanford job, did you have your NASA support? How did that work?

Schawlow: No, I didn't. It was later. [laughs] Actually I was a little naive. They did have quite a lot of money. They had Joint Service contracts here, and I just kind of assumed that they would take care of me and went ahead and ordered stuff.

I got the NASA contract after I'd been here a few months, I've forgotten just how long, and they supported me until they ran into hard times in the late sixties. And I guess I had to admit that my stuff wasn't very closely related to their missions. Fortunately, at that time NSF was growing and I managed to get onto NSF. I had some support from the Navy all along, and even a small grant from the Army Research Office. But they, again, ran into financial difficulties and dropped that.

Riess: How did you get the NASA money? Did you know people there?

Schawlow: No I didn't. I don't remember, tell the truth. I didn't know anybody there. But--gosh, I can't remember, somebody must have suggested that I apply to NASA. I guess I talked with one of their program officers.

I'm pretty naive. I didn't know much about how one got money for research, but as I say, they had this Joint Services contract. Money was still pretty plentiful then. Ever since then it's been getting harder and harder to get.

The Department, Plain and Applied

Riess: How about describing the department when you got here, who the other folks were and how you fit into it all.

Schawlow: You talk about negotiations--about the only two things that I had to make clear were, one, that I wouldn't come unless I got a full professorship. I was forty by that time and I didn't want to worry about having to get promoted. And there was no problem about that, they said okay.

The other thing was that at that time the department had in it a number of professors who had the title of "professor of applied physics and electrical engineering." Their salaries were split between the university and their research contracts. The regular physics professors all had insisted over the dead bodies of the administration that they had to be paid full-time, and they were not going to charge any of their salaries to contracts. Because they foresaw what happened later, that when money got scarce some people lost their contracts, and the university would have to find some way to pick up their salaries.

I said I didn't want to be in applied physics and engineering, I wanted to be just plain physicist, and there was no problem with that.

It was a nice little department really, the smallest of the good physics departments, I think, by a lot, I think they had maybe fifteen permanent members, about five or so assistant professors. But they were a brilliant group. Leonard Schiff was the chairman, and had been for years. He was a theoretical physicist, but he was a very good chairman and very democratic. He kept things going nicely and was good at raising money for his own research.

They had raised a lot of money. They had a lot of money from the royalties from the klystron patents. The klystron had been invented at Stanford by the Varian brothers, and they had gotten money from the Varian Associates. Both the Varians had died, but Mrs. Russell Varian gave money, as did the National Science Foundation. They were able to put up a physics building when the physics department had raised all the money.

Riess: The Varians taught here?

Schawlow: No.

Russell had gotten a degree at Stanford. I don't know whether he had a master's degree or not. Russell apparently was brilliant, but as Leonard Schiff once described, he thought in a way in which logic was only a special case. [laughter] They had given him and his brother Sigurd, who was an airplane pilot--they were trying to do something to prevent airplane accidents and one of them had the idea for this klystron tube, which is a way of generating microwaves--and they gave them a little space in the physics building, which was the old physics corner of the quadrangle. I don't know if they gave them money or not, certainly not much. There they built the first klystron.

During the war klystrons became important and the Varians and several others who were later part of the applied physics department--Ed Ginzton and Marvin Chodorow--they went to the Sperry Gyroscope Company--I guess Chodorow hadn't been at Stanford before then--and worked on klystrons during the war. After the war, the Varians started this Varian Associates to make klystrons.

Riess: Was there any question that Stanford owned this patent?

Schawlow: I don't know the details, but these patents were worth several million dollars. They'd been obtained by Chodorow, Ginzton, and their associates.

There are a number of threads here I have to tie up. Shortly after I came I think Leonard Schiff got tired of managing applied physics which was funded differently.

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Schawlow: There was a lot of pressure to add more positions in applied physics because it was cheap. Only a quarter of it came from the School of the Humanities and Sciences, and the rest came from engineering and government contracts. He really didn't want to build up too much in that, have it overbalance the department, so he pushed them into starting a separate applied physics department. It has done very well, it's a very strong department. Actually it's hard to tell, some of the things they do there are quite applied; some of the things could very well be pure physics.

Riess: So there's an applied physics department plus a physics department.

Schawlow: Yes. And the physics department does all the undergraduate teaching, which I think is in a way not so good. It meant that we all had to do a lot of undergraduate teaching, whereas they

could just teach graduate courses in their specialties. I taught very few graduate courses and I could've learned a lot more if I had had more time to work on that sort of thing. But after a while they made the point that these klystron royalties really had come from the people in the applied physics and not from the physics people, so we had to give up our interest in them. By that time they were nearly expiring.

Felix Bloch, Robert Hofstadter, and Bill Fairbank

Schawlow: Now, coming back to the physics department, the outstanding person in the department at that time was Felix Bloch, who had made a brilliant thesis in 1928 in which he set forth the quantum mechanical understanding of how metals conduct electricity. This really led to all the work on semiconductors, which in turn led to things like transistors and integrated circuits.

He had come as a refugee in 1933. He was Swiss, but he was Jewish, and just felt it was better to get to a safer place. He said he was visiting in Copenhagen, at Niels Bohr's institute, when he got a cable from somebody named David Webster offering him an assistant professorship at Stanford. Apparently, this came up because I think the Rockefeller Foundation had made up lists of brilliant European physicists who might be refugees. He had never heard of Stanford and he asked various people about it. Some of them had been there. I think it was Pauli who said, "Jah, it was on the West Coast and he had been there, and there was another university nearby and they steal each other's ax." [chuckles]

Well, he came there, and Enrico Fermi, who was also both a theorist and experimentalist, said to him, "You should do experiments. They're fun." So he teamed up with Luis Alvarez and they made a measurement of the magnetic moment of the neutron, which was a brilliant experiment, and that was in the late thirties. I think they did the actual experiment at Berkeley, but I think he prepared some of the equipment.

Then after the war he started to look for nuclear magnetic resonance, or nuclear magnetic induction was the way he did it, and they did discover it in '46, I think, just about the same time Ed Purcell and his group at Harvard also discovered it. They shared the Nobel Prize in 1952, I believe.

It became apparent--. Well, I'll tell you a little more. Bloch told me they used a big permanent magnet in their early

experiments, and they had to make the magnetic field very uniform, so they put little iron shims on the face at various places to even out the irregularities in the magnetic field.

He said they measured ethyl alcohol, CH_3OH, and that he found that the relaxation time was quite long. That meant that the spectral lines should be very sharp and the magnet was too crude to see that. So he said, "I just want to see a line that sharp," and he kept pushing on his people to shim the magnet better and make it more uniform. When they did they not only saw a sharp line, but there were several lines. This was a chemical shift due to the hydrogen being in different places--some of them in the CH_3 would be one kind and the OH would be a different one.

So this was the beginning of chemical shifts, and it soon became apparent that magnetic resonance could be important for chemistry. Varian Associates therefore decided they would manufacture magnetic resonance equipment commercially. They did and they sold a lot of them.

Riess: That's a story about the beginning of industry down here.

Schawlow: Of course, [Frederick] Terman had pushed various people into starting companies, Hewlett-Packard particularly, and had gotten Stanford to set aside this land for the Stanford Industrial Park.

Anyway, Bloch was there, back to doing theoretical work; after he got his Nobel Prize he gave up experimenting.

Also Robert Hofstadter, who had done brilliant work--. When I came out here for interviews in the spring of 1961--I was very impressed by what he'd done and he was very friendly--he invited me to go salmon fishing with him on one of these boats out of San Francisco. Fortunately he then got the Nobel Prize and I never heard any more about that. [laughter] I'm sure I would have been very seasick. He was a nice guy, and unfortunately died a few years ago.

Then Bill Fairbank had just discovered that magnetic flux in a superconducting ring was quantized. And that was a major discovery.

Riess: Now say that again.

Schawlow: If you have a ring of superconductor, the current will keep going forever and it'll hold whatever magnetic flux was in there. But he found that it came in quanta. The value was hc/2e: people had predicted this might happen, but the "2" they didn't predict. This was one of the things that showed that

the electrons in a superconductor are paired, they act as pairs. This later helped lead to the theory by [J.] Bardeen, [L.] Cooper, and [J.R.] Schrieffer.

Well, Bill should have had a Nobel Prize for that, but also apparently he delayed in publishing to make sure, and a German named Näbauer published similar results about the same time. Then Näbauer died, and I think because Näbauer wasn't alive to get the prize, I think that may have had something to do with the fact that he [Bill] didn't get it. He never did.

Fairbank then went on to do some grandiose experiments. People said about him that he would find an experiment where they needed to improve sensitivity by ten orders of magnitude to do it and he'd get nine. [chuckles] Some of them didn't work, but he did spark the construction of the superconducting accelerator, and also the search for a superconducting gyroscope to test general relativity--which is still going on, they still haven't flown it. It's supposed to have a space flight sometime in the next few years, but the people have been working on it some of them for thirty years.

[knock on door, pause]

Riess: We were talking about the department--.

Schawlow: Felix Bloch and Näbauer and Fairbank.

It was a wonderful little department.

Riess: What about Panofsky? You didn't mention him.

Schawlow: No. Panofsky had just decided that he would leave the department and head up the Stanford Linear Accelerator Center. He--well, he tried to pull a few fast things on us. He wanted to have professors there and said they'd just be research professors. Then he started demanding they should be allowed to teach.

And SLAC

Schawlow: There was a good bit of friction between the physics department and the linear accelerator center. Finally, they managed to get President [Wallace] Sterling to assign teaching to the physics department and SLAC professors could teach by invitation, and we have always invited a few to teach.

Riess: He wanted to get his staff on salaries.

Schawlow: Yes, partly that, instead of just being paid by the contract.

Well, they have a large number of professors. We were afraid we'd be swamped if they could do everything--there'd been a lot of fighting in the years just before I came as to whether they would add a lot of people in the physics department or not. Bloch particularly didn't want to have the thing overbalanced by high energy physics.

Riess: So they have professorial rank and are only doing experimental work?

Schawlow: Or theoretical, yes. There are about twenty of them, something like that--at least as many as there are in the physics department. They've done well. They've got three Nobel Prizes, so it's been a success.

Again, I had the feeling that, as with the applied physics people, they just wanted to teach the advanced courses and make us teach the freshman stuff. And really, that's the way it worked out, actually, they did teach mostly advanced courses. That's what I was afraid of when I came, but I just sort of got reconciled to it. It wasn't such a good thing.

We had to establish that the physics department's duty was teaching. If we didn't have this duty, we wouldn't get any staff. These other people are cheap, and the university would rather hire them than get another person in physics. But if the physics department has courses that have to be taught then they have to give us some staffing. The physics department didn't grow through the sixties at all, whereas in many universities it expanded enormously.

Riess: Stanford, in a way, really has three departments of physics then?

Schawlow: Some people put it that way and say we should somehow rationalize them. But yes, there are three places where physics is being done. Stanford is wonderfully disorderly. There're good physicists in electrical engineering and material science and so on, and students can do theses with them. Or in chemistry--good physical chemists are quite good at physics. So it's not hard for a student in physics to get a Ph.D. in physics supervised by a professor from another department.

Riess: And that's not good?

Schawlow: No, that's all right. It means, of course, that we again have to do the preparatory work and support them the first year or so.

To tell the truth, it used to be at first that the microwave lab was a place that we could dump the students who were not awfully good. They might be good at experiments but they weren't very strong theoretically. Now that isn't true anymore. They admit their own students and they get very good students, but sometimes some of them come over and do theses in physics. I had a student who was in electrical engineering do a thesis under me. So going back and forth is not bad. And physics has so much affected technology in the last generation or so that it's very reasonable that these places had to have their own physicists.

Riess: What about Sidney Drell?

Schawlow: Drell had been a professor at Stanford. He went on a sabbatical when I came, and when he came back he just decided he was going to go to SLAC so he never really did quite come back. It's too bad. He was a good teacher as well as a very good theoretical physicist. But he sort of--well, he just sort of treated the department with contempt. What really mattered was just SLAC. So I had very little to do with them.

No, I sort of felt that I had come a long way, going across the continent where I'd never been much, in order that I could do research and teaching. While these people just sort of had it easy. They could do all the research they felt like doing, and teach when they felt like it.

Riess: Others who were in your position must have felt somewhat the same way. It must have been a--

Schawlow: I think so, yes.

Riess: --gnawing debate all the time.

Schawlow: Yes, it was rather unpleasant.

George Pake had been here before I came, and he wanted the physics department to start splitting salaries and build up a big solid state group to balance the high energy physics. But people like Bloch prevented that. He [Pake] left then. He became provost of Washington University, where he had been a professor before. That's Washington University in St. Louis. He later was the first head of the Xerox-Palo Alto Research Center.

Fortunately those fights were sort of coming to an end by the time I arrived, but there were still problems getting SLAC in its proper place. I went over and saw Panofsky when I came. It was clear that he felt he had the backing of Terman who was the provost, and that they were just going to do whatever they wanted to do. He was not at all interested in trying to find a mutually agreeable solution.

Riess: What did you go over to propose?

Schawlow: Well, to see what could be worked out, you know. As the status of people at SLAC and--. Well, it was apparent by then that he had gotten permission to have professors there. He was claiming that well, professors are professors and he can teach when he wants to teach.

Riess: Oh I thought perhaps you were going over to propose something that was of an experimental nature.

Schawlow: No. Just try to find a better relationship.

Riess: Terman, as provost, was more involved than Sterling?

Schawlow: Well, yes.

Riess: Adjudicating all this.

Schawlow: Well, we had to go over his head and go to Sterling finally to get it settled. But Terman was an empire builder, you know, and he had gotten a lot of government-sponsored research and so on. He was pushing expansion of that area. He was an expansionist.

Riess: He wasn't a physicist, was he?

Schawlow: No, he was an electrical engineer. But he was provost, which is sort of the chief academic officer.

Riess: Provost for the entire university not just a school.

Schawlow: Yes, yes, the entire university. But Sterling was president. He was a very good president. He presided over the building up of Stanford and raising standards. I think it was under his presidency that it really became a great university, although it had always had some respectability.

Riess: Then there was the other university on the other side of the bay--

Schawlow: Oh, I've heard there was one there. [laughing]

Riess: Was there always the threat that one might defect to the other camp?

Schawlow: No, there was I think a gentleman's agreement that they didn't raid each other. I don't think that's in effect anymore, but it really happens very rarely. No, but there are other places in the country where one could defect.

Riess: If you went to visit Panofsky in the beginning, it's clear that you saw what the situation was very early. But you decided that you could live with it.

Schawlow: Well, we fought some to make sure that teaching was our business, at least, and that they didn't have authority to teach separately. I was on that side but it was a pretty serious fight. I understood the issues very clearly. Bloch and I were in complete agreement at that point.

The Big Picture: Teaching, Labs, Students, Postdocs

Riess: But the fight to teach some of the upper division courses?

Schawlow: Well, we really earned our living by teaching the introductory courses, because those were the big ones with hundreds of students in them, and that had to be done. Somebody had to do it. We had a small department.

In fact we had a very good tradition--started or continued by Leonard Schiff--that the introductory courses were taught by senior faculty. They asked me after I'd been there a few months if I would teach Physics 21, which was Mechanics and Heat for pre-medical students, without calculus. It was really no more than a decent high school physics course. Well, I might have been insulted except that Bill Fairbank was teaching the second quarter of the Physics 20 series and Hofstadter was teaching the third. With that company, it's an honor to teach.

Riess: That's interesting. Stanford could certainly say that our best minds are teaching our students.

Schawlow: Certainly in physics. They had trouble in other departments. I heard complaints that economics was having a lot of part-time teachers teach the introductory courses while the professors were all busy consulting.

Riess: You say that the loss to you in not teaching the upper division or graduate classes is that you don't get a chance to get back into that material?

Schawlow: That's right. I could have learned a lot of stuff. I would have been forced to learn more advanced topics which I never did learn.

Riess: Why are they things you didn't know?

Schawlow: Oh, there's a lot I don't know. I'm not very good at mathematical stuff, as a physicist I'm really not awfully good. Compared to the man on the street I'm pretty good, but I just hadn't studied a lot of the advanced theory. And of course, stuff was coming out so fast in the laser field, that if I had been teaching it I could have learned more things, perhaps gotten ideas from it.

I did teach a one quarter course in spectroscopy and quantum electronics in alternate years for a few years. Then after Ted Hänsch came, he sort of took that over.

Riess: I should think that would be one of the reasons they wanted you, was just *because* of this.

Schawlow: Yes, you'd think so, but it didn't work out that way. But I did have a lot of graduate students. I built up very quickly. I remember telling one of them sometime that I had ten graduate students and never given a Ph.D. But then they started coming out the pipeline and my students mostly finished degrees in reasonable time. There were one or two that were hard to drag through.

[pause]

Schawlow: I had a lot of ideas, and I couldn't do them with my own two hands. I wanted students to work on some of these ideas and that worked pretty well at Stanford.

Well now, you asked about how I raised money [referring to conversation during pause]. I was never very aggressive about that. I guess I would hear that a certain agency had some money and would take applications, and I talked with somebody there and applied for it.

But I was careful not to get overcommitted. I didn't want to commit to doing something I didn't want to do. I would mostly only take money that left me pretty free to do whatever I wanted, because generally whatever I proposed didn't work out, usually there was something wrong with it. And it's the things that I hadn't proposed that worked--you get an idea and

say, "Oh hey, let's try this." Really, that's the way it is with me. I'm not a good planner.

Riess: What did you propose to NASA?

Schawlow: Well, I think just general work on spectroscopy and quantum electronics.

Riess: You weren't proposing or developing the laser in six different directions.

Schawlow: No, no. I didn't do very much on lasers then, it was mostly on materials related to laser materials, trying to get at the spectroscopy.

I remember one of the things that we did early. We had observed these pair lines of chromium in aluminum oxide. They had been known fifty years before, but not understood at all what they were. But since we knew they were pairs, and you could look at the crystal structure and you could see you could have pairs in different directions--a pair along the symmetry axis, or off in the side direction, or another side direction, and we wanted to find out which lines belonged to which pairs.

So I had a student, Linn Mollenauer, working at applying stress to the crystals in different directions, seeing how the lines shift and which ones shifted the most. If you press directly along the axis of that pair it would shift more than if it was perpendicular to it. So he did some work of that kind, which was, as I say, related to laser materials but not really on them.

Riess: These were things that had been in your mind when you had been at Bell Labs?

Schawlow: Yes. And others, of course, came up as we worked.

Riess: What does it mean to put together a lab? What kind of a space did you have?

Schawlow: I was very, very fortunate. At first I was over in what was then called the Microwave laboratory. It's now called the Ginzton Laboratory. I just had a couple of rooms there. But then the new physics building opened and I think I had ten rooms, I had most of the second floor, and so I was able to expand fairly rapidly. There was enough money to buy basic equipment, but I never really had quite enough money.

I had very few postdocs because I would rather spend the money on students and equipment and only sort of accidentally got postdocs if somebody came along. Actually I turned down one very good man in the late sixties, I think '68, a man named Richard Slusher. He was getting his Ph.D. at Berkeley and he had an NSF postdoctoral fellowship. I told him I didn't think we had a very good place, we were very short of money and space. So he went to Bell Labs and has done very well there.

Riess: I thought postdocs did come with their own money, so why would you ever turn one down?

Schawlow: Mostly not, mostly not. Mostly you have to pay them, and you have to pay them more than you pay students. So I didn't get many. Mostly you pay them from contracts, and I had fewer than most programs do.

In 1970 I got this letter from Peter Toschek in Germany asking if I could take a young man who had done his thesis with him. Actually, they were sort of partners because Toschek was just learning about lasers then too. Well, I wrote back, said that I didn't have any money. He said, "Would you take him if he got a NATO fellowship?"

I said, rather reluctantly, "Oh, all right." And it turned out to be Ted Hänsch, who was absolutely brilliant. We saw that quickly and managed to find another hundred dollars a month for him somehow. Fortunately, about then we got an equipment grant from NSF and we were able to get some new equipment. Wonderful things happened from then on.

But in the sixties there was one assistant professor, Peter Scott, that I sort of inherited. Pake had hired him. He'd gone off to England, to Oxford, for a postdoctoral year, but they had this commitment to give him an assistant professorship. He worked with our group, but he did teach some too. He is now a professor at the University of California, Santa Cruz.

Bill Yen, from Washington University, worked with us about then. Yen is now a professor at the University of Georgia. Warren Moos, who was here at about the same time as research associate and acting assistant professor, is a professor at Johns Hopkins University.

We did have another visiting scientist, Serge Haroche, who came in 1972. He came from the Ecole Normale in Paris and was very brilliant. He did beautiful physics research here, and also after he returned to Paris where he is now the head of the physics department of the Ecole Normale.

In 1977 James Lawler came from the University of Wisconsin. He also had good independent ideas and the ability to carry them out. He returned to Wisconsin and is a professor there. He has received several awards for his research, particularly in applying laser spectroscopic methods to study gas plasmas.

Then in 1981, when I was president of the American Physical Society, the society provided half support for a postdoctoral researcher. Steven Rand, who had already been a postdoctoral researcher at the IBM Laboratory in San Jose, came and worked on spectroscopy of ions in crystals with laser excitation. He was enormously helpful, particularly in organizing the agenda for the November meeting of the American Physical Society when I was so occupied getting ready for the Nobel prize activities. He is now a professor at the University of Michigan.

Riess: You mentioned having a couple of rooms. The experiments you were doing could be done in a regular room?

Schawlow: That's right. We used one of them for kind of a workshop and others for labwork. After Hänsch came, he gradually took over more and more of the space.

Riess: Our discussion of Stanford has started out with a run-down of how it's divided up and all of that. And you said you went to visit Panofsky. Does that mean that early in your time at Stanford you got involved in administrative issues?

Schawlow: No, not very early, but these things were decided by the department and the department had to be unanimous on them. I didn't spend a lot of time on it, but I took positions on these issues. I just went to talk with him [Panofsky] once, didn't try again. In 1966, Leonard Schiff, after eighteen years, decided that he'd had all he could take as chairman-- I think the struggle to keep SLAC in its place had worn him down.

SLAC did do some things. They announced that Drell was going to give a course in general relativity, I think, which he was very well qualified to do, but still this was sort of an end run around the physics department.

And Administration: Department Chair, 1966-1970

Riess: You were saying that in 1966 Leonard Schiff decided he'd had enough of being chairman.

Schawlow: And I was foolish enough to accept the chairmanship. I remember when I started I realized that practically every piece of paper that came to me was routine, but I didn't know the routines.

I was chairman for four years, and I was very glad to get out of that. It was a lot of work. You had to know what people to consult on various issues and make sure you *did* spend time talking with them. The experimentalists were much concerned about the workshop. The theorists didn't care at all, but they cared a lot about the library. So you had to spend time talking with people.

I don't think I was a very good chairman. I wasn't very aggressive to try and expand, which we probably could have done. But still, I did keep the lid on, and things were reasonably peaceful when I was chairman.

Riess: Did the department have a strong tradition of journal groups and meetings and symposia? Does the chairman keep alive? Is that part of the job?

Schawlow: Well, you have to appoint committees. We would have a colloquium committee, for instance, that would be responsible for getting speakers for the colloquia. They'd have a lot of other committees to do things. The chairman had to appoint the committees and had to recommend raises for people, which was difficult because there never was as much money as you'd like to have. There I consulted with Leonard Schiff and we sort of went over them together.

Riess: And sabbaticals, you had to decide on that as well?

Schawlow: Yes, but there usually wasn't too much trouble with that. People usually had organizations that they could turn to for funding such as NSF or other laboratories, and they had postdoc's and so on to help cover when they were away. I don't remember sabbaticals being a problem.

Riess: Were there any interesting people brought onto the faculty when you were chairman?

Schawlow: I don't think so. As I say, I don't think I was very good at going out aggressively and getting people. But we had no encouragement to expand, and we had a good, strong faculty so I sort of let it ride as it was. We did make a few offers. We were trying to get an outstanding theorist, but the presence, proximity of SLAC was a detriment.

Riess: Because that's where they wanted to go.

Schawlow: Either they wanted to go there or they were afraid that they would be drawn into the discussions at SLAC. It just was difficult and so we didn't get one man.

Riess: Interesting, I really had no idea that SLAC was such a black hole.

Schawlow: Yes, it was difficult. Of course, if they went to SLAC they wouldn't have to do any teaching, and they had a big group--particle physicists seem to want to hunt in packs.

Felix Bloch especially objected to that. He felt that each theorist should stand on his own. Well, it's a field where the problems are fairly narrowly defined. Somebody gets an idea and then everybody rushes to elaborate on that idea. Anyway, it was not the kind of theory that Bloch was used to, and he didn't like that. Nor, I think, did Schiff.

Riess: What about women and minorities? Were those important matters in those years?

Schawlow: No. No, they weren't. That's more recently we've tried to do something about it. We did start making some efforts to get minority graduate students, and we got some. But we didn't have any women faculty at that time.

Riess: Do you mean on staff?

Schawlow: On staff, no, we didn't have any women on the staff. It wasn't pushed pretty much. I think it's been more important later. But the trouble is, frankly, if you have an appointment, and you're not going to have another appointment in that field for some years, you try and get the very best person you can, and that isn't very often a woman or minority person--because there just are few of them, and chances are very great that if you really have to get a top person, it won't be a woman or minority.

Riess: You were chairman of the physics department. There's also a chairman of the applied physics department?

Schawlow: Yes there was.

Riess: Did these larger questions get chewed over between the two?

Schawlow: No, no, we didn't really have very much contact, and that's probably my fault. Later on, people did try and get more coordination between the two departments and they had regular meetings of the chairmen I guess it was. But not at that time. I just wanted to get on with doing some physics.

Riess: Yes. And in fact, you didn't need to say yes. Why did you say yes?

Schawlow: Well, I looked around, and either you do it or somebody else will do it to you. [laughs] I just couldn't see anybody that was any better. After I got out I sort of pushed for my successor who, well, was only moderately successful. You have a limited number people there--it was a small department. However, they have managed and have had a succession.

I did one year as an acting head when my successor wanted to take his sabbatical, and that was sort of a nightmare, because one day some students came to me and presented a petition that we recognize the graduate student research assistants as a union [bargaining unit]. Research assistants, mind you. This struck me as absolutely ridiculous because these were just people who were being given some money to help support them while they did their own thesis. But this thing had obviously been drawn up by a lawyer, so I could do nothing but hand it over to the university. I couldn't talk to them at all, tell them that I thought they were stupid. I couldn't say anything.

Riess: And it was probably for the entire university?

Schawlow: No, just the physics graduate research assistants. Well, ridiculous. Eventually it got to the labor relations board and they decided that it wasn't a sensible bargaining unit and that ended it. But I had to deal with that.

Riess: Also you had some dealings with the free speech movement?

Schawlow: Oh God, yes. I was chairman of the university's research committee at the time of the Cambodian invasion. The committee had already decided that they wouldn't allow any secret research--that had been banned. There had been some secret military research, in engineering particularly. They had allowed that research could have some classification if it were such things as needing to know the launch date of a satellite, something like that, which was considered secret information. But even that we had about decided to stop.

I had to go around with Bill Miller, who was then I think vice provost for research, or something like that, and go to departments like the music department and German department, and explain to them what research was, let alone classified research. The radicals wanted to stop all government-sponsored research, all defense-sponsored research.

Riess: So why were you going to the humanities departments? To explain all this?

Schawlow: They didn't know anything about it, and it eventually would come to a vote of the faculty senate. The issue was ending all government-sponsored research, which would have been utterly disastrous.

Riess: Were university-wide meetings called?

Schawlow: Yes, there was a meeting of the senate I particularly remember. They'd had this research committee, and a subcommittee on classified research, which was supposed to make sure that the classification was only incidental and not really secret stuff. The subcommittee had clearances, so they could see. One of the members of that subcommittee got up at this meeting and denounced the research committee for allowing classified research. Oh! It was disgusting. I had to get up and point out that we'd already stopped the classified research and that'd only been incidental.

Riess: Was Stanford brought to a halt in the way Berkeley was?

Schawlow: Not as much. I think there were a few days. Leonard Schiff was very active in going to talk with students and student groups about things. There were some sit-ins.

Riess: Did you have anyone like Charles Schwartz?

Schawlow: No, nobody quite as bad as that. Charlie Townes was president of the American Physical Society and had to contend with Schwartz, who tried to get things done there--silly things.

However, we had Bruce Franklin who was an English professor who actually incited the students, you know, saying, well, he wouldn't turn his back on people who use violence, something to that effect--not exactly telling them to do it, but sort of encouraging them. Fortunately, they had long hearings afterwards and they did fire him, which was a good thing. But he was a pretty troublesome person.

Riess: Was he a professor?

Schawlow: He had tenure and everything, but they got rid of him.

Riess: Okay, so other issues while you were chairman?

Schawlow: Well--. You had to get unanimity for everything. There were people who didn't like a couple of associate professors and didn't want to promote them, but I managed to get these reconciled. Frankly, I felt it was foolish to make a big issue

of promoting from associate to full professor. The guy has tenure, he's going to be there anyway. So why do that? You can still make his salary less than the person who's brilliant. But I did smooth those over.

Riess: Is there a system of assessing teaching ability?

Schawlow: Yes, there is. We have to worry about that some, but as long as it comes out adequately--. We have faculty members sit in on other faculty members' lectures. And they have to put out questionnaires at the end of every quarter for students to give an assessment of the thing.

Riess: And they do that conscientiously, or is that just honored in the breach, or whatever?

Schawlow: Well, we have had cases--later on, not in my time. We had an assistant professor who was a brilliant theorist but not a good teacher. We had a very hard time getting enough good things said about his teaching in order to get him approved for promotion to associate professor. We did, but he decided to leave anyway. He took a job at a national laboratory.

Riess: As you go up through the ranks do you teach less?

Schawlow: No, it's about the same all the time. Leonard Schiff had kept the number of courses down so that we had reasonable teaching loads. It didn't change.

Riess: Your research associates and teaching assistants would be your graduate students?

Schawlow: Yes. The teaching assistants would be the graduate students, not necessarily the same ones who were doing research with you, but there would be a bunch of them assigned to each course. They would have discussion sections and they would do all the grading. I never had to do any grading.

Making up exams was always something I hated, though. I did my best, but I found that when you have a big class, no matter how carefully you word something, have it checked by several people, there is always somebody who finds a different way of misinterpreting it.

Riess: Isn't there a tradition of finding brilliant alternative ways of looking at things?

Schawlow: Well, of course you're trying to do that, but when you have an introductory physics class, usually they just misunderstand

what it is you are trying to say--even though you tried to word it as clearly as you possibly can.

And worse than that, teaching these introductory classes--which I enjoyed in some ways because they had a lot of demonstration experiments to do--you can't ask for anything very difficult because it isn't a very advanced course. So you have to keep repeating similar things, but you know that the fraternities at least have files of the old exam papers. So you have to try and keep finding something different. After a couple of years it gets really pretty hard. I did keep switching around different courses of teaching every few years, but still making up exams was something I was very glad to get rid of when I retired.

Riess: Do you think that there's been any anti-Semitism in the department?

Schawlow: No, no. In fact, somebody who had been there as assistant professor said, "That's a nice little Jewish department they have there." And in fact they had--well, Bloch, Hofstadter, and Chodorow, for instance. But they weren't all Jewish. But no, anti-Semitism, if there'd ever been any, there wasn't any when I was there.

There had been--I noticed at the University of Toronto when I was a student, as far as I could see there were no Jewish professors at the university before the war. There are now. They just didn't--. It's not the virulent anti-Semitism that you have in Germany, but well, they just didn't think those were nice people to have around. Of course, a lot of the Jewish people there were immigrants, fairly recent immigrants from eastern Europe, and they were different, but they have plenty of Jewish people there now.

At Stanford, by the time I arrived, it was not a problem. I guess Bloch was probably the first one and he came in 1933, so he'd been around a long time.

Riess: Yes, that's right. And the kind of scapegoating usually just has in part to due with the fact that there's a lot of economic pressure and so you look around and wonder who's getting it.

The Family

Settling into Palo Alto

Riess: In these last minutes, why don't you tell about how you settled your family here.

Schawlow: I came out in April for interviews, and in May I decided. Then I came out in June, and I had two days to find a house. We found this house. It was an Eichler house.

Riess: Aurelia came out with you?

Schawlow: No. She couldn't. We had the three children then. But she let me decide. And I figured this was a standard California house. We could always sell it and move to something else. Well, we never did, we got in there and just sort of adapted ourselves to it.

Riess: It was in a new tract then?

Schawlow: Actually, it was three years old. It was the oldest house in that section of Stanford. They'd opened it up in 1958, and Eichler builds faster than other builders, so it was the first one finished. It had been owned by a librarian who had moved to become head librarian at University of Nevada. Then it had been rented by somebody, and it just came on the market the day I was there because the renters had a daughter graduating from high school and they wanted to wait until after she'd had her graduation to show it.

I checked with Aurelia, it was all right, and I decided to buy it. We paid just $27,000 which is what we eventually got for our house in New Jersey. I sold it recently for $457,000, so inflation has really taken place there.

Riess: Yes. Eichler houses were quite stunning, modern.

Schawlow: They're more open than I would have liked. I'd like to have more closed-off sections. But it was all right. We got used to it. It had some advantages. It had this radiant heat in the floor, which is very clean and the floor was always warm in the winter, so you could go around with bare feet. It was built no worse than it had to be, and no better either, I think. But it was well designed and well situated. Big windows on the north and east sides, very small windows on the

south and west, which are the hot places. It was comfortable. We thought of moving once or twice, but we never did.

Before we came we had hired an au pair girl from Sweden. She went with Aurelia to Aurelia's parents' farm in Greenville while the move was going on, because the house would be all packed up, and I came out here to meet the movers. Ingrid [the au pair] had a boyfriend that she'd met somewhere who lived near there. I think the day she came here she called this boy, and his father had died just that day, but he came to see her anyway. They were quite friendly for a while, though eventually she married a friend of his here staying in the United States. Not the one that she'd known before, but a friend of his.

The point is, we had this au pair girl, and she would work, I don't know, five days a week, eight hours, something like that. But I found I was spending thirty-five hours a week taking care of Artie, and on the weekends and so on.

I had been approached by about eight different universities that year, because it was the first year after lasers operated, but the main reason we decided to come here was because of Peninsula Children's Center. The Hofstadters had an autistic daughter, and Mrs. Hofstadter had helped set up the Peninsula Children's Center, which was at that time a school for handicapped children that met in an old house way out in a back part of Stanford. Here was a place for Artie to go. So proximity was probably the deciding reason we came here.

Autism and Artie ##

Riess: You've said about Artie's autism that they didn't *know* that's what it was.

Schawlow: They didn't know what it was and they didn't know what to do about it, either. The name had only been invented, I think, about ten years before he was born, and nobody knew much about it. By the time we came out to California he had the label of autism, but it didn't help much. There was no real government funding for people with autism.

There was one juvenile court judge here who was willing to make autistic people ward of the court in order to get them

whatever services they needed. We didn't have to rely on that, but there was this day program that was set up in a house on the Stanford property [referring to Peninsula Children's Center].

The first year Artie was there it was a good program, but after that the woman who was running it left and Artie didn't seem to take to it. He would go but he'd just sit off by himself and not participate in anything much.

Riess: How did your respective families deal with this when you were back on the east coast? Was there some sympathy?

Schawlow: Oh, yes, there was certainly sympathy. My mother was always good with children, and she liked Artie. She would visit occasionally and they would get along well.

Aurelia's parents were older; Aurelia was almost the youngest in a large family. They did visit. Her mother, I remember, visited us. Oh yes, we took Artie down there several times and people were sympathetic. But he was little, and at first you couldn't really tell whether he was slow starting to talk or what it was, because he wasn't aggressive or anything like that. He just was sort of a loner.

We really didn't know how bad it was until he got to be about four or so. Then the only thing we could find in New Jersey was a pediatric neurologist. She thought it was petit-mal epilepsy, had some sort of an EEG taken, but I gather that EEG's don't mean anything much at that age. And she prescribed some kind of a drug for epilepsy. That made him incontinent, and so he had trouble. He was going to a nursery school, or a day care place. That was a difficulty. I think he had to leave.

Riess: But having a name for his illness must have been some help.

Schawlow: Yes, it was I think. And later on it became a greater help because there became funding for autism--but that was much later.

[sighs] Oh! At Stanford we consulted a psychiatrist, and he didn't want to look at Artie, he just wanted us to talk about what it might be that we were doing to unconsciously hurt this poor child. After a few months I gave that up, though Aurelia continued to go to him. But I think he was harmful.

We also went to a neurologist at Stanford--they tried to do an EEG, and Artie was upset, so they gave him a strong sedative which I'm sure made the results pretty meaningless. Then the

neurologist thought amphetamines might have a paradoxical effect--they sometimes do--and calm him down. Instead, they just made him more excitable and made him more locked into doing things over and over, like jiggling shoelaces or sifting sand.

He would do that for hours and he wouldn't eat anything much. I forget--he would drink orange juice, but I don't know what else it was, there was very little that he would eat, but somehow that turned out not to do any serious harm. But this amphetamine was keeping him awake until one o'clock in the morning, so I had to be up with him until then.

He liked to go for rides in the car, and go swimming, which we'd been able to do in New Jersey, and also at Stanford when they opened a pool. Then he got to running away, and we built a big fence around the back yard and put in hooks on the doors. But you'd forget sometimes and then get a call early in the morning and he was in some neighbor's swimming pool. He wasn't in any danger, but--.

So, we were kind of desperate. Molly Hofstadter had gone to a place, Clearwater Ranch in Mendocino County, where they had people who were supposed to have mental handicaps. (Well, autism is kind of physical and mental.) We sent him there for the summer when he was seven, and the next year we sent him there to stay. That was a pretty good place for him. We would visit him practically every week, and have him down occasionally--I would go up and bring him down for the weekend.

Riess: Was there any kind of communication with him? From him?

Schawlow: Not really, no.

Riess: You must cut me off when it just becomes too intrusive, but for me it's learning also.

Schawlow: Well, I may start crying, but I'll keep going.

[close to tears] Well--was there any communication? No--.

Riess: You were speaking to him, I'm sure, all the time and the question is--.

Schawlow: Yes, but we were not doing the things we should have. We didn't tell him he was going to this place; we just took him there and left him. We didn't know whether he could understand it or not, we couldn't tell. He just didn't show much sign--.

He'd been up at this ranch for a couple of years and we'd go and see him often, take him out on outings in the woods, have a picnic or something like that. Or sometimes we'd bring him home for a weekend.

But then there was a very nice lady, Grace Turner, who was running what they called Townhouse. It was a house in the little town of Cloverdale, right on the main street. She saw Artie, and he reminded her of a boy with whom she'd had some success, and actually had gotten him to begin speaking. So she asked for Artie. Those were good years. He was there for several years. And that's where he learned to read, he tells us. He was ten years old. She taught him to read. But he didn't show us at all. We didn't realize it.

Then when he got to adolescence--they had young girls there and they were afraid of him. I don't think he had been hitting anybody then, but he began to have some tantrums and so they felt they couldn't keep him any longer.

Riess: You mean at Grace Turner's house?

Schawlow: Well, Grace had left, I forget just why. The last year or so there [at Townhouse] they had been taking him down to a day-school program, but he apparently hadn't been participating really.

Riess: Through all of this, the understanding of autism must have been changing.

Schawlow: Yes, slowly.

##

Schawlow: Bernard Rimland--he's a psychologist who was working for the Navy in San Diego but he spent a year at the Stanford Institute for Advanced Study in the Behavioral Sciences--he wrote this book in which he came out flatly saying autism was a physical problem and not just the mother was not warm enough.[1] And the people began to change their attitude toward it, but they still didn't really know anything. People began to use behavior modification, which is helpful in some cases, although it can get too rigid if that's the only thing you do. You know, where you reward good behavior and not bad behavior.

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[1]Bernard Rimland, *Infantile Autism, The Syndrome and Its Implications for a Neural Theory of Behavior*, Appleton-Century-Crofts, 1964.

But that didn't reach us, really, any of that. We tried to find another place for him. Oh, several places turned him down because they didn't know how to deal with autistic people. There was one place that had retarded children. He just didn't fit their type. Then we got into a farm near Petaluma. He was there for some months, but again he was being very withdrawn and not cooperating with the program, tearing up bedsheets, things like that. So they kicked him out.

At that point, there wasn't any place to go but Agnews State Hospital, or as it is now known, Agnews Developmental Center. We went down there and they told us about all these vocational programs they had. We thought, well, it might be all right, but what they did actually was they just doped him like a zombie, and they never had any vocational programs for him.

[sighs] He was occasionally violent. He broke somebody's finger, I think, slamming a door. I think they sort of were very wary of him and really didn't do much for him. And it was a terrible place, just terrible. Very noisy, with a lot of other people and the bad things they did, like smearing feces around. We tried taking him out. We tried taking him at home for some weeks and getting somebody to take him to a day program. Well, he hit that girl and she wouldn't do it anymore. So back he went. We kept looking. We found a place in Concord where they took him for a few months. And we were paying for an extra person, but they still felt they couldn't manage him. So that didn't last and he had to go back to Agnews.

Finally--Agnews was trying to get rid of him. We had gotten a court order making us conservators with the right to control his medication and got them to stop giving him drugs. They were so angry at that they tried to kick him out, and they tried to persuade us to take him to Napa State Hospital, which had something that was alleged to be a program for autism. Well, I went up there with Aurelia, and then Aurelia and Helen went up there and spent most of a day, and decided that was no good.

We asked the woman who was the head of the [National] Autistic Society chapter in Sacramento, Marie White, if she knew any parents of people at Napa. She said, "Oh, don't go there. It's no good. But maybe he can get into this place where my son is." This place was in Paradise. (And she had had lots of fights with the authorities and forced them to find a place for him.) This man in Paradise [Chris St. Germain] had this school called Paradise School for Boys, which had teenagers who were going out to day programs, to school, and so

on. He had admitted her son Doug White as a special case. They got an exception.

Then he looked at how much they were paying for adult autistic people and he thought he'd make more money. So after some months he managed to get his license changed so he could take adults and switch over to that. Well, he wasn't very smart. He didn't realize that the adults required a lot more staffing because they weren't in school all day. Artie had been there just a few months and--

Riess: How old was Artie at that point?

Schawlow: I think he was twenty-seven.

After a few months, Mr. St. Germain came to us and said he was going to have to close the place. He had broken up with his rich wife, was losing money, and couldn't afford to keep going. Well, we took a mortgage on our house and lent him $100,000. It was about 1983, so--it was after we got the Nobel Prize. We lent him the money and he kept going for a while.

We tried to get some sort of check on his finances--he had an accountant looking over his figures, but she wasn't doing her job, she would just take anything he gave her. He was going through this [money] so that by 1985 he was running out of money again. Apparently he had to admit that he'd been dipping into his clients' money. I thought he was going to lose his license.

But Marie White and I had set up a foundation, a non-profit organization which we called California Vocations [Inc.], because we wanted to provide some vocational training for the people there. But we found we couldn't put money into this so-called for-profit organization, we couldn't find a way. Well, when we saw this trouble coming and he was going bankrupt again, we had the charter changed so we could operate a group home. By the early fall of 1985 we were told that if he [St. Germain] stayed the income tax people would close it down because he owed $58,000 in back payroll taxes. But if somebody else took over, they would follow him for the money and let us start fresh. So with essentially no warning at all, we took over and he disappeared.

We took over, not knowing what we were getting into. We had hired a lady bookkeeper, retired from one of the big aerospace companies. She had tried to keep him straight but couldn't. Then we got some friends from our church to help us on the board and one of them straightened out the books. She [the bookkeeper] had been treating the books like she might her

household expenses, not really keeping things well-budgeted and careful. But we did get the books straightened out and hired a new director who's still there, Phil Bonnet.

Riess: He had studied autism?

Schawlow: Yes, and he'd worked in several group homes before. He had a degree in psychology and had worked in several group homes. I think the regional center had not trusted Chris St. Germain, so they didn't keep his place full. That was one reason he was losing money. They took somewhat better to the new management and let us fill up. He [Bonnet] has managed to keep the budget balanced, although it's been very tight and we don't do all the things I'd like to see us do for our clients. I've put in a lot of money, given them a lot for very special purposes.

When Aurelia died, I had some stock that had gone up in value and I gave them something like $125,000 to build a recreation and training building, now known as the Aurelia House. That was a success, they got a good builder and he did a good job on that. Artie doesn't use it much anymore because we rented an apartment in Paradise which we could use when we went up there, and Artie comes down there nearly every weekday when he has one-on-one. He blows up occasionally, and at one point they got the regional center to give him one-on-one staffing for five days a week.

Riess: The regional center administers the disability money?

Schawlow: Yes, that's right. There are nominally private organizations that administer the state's money. They are, of course, very much the creatures of the state. They have to do what they're told. We are still supported by the San Andreas Regional Center which is down in this area, and not by the Far Northern Regional Center. The Far Northern has been rather tight in providing extra services, and so I felt it was better for us to stay where we were.

Riess: So he gets one-on-one--

Schawlow: --five days a week.

Well, they say he's doing very well. He has epileptic seizures occasionally. They've been increasing in frequency, which worries me a good bit, and I've been trying to find out what's the matter. At one point where he had some bad outbursts they got the psychiatrist to prescribe Haldol, a so-called antipsychotic drug which has very bad long-term consequences, and also lowers the threshold for seizures. I've been pushing on them to cut down the dose and they have cut it

some. I haven't gotten statistics lately of how frequent the seizures are. But they used to be one a year, and now it's been about one a month.

Riess: So he does have a standard panoply of medications.

Schawlow: Tegritol for the seizures. Tegritol and Haldol are really all he is taking. We've really made it clear that we did not want him to be heavily drugged. And he seems all right. He doesn't seem dopey like he did in the hospital. He really looked like a zombie there.[1]

Now, in addition to that, we have hired teachers to work with him one-on-one. There was a nice young woman who worked with him for maybe seven years or so, but then she got cancer and died--no, it was longer than that, I guess, that she was with him. It was only last year we hired new teachers. We didn't know which one to hire. Artie sat in on the interviews, and he didn't agree with Phil Bonnet, so we hired them both. The one Artie preferred was the better one, I think. And I hope she's still continuing. She took the summer off. I'm not sure that she's come back. The other one did quit after a year.

Riess: What are they working with? Are they working with the facilitated communication?

Schawlow: Yes, they've both learned to use the facilitated communication.

The one he had before, Linda--oh God, I'm so bad with names--he wanted to write with her. Just after we found out he could communicate, we found he also could write or print. Again, he wanted a hand on his. The way he did it was take the top end of the pen while the bottom end was in his mother's hand and manipulate it. With Linda he wanted to write and he'd write with big scrawling letters, about one word on a page. Apparently some other autistic people write that way too. I think they have difficulty starting and also stopping a motion. Although we've heard that you can use a squiggle pen which puts a vibration on the hand. Some autistic people can write smaller when they have that. We've tried it half-heartedly with Artie. I have to see whether it's being used now or not.

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[1]July 1, 1997. We have a new psychiatrist and a new neurologist. Art is not taking Haldol, and the neurologist has added another drug to help prevent seizures. [A.S.]

Linda didn't know very much mathematics. She took him through grade school arithmetic. He already knew how to add and subtract, but she showed him how to multiply things, how to carry, and that about used up what she knew in mathematics. So we hired a junior high math teacher to teach him more advanced stuff. At one point, Artie said, "I can do so-and-so's baby math, but I really want something more advanced." So we got a man who was a lecturer in statistics at Chico State. He worked with Artie, went through algebra and I think was even getting into calculus. It's hard, though, to do, because Artie can't write very much and you can only ask him questions which he'll give you a yes, no, or a number for an answer.

Riess: He can say yes or no?

Schawlow: He'll type it--either type it or point to a card that has words on it.

Riess: What impedes speech?

Schawlow: I don't know what it is, whether it's difficulty in initiating it, or something is inhibiting it. But he has occasionally said a few words very clearly. At the Autistic Society convention last summer I ran into a number of people, about ten or so, who knew at least one autistic person who just very, very occasionally said something. So all the speech mechanism is there, but they just can't produce it on demand, I think.

Riess: It is the most important diagnostic symptom?

Schawlow: Well, there's a wide range of autistic people. Some of them can talk. Some of them use echolalia, where they repeat what's been said and what somebody else said to them. Like if you say, "Do you want to eat?" they'll say, "Do you want to eat?" really meaning they do. But he never did that.

Failure to communicate somehow or other is a difficulty, but there's a wide range. There's some people now who I think are among the forefront who think it's a neuromuscular problem. They just can't control muscles that they want to do things. I think there's a lot of truth in that.

Riess: What does learning algebra do for the whole personality?

Schawlow: I think it's something he wanted, he asked for it. He's certainly much more relaxed than he used to be. In fact, Phil Bonnet was mentioning that. He's participating more when they have parties, he's not so withdrawn. Although I don't think he really makes friends with the other residents. But he is quite friendly with some of the staff.

Riess: They're all adults there?

Schawlow: Yes. I have a movie--well, there are a lot of movies with Artie in them, but there's one--a film company from Luxembourg was making a series of Nobel Prize winners. I told them about Artie and they sent a film crew up to film him at Paradise. They used it, I think, for some medical program in Germany and Germanic countries. I have a copy of that film and also one where Artie was working with Aurelia and me using facilitated communication. There's also a video about Cypress Center in which he appears occasionally.

Riess: Cypress Center was the name?

Schawlow: Oh, this Paradise School for Boys we had to take over on very short notice and had to change the name because Paradise School for Boys had a bad name. It was on Cypress Lane so we just decided to call it Cypress Center. Turns out, we hadn't realized that just up the road, hidden behind some trees, there's Cypress Acres which is a large convalescent home. They do get confused. Mail gets scrambled sometimes. Probably should have taken a different name, but Cypress Center was one that didn't describe exactly what we were doing. I didn't want to do that.

Riess: You and Aurelia really got into the whole world of autism. You went to meetings.

Schawlow: Yes, we did and we learned stuff. [First] the Los Angeles chapter of the Autism Society put on several conferences that we went to. It was good to see other people struggling with the same problems. We met some people there and got involved with the Autism Society. We weren't in the beginning, but we started going to their meetings. Met a lot of people and picked up a few things here and there.

When Chris St. Germaine changed to this adult program he hired Gary LaVigna, who was one of the foremost experts on behavior modification for autistic people. He hired him as a consultant, but they never did implement much of his program. He also hired a very incompetent guy to be their psychologist, and nothing happened. But LaVigna sort of pointed them on the right track for a completely non-aversive program of behavior modification, where they just reward good behavior and--

Riess: No punishment.

Schawlow: --no punishment. No consequences other than what are unavoidable: if you put your hand on a hot pot, you'll get burned. That isn't punishment, but--.

Riess: Is much money going into research on autism?

Schawlow: I think there's more money now going into the medical side of autism, and there's certainly no doubt that it is a physical defect, at least caused by that. But of course, they have the strange perception and difficulty communicating that can lead to some bizarre behavior. I think there's not enough going into the behavioral side of it.

There's a big program at Stanford looking into genetics, trying to find out to what extent some cases of autism have a genetic base, where in the genome that is. But frankly, I'm not very interested in that because it isn't going to do any good for my boy. There are people doing studies of brain, both by magnetic resonance and also by autopsies of autistic people. They slice the brain into small slices. They're finding abnormalities and getting a general idea where they are, although the brain is very highly interconnected, and so if there's something wrong in one place, it may cause problems elsewhere.

I guess I like to see anything going on in the medical world, but I sort of feel that isn't going to help my son, not going to happen soon enough. On the other hand, adopting a teaching and somewhat behavioral strategy seems to help.

Riess: It gives him a life.

Schawlow: Yes. He's also held various part-time jobs.

Riess: How does he manage to do that?

Schawlow: Well, there's always somebody with him. They call it supported employment where there is somebody there, his job coach, in case there's any problems and also to show him how to do things. They've been rather menial jobs. Some of them haven't lasted long, not through any fault of his. He even worked for a while as a dishwasher in a restaurant. He hated that apparently, but the restaurant went out of business just about the time he got really fed up with it.

Now he's doing some recycling a few hours a week. They started a recycling program because the town of Paradise doesn't have one. They distributed boxes and they go out and collect the stuff in the boxes and bring it back there. Some other residents sort it out. Somebody who's in the garbage disposal business buys it from them. He likes that. He likes going out. He's enjoyed emptying garbage pails for a long time.

Riess: [laughter]

Schawlow: He used to do it too much. He'd throw out things.

Riess: Is he very strong?

Schawlow: Yes, he's quite strong. Yes, I couldn't stop him from doing something he wanted to do. Fortunately this young man who's working with him now is bigger and stronger than he is, and he can handle him if there's any problems.

Riess: Is the young man who's working with him a professional?

Schawlow: He was in the Army Medical Corps for a few years and has qualified as an emergency medical technician. He's studying slowly to become qualified as a nurse. He's not in the full nursing program yet, but he's been taking things like anatomy and he has most of the requirements.

Riess: I realize how usually I turn aside when I see people with needs. You know, maybe they have a cup out or something like that. But you must have a whole different view of the world.

Schawlow: Well, I tell you honestly, and I've told other people, I really am only interested in helping Artie. But I know that to help him I have to help others. I have to keep this place going, for one thing. I've had a lot to do with that and risked money to have a place for him, and of course that benefits others too. Because you can't just have him by himself. I've still pretty narrowly focused on what might help Artie. I go to these meetings. I pick and choose among the sessions.

Riess: I was just thinking that it's almost like a religious thing, a kind of compassion.

Schawlow: Yes, well, you do have more sympathy for others. But I couldn't see myself actually working with these other autistic people. Certainly I do feel sympathy for other people with autism. They vary widely. Some of them recover almost fully, some of them are a little strange. Looking back, I know one person I remember who may have had some mild autism. He was a rather withdrawn sort of person. He worked as an accountant. I met another young man who got a Ph.D. in mathematics from University of Michigan. He tried teaching, but he just couldn't do that because he didn't have enough empathy with the students. So they come in all levels. Some are much worse than Artie. But yes, I do feel sympathy.

I've tried very hard. We've brought in all the best experts we could find to consult there. It's been very hard

because they really didn't want any outside interference and they sort of run a minimum program. They don't hurt these residents, but they don't really do nearly as much for them as they could. Like each house cooks their own meals, but I think the staff does the cooking. They should be training the residents to do that because some of them will move onto semi-independent living.

They have one girl who demanded to have a place of her own. This is now the state's policy, to try and help people live as independently as possible. So they did get a place for her. Somebody checks up on her from time to time, probably every day. I don't know just how it works.

Helen and Edith ##

Riess: Your daughters, Helen and Edith, did they manage to have a normal upbringing in the face of all the concerns with Artie?

Schawlow: Well, yes and no. I think I neglected them. I was there, you know, but I didn't play games with them or do anything much with them. But there's nothing I could do about it, I just did the best I could. But it was a struggle.

Riess: They've gone on to interesting careers.

Schawlow: Yes.

Riess: Were they very academic girls?

Schawlow: Oh, they were both pretty smart. Helen had a lot of trouble with mathematics. She got interested in French. Her French teacher at Castilleja, this private high school she went to, got her interested in French and she was very good at that. She has a very good ear for sounds. She could imitate--she could say a word in several different ways, in different accents, imitating different people.

I think the public school near us at Stanford was really pretty bad. They should have drilled her more on arithmetic tables earlier and she would have done better. Because I think she's really not that bad. Now, she does arithmetic in her head quite competently. She just got started wrong.

At this school, they combined fifth and sixth grades in her last year there. So they were reading this silly book, *Little*

Britches, that she had read the year before in fifth grade. Then she enrolled in junior high and they put her in with the bonehead English class and the advanced mathematics class. Well, I think they were going to read *Little Britches* again, but at this point Aurelia decided to enroll her in this quite good private school, Castilleja. That's in Palo Alto. And there she did quite well.

Riess: Did Edie also go to Castilleja?

Schawlow: She went there for several years. Then she decided that she wanted to be in a regular high school--maybe she wanted to meet boys or something like that--so she went to Gunn High School her last two years. But she went through junior high and the first couple of years of high school at Castilleja.

Riess: And then where did they go on to school?

Schawlow: They went to Stanford. Fortunately, when I came there was an arrangement where children of professors, if they were admitted to Stanford, could get free tuition, but they couldn't get any help if they went elsewhere.

Then about two years after I came, they changed the rules so that you could have half of Stanford's tuition anywhere. But people objected. I didn't, but they objected. So we had a choice, and I chose to have full tuition at Stanford. I thought that if they couldn't get in the Stanford, the state universities are quite good here in California and that would be okay. But fortunately both did get into Stanford and did reasonably well there.

Riess: And lived at home?

Schawlow: No. They lived in the dormitory. But of course they would come home quite frequently. It's only a mile or so away. And they'd bring home washing.

##

Schawlow: Helen thought she might be a high school French teacher, so she went to Berkeley to get an M.A. in French education. [She] got very good training there through a very good man, Gian, an outstanding teacher, and she learned a lot about teaching. Then they sent her to do practice teaching in downtown Oakland, and she came out one day and a big man stuck a knife in her ribs and said, "You lay off my girlfriend." She didn't even know who his girlfriend was, but that was the end of her high school teaching career.

Anyway, she decided to finish her master's degree in French there and came back and get a Ph.D. at home. By that time, well, she and her mother got on each other's nerves, so we bought a little house for her and she shared it, rented rooms to a couple other girls, and lived there quite happily. That worked out pretty well. Sometimes, young people do get on their parent's nerves, and vice versa.

Riess: Let's finish off the rest of Helen's story.

Schawlow: She got a Ph.D. in French at Stanford and she feels she also had some very good teaching experience here as a teaching assistant. Professor Hester was her master teacher, and he and Gian had written a book, an introduction to French, a first year textbook. She did all the exercises, to check out the exercises for them. So she was very well-equipped to teach French.

But jobs teaching French were very scarce and I think maybe she should have waited a little longer--she started applying for jobs before her Ph.D. was actually granted. You know, a lot of people unfortunately give it [that practice] a bad reputation by saying that they are going to get their Ph.D. and they don't, but hers was quite certain.

She had gone with us to France on my third sabbatical in 1985. While she was there she spent a lot of time doing research in French libraries and had good material for a Ph.D. thesis on an obscure surrealist writer, Pierre Unik--U-N-I-K. He had not written an awful lot, but he had been associated with some of the more famous ones like [Andre] Breton. He also had worked on films with--what was his name?--[Luis] Bunuel, I think it is, the famous film producer, who had mentioned him very enthusiastically somewhere and had said, "Why doesn't somebody write something about him?"

Pierre Unik was one, when they had the split between Aragon and Breton--I think Aragon was a militant communist and follower of the party line, and he [Unik] went with him and wrote mostly for party newspapers after that. He was captured by the Germans and imprisoned during the war. I think he was drafted into the French army. He wrote some poetry then which people think is pretty good. Toward the end of the war he escaped from a German prison camp, disappeared into the mountains, and was never seen again, probably died.

Riess: Quite a tale.

Schawlow: Yes.

Riess: Did she publish it?

Schawlow: Yes. She got a little book published on that. It was a nice piece of work, although she didn't feel she wanted to continue that research. Now she's gotten very interested in French in North America, and is thinking of doing a book on that if she can get some time off. But it's very difficult. This university has a foreign language department, and just has two people in the French section.

Riess: Which university is this?

Schawlow: University of Wisconsin at Stevens Point--it's a branch of the university. It's quite big, but it's mostly undergraduate. There's only two people, so it's very hard for anybody to take time off for a sabbatical. They don't have money to replace them.

Riess: And she has a family?

Schawlow: Yes. But she has some ideas of possibly getting half-time off, and perhaps that can be done.

Riess: When she thinks about North America, is she thinking back to her Canadian roots?

Schawlow: Yes, well, Quebec and Louisiana. We went to Louisiana last May. There was supposed to be a festival of French culture in New Orleans, but when we got there--the whole family went--she decided that it wasn't worth bothering with, so we just kind of explored New Orleans and then we went to the Cajun country, Lafayette and New Liberia.

Riess: You must have loved the music.

Schawlow: Well, the music in New Orleans was good. Not great, but good.

Riess: I was thinking of the Cajun music.

Schawlow: No, I didn't hear any unfortunately. I don't really quite know why we went there, except to see the places. We didn't really get involved with the Cajuns, as such.

She has quite a collection of movies and records of the various kinds of Louisiana music. There's Cajun, Creole, and Zydeco. She knows the differences between all these. She gave a lecture to the Wisconsin Association of French and Language Teachers earlier this month, and apparently it was very well-received. She had about a hundred people. She showed a little film clip recording. She's very good at teaching. She had

handouts for them, outlining how they could use this in a classroom unit on Louisiana culture. She's very good at that.

But it's a tough job, and getting worse because the enrollment in French in high school is dropping and so they are getting fewer who come with any high school French who would become French majors. They've had a lot of majors, they're second in the state or something like that, but if they come in with no French at all, they really can't do a major. Well, Spanish seems to be taking over the world. It has a reputation of being easier. I mean, here it's useful. In Wisconsin, they'd be better to learn French because there are a lot of French Canadians just across the border, not only in Quebec but in Manitoba.

Just one more remark: she wanted to show something about these Cajuns who are working people, farmers and so on, to get away from the image of French people, the perfume sniffers. [laughter] But in fact, the students are really more interested in France and French culture.

Our other daughter, Edith, is very bright, but never really much of a scholar. I think she did very well. She majored in psychology. Then she decided she would get a master's degree in nursing from UCLA. She went there after she graduated from Stanford in 1981. She had to take off a few weeks early in December to go to Stockholm with us and she didn't go back. By that time, she was very deeply involved with her boyfriend, Bill Dwan, and they got married the next summer.

Riess: What's her last name?

Schawlow: Dwan, D-W-A-N. It sounds kind of Chinese but actually it's Irish. I think it's a variant of Dwayne--D-U-A-N-E or D-W-A-Y-N-E, but it's Dwan. When I was in Ireland later, I looked it up in the phone books. There are two phone books for all of Ireland, one for Dublin and one for all the rest of it. It's not a big country. There are a number of Dwans around the town called Thurl, it seems. Maybe there are twenty or so, not very many.

We were on a sight-seeing trip, my wife and I, and in Kilkenny we saw a truck and I almost swallowed my teeth because the sign on it said "Dwan's Makes Better Dwinks." [laughter] There's a soft drink company named Dwan and that's their slogan. Unfortunately, I didn't get a camera out quick enough. But later, Frank Imbusch sent me a couple of things with that slogan on it.

Riess: So instead of getting her nursing degree, she--

Schawlow: --got married and has had three children.

Her husband was from a Catholic family. In fact, he'd gone to a Catholic high school, and they were married by a priest who had been his high school mathematics teacher. I expected some difficulties, religious difficulties, but it didn't turn out the way I expected at all. Edie had not been much interested in religion as a child--you could drag her to Sunday school, but she showed no serious interest. But they fell in with some Baptists and before you knew it, they were both being baptized in a Baptist church, a rather fundamentalist group.

Riess: Where is this?

Schawlow: Well, they lived in Menlo Park at that time. They had a house in the country, in Woodside I think it was, where some neighbors were Baptist. Then they lived in Los Altos for a while. Then they got really deeply interested in religion.

Bill wanted to do something to help religion. He had gotten a dual major or he'd gotten a B.S./M.A., I think, in biology and mechanical engineering. He thought he wanted to do something in prosthetics, or something like that to help people. He came from a rather wealthy family. His great-grandfather was one of the three founders of the 3M Company, so he has a good bit of money, so he can do what he wants. He did work for the Veterans Administration but I think he found that they were treating him just as a technician, rather than part of the research team, doing programming. Then he took a job with Lockheed doing programming for a while, image processing for space missions.

But then they said they wanted to do something with religion, so he got this job with a company called Walk Through the Bible, whose office was near Charlotte, North Carolina. It's actually in South Carolina, in the former PTL complex, this outfit from Atlanta has rented a building there. They were preparing materials for teaching Christianity and the Bible, making movies and other educational materials. He was doing some computer work there and he liked that quite a bit. That's why they moved to Charlotte, North Carolina. Houses there are cheaper. You can get a huge house for what he sold his house here.

But just recently this year, that company has closed that office, decided that they couldn't afford it anymore, and he's now taken a job as a science teacher in a private elementary school. He enjoys the work. He may decide to get a teaching credential later. As I say, he can afford it, he doesn't have

to work if he doesn't want to. But he's a very conscientious guy and wants to do something worthwhile.

Riess: And what is your daughter's role in all of that?

Schawlow: Well, she has three children which keeps her busy. But she also has gotten very deep in it and she's teaching a Bible class in this church of a small denomination whose name I forget. It's an offshoot of the Lutheran. Anyway, she teaches this Bible class every week I think, and does a lot of work preparing for it. I'm sure she does a good job.

They don't seem to have any desire to come back to California. Charlotte is a nice town. It's growing very fast. It has a lot of big new buildings. It has a lot of things; there's a fine science museum, a concert hall, good hospitals. It's a pleasant city. It reminds me of Toronto when I was a boy--you know, a moderate size city, not a megalopolis like New York. So they seem happy there. I think Bill doesn't really know what he's going to do eventually. He's an engineer at heart, I think. He's very good at fixing things, and apparently a good programmer, too.

Riess: We haven't mentioned Helen's husband.

Schawlow: Oh, yes. His name is Tom Johnson. He comes from a Swedish American family. Of course, Jansen is a very common name. It's spelled J-O-H-N-S-O-N, but the Swedes would pronounce it "Jansen." They're very, very proud of their Swedish heritage.

He got a Ph.D. in anthropology from University of Illinois, and he's on the faculty in anthropology at this university [University of Wisconsin at Stevens Point]. They met at Stevens Point and got married and they have these two daughters. He's a very intelligent man and has diverse interests. He's wonderful at getting along with people. Indians was his specialty, American Indians, and he participated in the Sun Dance with the Shoshone tribe. He really gets their confidence. But he tends not to publish very much; he's a perfectionist who can't finish things off. So he's not famous, but he's a good anthropologist.

Riess: I wonder about fame, the theme of fame in your family, and how your daughters have loved or resisted that.

Schawlow: Well, I can't say much about that. I mean don't know much. Things were the way they were. I think they enjoyed going to the Nobel ceremonies, but I don't know. I don't think either of them had any interest in going into physics or doing

anything with physics. If I had had more time with them as children, I might have played with them more, with Meccano or something like that, gotten them interested in mechanical things. Edie probably would have had the talent to do that sort of thing if she wanted to, but she never did really.

You know, after Artie was such a disappointment, I never felt ambitious at all for the girls to do anything particular. If they're just reasonably normal, that's good enough. I never pushed them at all.

So, is that enough about that?

Riess: That is enough, yes. Did you have any graduate students who were girls?

Schawlow: I had a few, yes. One of them, Antoinette Taylor, she was really quite bright and good at measuring things. One day, though, I was getting a bit worried about her. I had suggested some things that she might build, to improve the apparatus, but she didn't get around to it. I said, "Look, if you go on like this and never build anything you're going to end up in the traditional woman's position of taking measurements for some man. So you really ought to build something." She took my advice and did build an electronic circuit that they needed for the experiment.

Riess: And so that was a breakthrough for her.

Schawlow: I think so, a little bit, yes. She had all the ability she needed. She got married then to a theoretical solid state physicist, and they're both at the Los Alamos Laboratory in New Mexico. I saw her briefly at a conference in Baltimore a year or so ago, but I turned away to get a cup of coffee and never saw her again. [chuckle] Too bad. It was one of these big meetings.

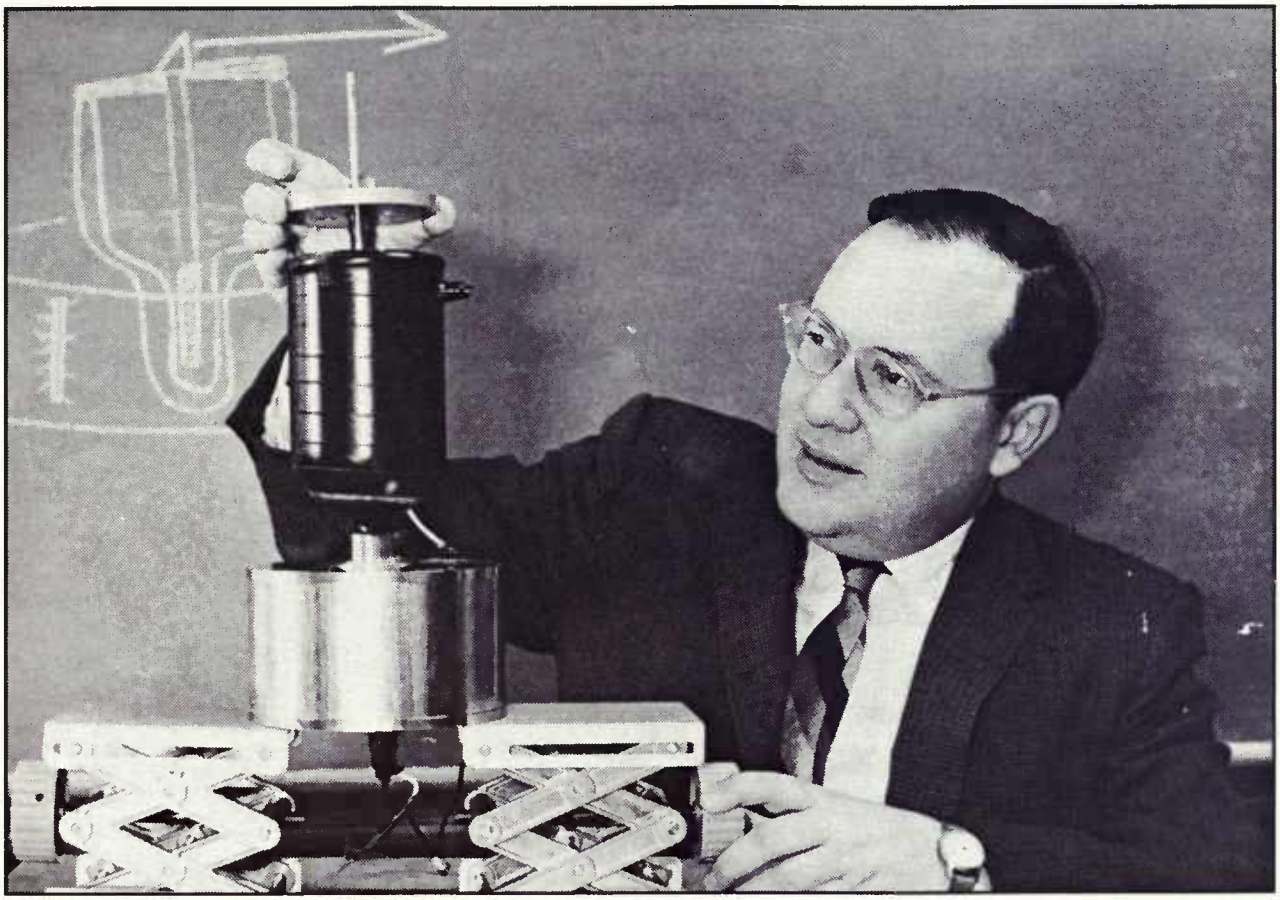
Riess: But can you actually make someone into a physicist? From your accounts of your own childhood, you were a physicist from the minute you could lift a pencil.

Schawlow: I don't really know. I presume that anybody who comes to be a graduate student in physics has some interest, at any rate, and you try and find out what their abilities are. Some of them are really not at all creative, and they're just not going to be real physicists. They may be good at doing exams as undergraduates--oh, they're so different, there's such a tremendous range of abilities.

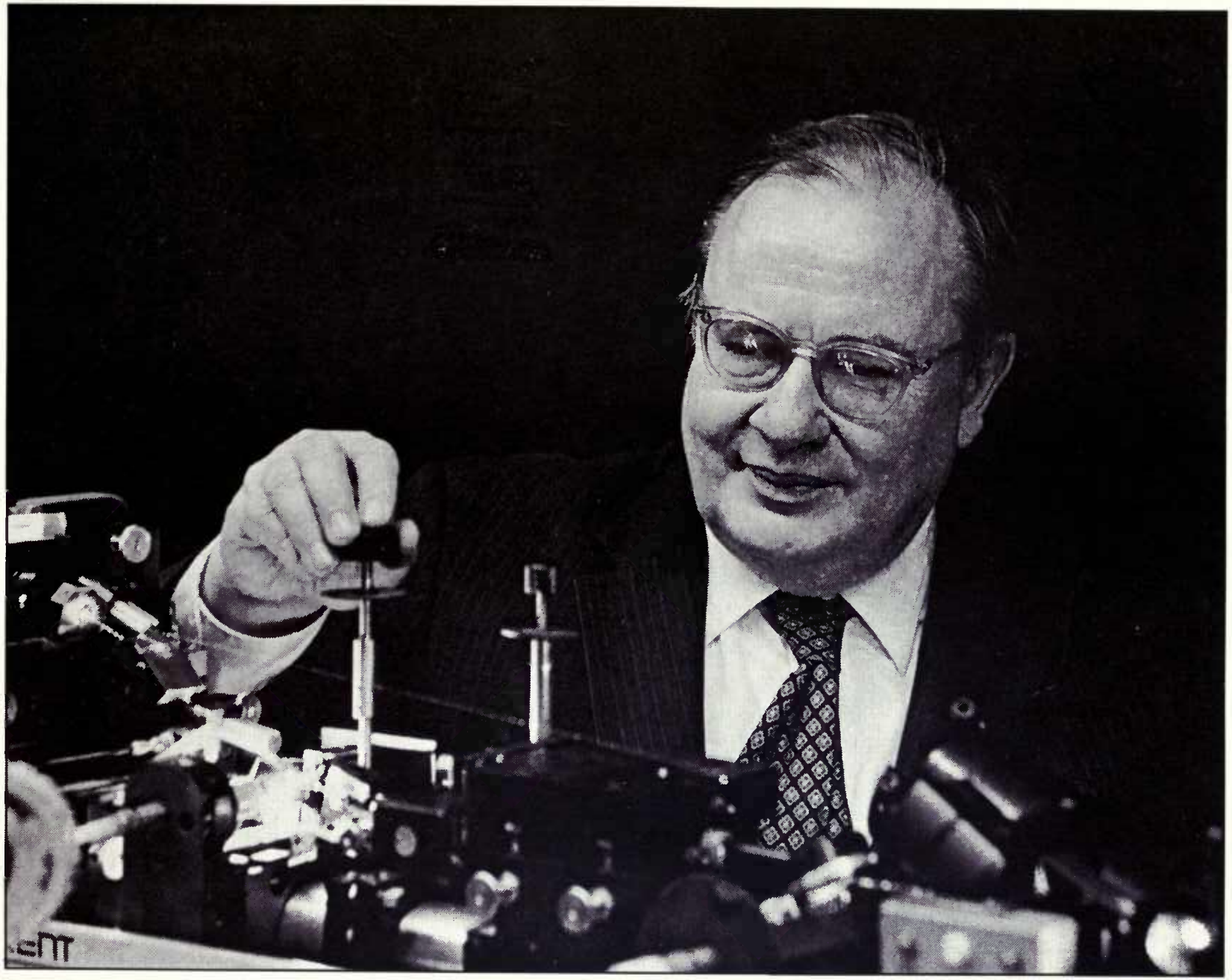
Riess: I was reflecting on your comment about doing more with your daughters. Do you think the early education is essential in setting the stage for the development of a future physicist?

Schawlow: Well, it could help.

Arthur Schawlow at Columbia in 1949.



Arthur Schawlow with a laser consisting of a rod of ruby cooled by liquid nitrogen and excited by light from a flash lamp reflected by an elliptic cylinder reflector. Stanford, 1962.



Arthur Schawlow adjusting a tunable dye laser used for spectroscopy, 1981.

Arthur Schawlow with his family in Stockholm to receive the Nobel Prize in Physics, 1981. Left to right: daughter Edith Schawlow, sister Rosemary Schawlow Wolfe, wife Aurelia Townes Schawlow, Arthur Schawlow, and daughter Helen Schawlow.

Arthur L. Schawlow, 1991.

V WORK AND STUDENTS

Secrecy, Motivation, Morality

[Interview 6: November 7, 1996] ##

Riess: I think I asked you much earlier in these interviews what it was that bothered you about Joan Bromberg's book, which seems like an authoritative report on the laser in America, but let me ask it again, now.[1]

Schawlow: There are two things about the Bromberg book that seem to me less than satisfactory: one is that she somehow has the fixed idea that the military were orchestrating everything in this field, and it wasn't true at all. They did supply some money, but they really didn't initiate anything. When I was at Bell Labs, of course, they didn't look to the military for money. We didn't take any outside funding. At that point we could do whatever we wanted to do--as long as it seemed relevant, in some way, to communications.

And I think also she gave a little too much credence to Gordon Gould who really contributed almost nothing to the growth of lasers. He had a lot of stuff written in his notes from time to time, [from] which he managed to get patents. But everything that he revealed later had already been found by other people. Those are the things that bothered me.

Riess: How did you work with her? She interviewed you?

Schawlow: Yes, she did. I guess I gave her what materials I had, copies of my articles. There's a rather better book by an Italian, M. Bertolotti.

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[1]Joan Lisa Bromberg, *The Laser in America, 1950-1970*, MIT Press, 1991.

I will say that the parts of the Bromberg book that I didn't really know anything much about, like the semiconductor lasers, seemed better to me. [laughs] I guess it's always the case that when a reporter, or even an historian, writes about things that you really know, it's never quite right.

But she really is wrong on the motivations, at least for the early work. We just had no thought of military interests at all, really. It was a classic problem, really, like the search for the origin of superconductivity. This going from longer to shorter waves, and trying to get still shorter, is something going on through the whole history of radio from the beginning of this century--and one with which I was certainly very familiar. I think Charlie was too. I never gave death rays a thought, and I really expected that the first laser might produce microwatts or something like that. Whereas I was really very surprised when Maiman's first laser produced a kilowatt in short pulses.

The military did start putting money in there. They wanted me to get a clearance and serve on committees, but I knew that if I accepted a clearance, I'd have two problems. One is that I would know things that I couldn't share with my students, which I didn't want to do. The other thing was that it would take a lot of time. I'd probably have been on every laser committee in the country. So I just refused to get a clearance until much much later, when I did get one to serve on the National Research Council's Committee to study ways of preventing forgery of currency using color copiers. That, I thought, was a worthwhile project. I don't think we solved the problem, but we made some suggestions.

Our report had to be secret, of course. Still, some of the things we discussed are already in place, like the threads in the paper, and also some fine print. At the moment there's fine print on the higher currency, the hundred dollar bills and so on; there's fine print around the picture which is too fine for the current generation of copiers to copy. But that won't last, and I know they're in a running battle with the color copiers. It's so easy for a person who has something he can't share, like a girlfriend he doesn't want his wife to know about, or a drug problem, to just put a twenty dollar bill on the office color copier. You can get away with some amazingly bad currency if you pass it under the right conditions. Anyway, that was around the late eighties. But up until then I wouldn't take a clearance at all.

Riess: How did you work on that problem? Did you get together as group each time?

Schawlow: Yes, yes, we had a few committee meetings. I didn't do any work outside of the committee meetings. Brian Thompson of Rochester was the chairman. I got off it after, I don't know, a couple of years when they did our first report. But I know that the committee did continue.

Riess: So the new Hamilton hundred-dollar bill reflects all of this?

Schawlow: Well, some of the things that we talked about, not everything. It probably has some things that we don't even know about. They try to have secret [features]. There are some very good counterfeiters, a gang in East Asia that has moved from country to country apparently makes very good copies--they even know where to put magnetic ink and so on.

Riess: Well, it's probably a field worth studying.

Schawlow: Yes, there's money in it. [laughter]

I really never got very deeply involved in military things although you heard a lot--people would come and tell me things. In fact, many years later Elliot Weinberg, who was working for the Office of Naval Research and supervised some of our contracts, said, "You know, there never was anything going on that you didn't know about." I think that's so. I really have the opinion that military secrecy usually hides incompetence, at least when it's military research.

They had a project to make a hundred joule ruby laser, which cost a lot of money and didn't lead anywhere. They were trying to get weapons right away and the state of the art just wasn't there yet--maybe it isn't even now. It was satisfying to me that one of the first applications was for medical uses, for surgery on the retina of the eye. But I have very ambivalent feelings about the military. I don't like the idea of wars and killing people, they don't make any sense, but I know they happen. And I remember, of course, very well World War II when we were really faced with some horrible evil that had to be fought, in Hitler. In that case I was willing to do my small part, but generally I think it's a waste of time, most military research.

Riess: In 1969 you published a paper in *Physics Today* called "Is Your Research Moral?".

Schawlow: Oh yes, I have to talk about that next week. I foolishly let myself be inveigled into giving a presentation at a seminar that some undergraduate has organized on scientific ethics. He's gotten all the Nobel Prize winners around Stanford to each

take one session, and mine comes up next week. I'm really very reluctant to talk about that.

Riess: What did you say in the paper?

Schawlow: Have you ever seen it?

Riess: No.

Schawlow: I'll get you a copy of it.

What I said essentially was that people try to blame scientists for the consequences of their research, and that's ridiculous because you can never know what other people will add to what you have done. You just can't really predict the consequences, both good and bad. You just have to have faith that the good consequences will somehow outweigh the bad ones. And that's quite different from development, say, when you're trying to build an atomic bomb. I think people knew what they were doing. On the other hand, discovering the properties of nuclei, the people who did that clearly couldn't accept any responsibility for what was done with it.

Of course, we just mentioned the example of lasers, where people talked right away about death rays, it was a very old idea from comic strips and fiction, but that wasn't what the lasers were like at all. In fact, there have been many good consequences. When I was in Akron and had the pleurisy, Dr. Bird bought one of his respirators and gave it to me because he was grateful because lasers had been used to do an operation on his wife that would have been very difficult without them.

I still think that in the case of lasers there've been all sorts of different applications that surprised me. I couldn't hope to imagine them because I don't know the needs in a lot of these different fields. The progress of lasers in many directions has been quite spectacular. Science is cumulative: everything that one person does is there as a foundation for other people to build on. Having said that, it's about all I have to say.

Riess: Do you think that the ethics debate, or discussion, will end up being very challenging?

Schawlow: I don't know. Of course, they have people there who are in biology and medicine, and well, they have different problems.

One of the things about physics is that the results have to be reproducible. If you faked a result, people would find out, and that's a quick way to ruin your reputation. In some other

branches of physics, particularly high energy physics, it's extremely competitive because there are only a few machines and they're narrowly focused on a few problems. And there, some really dirty work goes on to try and beat out the other guys who are working in that field.

That really doesn't happen in the things I do. As I have told you, I am really one of the least competitive people you ever saw. Unless there's a student that is committed to a particular project, I would just as soon move out of the way and do something different if somebody looks like they're competing with me.

Uses of the Laser, Unusual and Medical

Riess: A number of things come to mind from what you're saying. First of all, having seen the Science in Action video, there's a charming part where you come in with a potato that your wife has suggested could be more efficiently peeled by laser. Was that in the spirit of emphasizing that it's benign?

Schawlow: Well, yes, I did a lot of stuff to show that lasers were really not the death rays. That's one reason I invented the laser eraser, which worked--and I even got a patent on it, at the urging of our contract monitor--but it never got used. But here was something you could build. People were talking about these death rays that you couldn't build, and here was something you could build. If it had ever gone into mass production, it could have been practical to have one built into a typewriter. If you make a mistake, you bring it back to where it was typed, press the zap key, and off it would go.

I didn't intend to patent it or try to make anything of it, but I just wanted it as an example of something you could do. I thought people might take up the idea, but they didn't. First of all, IBM brought in the sticky tape for erasing and then word processor computers really took over.

Riess: Could that ever have been cheap enough? The zap of light? I'm figuring that zap of light's got to cost something each time.

Schawlow: Yes it does, but for a secretary's time when he or she only has to take out a few letters, a few characters, it would be cost effective. I had a letter from a newspaper publisher who publishes the *Army Times*, wanting to know if you could use this for de-inking newsprint for reuse. I did a rough calculation and said I thought the cost of the electricity would be more

than the cost of the paper, even if the lasers cost nothing and were a hundred percent efficient, which they weren't. So it wouldn't have done for that.

I did have a chance to make something out of it: *National Geographic* was, of course, very careful with their books, but they put out one book and they had right in the frontispiece a picture--I think it was either Arizona or New Mexico, but they had put it in the wrong state and they wanted to know if I could erase a hundred thousand copies of this thing. Well, I wasn't set up to do that. I think it could have been done, but I hadn't engineered the thing.

Riess: You mean it could have been done through the layers?

Schawlow: No, you'd open the page and zap the thing that you wanted to get out. It wouldn't take very long, just open the page.

I also got some interesting correspondence. There was a man up in Oregon who was in the lumber business, and he wanted to know if you could use lasers for cutting wood. I wrote back that yes, you could do it, but the lasers we had were too small and inefficient. He said he knew that, but he was trying to look ahead to see what could be done in the future. He was saying that in cutting trees sometimes they'll hit a hard part, or somebody may have put a nail in the tree, and that'll break the saw blade and maybe cause a dangerous accident. He thought the lasers would be better. He was right in a way, but the question of timing--I don't know, I think he died before he got a chance to do anything on that, a few years later.

It certainly is good to look--. I felt the applications have to come mostly from the people who have the needs. And the eye doctors are a great example. I think I've probably said already that neither Charles Townes nor I had ever heard of a detached retina. But the doctors knew about them, and they knew that they could prevent detachment by putting in a flash of bright light. Originally, I think somebody in Switzerland started it with sunlight. And then they used xenon arc lamps. The laser was a brighter light that could be very sharply aimed. So they knew what to do with it right away, and within a couple years of the first lasers they were beginning to use ruby lasers for preventing retinal detachment.

Riess: What does it do? How does it work?

Schawlow: It puts a little scar tissue on that sort of welds the thing together. The retina, I understand, is not really attached to the back of the eye. It's just pressed against it by the fluid, and if it develops a tear then the fluid can seep in

behind it and lift it off, and then you can't focus. In that case, the eye doctor has to go in and turn the eye in the socket and come in from the back. They can do it, but it's a fairly serious operation. But if they get it in time, they can prevent it by using a laser.

The ruby laser wasn't ideal for that purpose. It had an advantage that it didn't hurt, but it wasn't absorbed strongly enough so that sometimes it would penetrate too deeply and rupture a blood vessel, in which case the surgeon would have to take over. But it still did save quite a few people's eyesight. I think Bob Hope was one who had a laser retinal operation.

Riess: So the lasers that end up in the hands of the surgeons get developed for that purpose by some middle person, not the physicist?

Schawlow: Yes, yes, that's right. And I know one company, Optics Technology here in the Palo Alto area, did develop one of the earliest lasers for eye surgery. Dr. Narinder Kapany, a very inventive person, was the president of that company, and he worked with a couple of eye doctors here, Dr. Christian Zweng and Dr. Flocks. There were others, other places, but they were one of the first to do it.

I'm not an engineer. I couldn't have done that, I don't think. Well, if I had dropped everything maybe I could have.

Riess: But you can have a lot of roles in this business. You can be the physicist. You could possibly be the engineer. You also could be the entrepreneur, the developer. There are lots of directions that come out of all of this, and it's interesting that you stay clear of them.

Schawlow: Yes, I did--rather deliberately. I know there were a couple of students in the business school who wanted to form a company to make laser erasers, and I wouldn't have anything to do with it. They were going to raise money.

But if I'd taken on that responsibility, it would've been a full-time job and I wasn't really sure of success, that I could make it practical. Well, you just have to choose, all the time. It's hard. I mean, sometimes you make mistakes. I've certainly made mistakes. I made a *bad* mistake in not trying to build the first laser, which I did know how to do but I just didn't push it, I had a lot of other interesting things to do.

Riess: Do you know yourself well enough to know what really "powers" you, as it were? It's not money, I guess. Money being often the thing that powers people.

Schawlow: Well, as they say, "Money is maybe not the best thing, but it's a long way ahead of whatever's in second place." [laughter]

I guess I have to realize my limitations. I know myself pretty well, but I never can tell when ideas will strike and they may be quite a different direction than what I've been doing. So I enjoy getting at new ideas and trying them out. I'm not sure that I could undertake a linear development job where you have to do one thing after another.

[telephone interruption]

Schawlow: That call was from the new home where Artie lives. They have some problems. They have one young man who's been there for ten years, but lately he's started wandering into somebody's house nearby and they got very upset. They tried various things--even put one of those alarm things on his wrist, or ankle, I don't know. But they've been unable to keep him in there, so they're trying to find another solution, try and rent a house somewhere that he can live in by himself for a while.

Riess: Why do they call you about it?

Schawlow: Well, he called me about various things. I'm a member of the board of directors. I'm vice president, I guess, and was really one of the founders of this place. But he also called about Artie, wanting to know if I was coming up this weekend.

Riess: Now, you were saying that you didn't feel that you were the type to be doing a kind of linear development thing.

Schawlow: Yes. Well, I don't know. I've never really done it. Maybe I could do it. Didn't really want to.

Riess: Do physicists do this? Do they drop out of basic research?

Schawlow: Oh, yes, sure. Lots of them do because there aren't that many jobs in basic research--it's a great privilege to be able to do basic research--so a lot of them go into industry, and lot of them do jobs like that. I have one student who was in various research labs, and he lately was in Livermore. Then he took a job with a company that makes semiconductor lasers. But they want him mostly to do sort of sales engineering--contacting customers and that sort of thing.

Funding and the Military

Riess: Back to Bromberg: one of the things that was interesting to me was the--it's just so obvious--the amount of money that started flowing in and becoming available.

Schawlow: I think a lot of it was wasted.

Riess: One example, though, is ARPA funding TRG.

Schawlow: Yes. Well, TRG and Gould probably did have more insight that lasers could be powerful. In fact, they got the Air Force to support them before any lasers were made after our paper came out. Gould made a deal with them to give them his patent rights, to license them under his patents and give them rights after on anything else he did. But he kept delaying giving them his initial stuff so that he had more stuff that was his. They [TRG] tried to get the Air Force to classify all the work on lasers--this was before anybody had made a laser--and we simply told them that if they did that, we would stop working on it. They didn't.

Riess: What was the dialogue? Who got in touch with whom about that?

Schawlow: I don't know the details, but I think it was initiated by TRG and the Air Force. Which one started it, I don't know. But then somebody, I don't remember who it was, somebody at Bell Labs got word that that's what they were trying to do. Certainly my reaction, and I think others at Bell Labs, said, "Well, if you're going to classify it, we're not going to work on it." Because we wanted to do publishable things. We were trying to build up our reputations in physics.

Riess: But the ABM defense idea?

Schawlow: Oh, that was later, I think. That probably was one of the goals, yes, but that was so far beyond anything, that nobody--. I don't know what went on in the military circles--Charlie would know better than I on that--but I would hope that they had more modest goals than that.

Riess: Well, that's the way Bromberg explains ARPA funding TRG, because they were looking for ABM defense--"Though no laser has yet been demonstrated, lasers were even then being taken into account..."[1]

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[1]*The Laser in America*, p. 82.

And that leads to another book, *The Physicists*.[1] It's a book about physicists in America. The period we're talking about is a period of big money where it's hard to imagine greed not being a real factor.

Schawlow: It really was. A lot of people started going into physics because they thought they could make big money. A lot of physicists took jobs with companies or started their own companies. Oh, a lot of stuff went on.

I had a friend who, a few years earlier, had gotten his Ph.D. at the University of Toronto. I guess he had a teaching job for a while, but then he went with a company that was started in Cambridge, Massachusetts. There was a Harvard associate professor who found that several of his students were getting more money for their initial jobs than he was making. So he decided he was going to make a fortune and he started this company, hired a lot of people, and then sold it after a year or so. And it did make a lot of money because he just had assembled a staff--and although they'd never made any profits.

There was a lot of that. It really didn't touch me much. At Bell Labs we didn't have anything to do with government funding. Not in our department. Bell had a military division at the Whippany Laboratory, but it didn't touch the basic research.

Riess: What about the meetings at exotic locations, jet-setting around, and good salaries at the universities, then, too.

Schawlow: Well perhaps. Certainly, well, I don't know how much that affected--. Of course, there were a lot of people at the universities who took jobs where they were paid mostly or partly by government funds.

There'd been a battle at Stanford. The engineers were into that heavily and engineering at Stanford had grown very big and very good. Our electrical engineering was either the best in the country or maybe MIT some years would be rated better.

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Riess: That's why they could offer so much.

Schawlow: Yes, and could hire so many.

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[1]Daniel J. Kevles, *The Physicists, The History of a Scientific Community in Modern America*, Harvard University Press, 1987.

Riess: The glamour field was high energy physics, which accounted for only one out of every ten physicists, but had a third of the federal funds.

Schawlow: Well it's a very expensive field, and still is. Of course, they turned out Ph.D.s there who couldn't get jobs in that field, and some of them became computer programmers because they had been heavily engaged in computer programming in that field. And some of them went into different fields, I guess. Some of the people who got their Ph.D.s in high energy physics went into lasers because it was growing and had money in it.

I remember a meeting in 1963 at Brooklyn Polytech. The excitement there was really palpable because there were a lot of people there from companies who wanted to know how they could get into this laser field. As I say, I think it was overblown but it was there all right.

Riess: And by the end of the sixties, well--.

Schawlow: Money was getting scarcer. Even when I started in '61 there wasn't as much money as there had been a few years earlier when you really could fund anything. But we managed to find some money to do some research--that was from NASA mostly, and we had small amounts from the Navy and the Army. By the end of the sixties NASA decided that they had to be more selective in what they were doing and they couldn't support us. Fortunately the National Science Foundation was growing and we were able to get in there and get about the same amount of money from them.

Riess: It was not just NASA. The whole country at the end of the sixties was taking a strong dislike to science, whereas they loved it in the early Kennedy years.

Schawlow: Yes, well, it isn't just a dislike to science. I think there's a disenchantment with higher education. Perhaps that came a little later, but--the trouble is that universities had expanded so much that they were turning out an awful lot of people. And there simply were more educated people than there were jobs for them.

I think in the early seventies they made some manpower projections that the teaching at every level, from grade school to college, was saturated and there really weren't going to be any large influx of jobs in teaching for a long time. For centuries that was the chief place where university people went. So I think there began to grow a disenchantment with higher education, which was so expensive, getting more expensive--faster than the cost of living.

I think we're feeling that now and going to feel it more. There was one big disappointment: people thought, looking at the demographics, that there were going to be a lot of people to be educated in the nineties because there was sort of a second baby boom, but it hasn't happened. They thought the universities would have a lot of replacements and openings, but instead they've been squeezed in budgets and they're cutting out programs. Prospects for university teaching are not very good unless you're exceptional.

Riess: That's interesting. So [in the sixties] there was that and then there was the whole bad odor of the military industrial complex.

Schawlow: We had very good relations with the military.

Riess: No, but I mean the swing against science, don't you think that had to do with the military ties?

Schawlow: Well, one of the problems was that the military, the Navy particularly, had taken a very far-sighted view that they wanted to have good relations with the scientists in case some emergency came up and they needed to enlist them the way they'd done in World War II.

They also wanted to sponsor far-out work that might eventually lead to something more dramatic. You know, the question: do you want to improve the sights on the rifle or build an atom bomb? They had that sort of attitude. Partly, yes, they wanted to have good relations with the scientists. They were very good to work for. They didn't try to interfere at all and they had people in their liaison jobs who understood what we were talking about. They didn't try to make us justify everything.

Now Senator Mansfield was worried about the growing influence of the military on universities, and he put through this--the Mansfield Amendment, I think it was called, which required that every project they have at the university have a specific military purpose. And that was really pretty harmful. Well, it didn't hit us very directly because certainly anything having to do with reconnaissance or communication served a military function. Work with lasers, I think, fitted into that pretty nicely without having very specific weapons-related things. But it was typical of the time--he was doing that because he felt that universities were getting too cozy with the military.

Riess: And that that basic research was not--.

Schawlow: Yes, that they didn't always have clear military applications, which was, in my mind, a perfectly sensible way to do it because you couldn't know what was going to come out of this basic research.

But they did support what was then high energy physics. For instance, the high energy physics lab at Stanford which had a one billion volt accelerator, on which Bob Hofstadter did the work for which he got his Nobel Prize, that was mostly sponsored by the Navy. Of course, they had advanced accelerator techniques. It was the first big linear accelerator and it helped in the development of large high power klystrons, spurred the development.

Riess: Nibbling at the edge of the ethics issue is what the goals are. It sounds like it comes with the territory, doesn't it?

Schawlow: Yes. I guess so. Well, my attitude has been expressed in that movie. I think I said that you do science because you think it may benefit mankind in some way, but when you're actually doing it you have to put all that out of your mind and concentrate on the problem itself. If it is basic science you just really can't try to aim it at a particular problem.

On the other hand, it's true that money is available for some things and not for others. And it was available to some extent for lasers because they had, I guess, military application--and one that I never thought of, which actually is the real one, was the target designators, where the plane will send a laser beam at a target and the bomb will home in on that, either from the plane or from somewhere else. I certainly had no idea of that.

Whereas there was not that much money for some other things like maybe cosmic rays--I can't think of examples--acoustics, for instance, things that were not considered very important. Certainly there was money for underwater sound sort of things, ultrasonics, but not, say, for musical acoustics.

Riess: Did you come down on one side or another of the space exploration questions? Have you spoken out?

Schawlow: No, I avoided them. In fact, I didn't even get in a public debate on Star Wars, either, although unfortunately when Reagan made his announcement I think *Time* got hold of something I'd said a few months before about the impracticality of those things. But I didn't say anything more than that.

Riess: And were you a member of the Jason Group?

Schawlow: No, they really were after me and I did go to a couple of their sessions. I think I went twice for a day when they were working on some problem. My work was strictly unclassified and I didn't get involved in anything secret.

Riess: That was your reason, that it would've compromised your ability to--

Schawlow: --work with my students. I guess when it comes right down to it, I don't like having to keep secrets. I like to tell people what I know.

Riess: When you were on the phone with the administrator of the place where your son Artie lives [Cypress Center] it made me think how much you would like, probably, to have made lasers work for him in some way. Have you thought about that?

Schawlow: No, not really. I've often thought it would be nice to have some kind of a laser operation on myself, but fortunately I've avoided that. [chuckle] No, I couldn't see that lasers were going to help.

Riess: I mean for some of the technology for learning.

Schawlow: Well, I did spend a good bit of time trying to use computers [for Artie] without very much success. But I just never could think of any way that I could use lasers for him, so I didn't give it much thought.

Riess: You say you weren't thinking about retinal surgery and yet it's such a wonderful result. Does your imagination run to problem solving or do you stay at the basic level?

Schawlow: I try to stay at the basic level. I'm really interested in fundamental questions in science. No, I don't think much about it. I've been on boards and I've been a consultant to several small companies, but I really haven't contributed much on the technical side. If I do any good, it's mostly from steering them away from crazy things--but not a lot of that, either.

Facilities at Stanford

Riess: Last time we talked about some of the physics faculty at Stanford. A couple of others--I don't know whether they're relevant, but you didn't say much about the Stanford Microwave Lab and Edward Ginzton.

Schawlow: He was already pretty much out of there. The president of Varian Associates--he'd been associated with them from the beginning--but the president was involved in a plane crash and sort of didn't have it any more to really lead the company. So Ginzton had to take over as president--or chairman, I'm not sure. By that time he wasn't spending much time around the university, so I didn't work with him.

Riess: Did the microwave lab continue?

Schawlow: Yes. Oh yes. It's now known as the Ginzton Laboratory. I guess somebody gave some money to rename it. It has been expanded some and it's a good lab. I worked there for a year or so, I think a little more than a year, because the old physics corner was very crowded and the new physics building was planned, but I think they finished it at the end of '62 or '63. And then I had lots of space and moved into the new physics building.

But my contracts were still administered by the microwave lab for many years because they had a very good contract administration. They also had a very good machine shop, which has unfortunately has had to be cut down over the years. They had a good drafting department, too, which is all gone. Now everybody does their own drawings on their computers. I don't think they have any drafting at all. If they have to get something drawn, they would send it out somewhere.

Riess: It sounds complicated--these little fiefdoms and labs.

Schawlow: Well, the Ginzton Lab had a building--it was the microwave lab then. Had some good people there. Tony Siegman was there, and he wrote several good books on masers and lasers. There were some people who had been working on masers and switched to lasers. Siegman had some very good students. One of his students was Steve Harris, who was so good that they kept him on the faculty there and he's done very good things. And one of his students was Robert Byer who also is on the faculty.

Riess: When you came here, it was partly the attraction of the microwave lab?

Schawlow: Well, not really. It was a place for me to work. I never worked very closely with any of those people. They were nice, and we'd talk occasionally, but they were--. I don't know, I was always interested in something different.

Riess: Another one I wondered about was Henry Motz.

Schawlow: Yes, he had built a far infrared or sub-millimeter wave generator using an electron beam. He had left, I think, by the time I came here, he retired. I thought it might be interesting to do something with that, but people around there felt that it was a dead end and they wanted to take it apart and use the space. So I didn't push on that. They were good people, but I never worked very closely with any of them.

What I think was more attractive was that they had people like Bob Hofstadter and Bill Fairbank in the physics department who were doing really outstanding physics. I just wanted to be in that sort of an environment, even though I wouldn't actually work with them on the same problem. But they were people who understood the process of discovery since they had made important discoveries themselves.

Riess: And that was the environment that was really lacking at Bell Labs?

Schawlow: No. They had good physicists at Bell Labs, too. The thing that was lacking at Bell Labs was that they wanted to cover a lot of fields and do nothing very intensively. It's the same thing that Charlie Townes remarks, that they were quite happy to support what he was doing, but they wouldn't let him expand it. So you could do what you could do by yourself with one technician, or occasional collaborations with other people, but you couldn't really get a lot of people working on your ideas.

Riess: What happened to Ali Javan? Did he stay on at Bell Labs?

Schawlow: No, he went to MIT. He stayed at Bell for a while, then he went to MIT fairly early. For some years he didn't produce anything. He's still at MIT and has some good work going on, but he doesn't seem to getting around to publishing it. He did some very good things at MIT. I remember one of the military liaison people said that he's a real national resource.

Riess: I see. You were still at Bell Labs when Ali Javan was there.

Schawlow: Yes, I was there when he, [Donald R.] Bennett, and [William R.] Herriot got the gas laser working. But he [Javan] came in there, spent a lot of money right away. He was going to work on liquid helium and he bought a cryostat and a big magnet, and never used them. He got interested in lasers and had this idea of a gas laser and he managed to persuade them to let these other two people, Bennett and Herriot, work with him.

I remember there was a time when the management was getting worried. Sid Millman, who was the department head, came around and asked me whether I thought this was going to work. They'd

had some results in measuring some gain, though not for laser action. I said, "Oh, yes, I'm sure it's going to work." I guess other people said the same thing, so they continued it, and it did work.

Riess: In Charlie's book, *Making Waves*, in one of his chapters he talks about the interaction between pure and applied science. Last week, when we talked about Stanford, you really made those distinctions quite clear here.

Schawlow: Yet it's nice to be able to move back and forth. I think some places they would have called what I was doing applied science, but we didn't have to make that distinction. I certainly would distinguish between engineering and physics. And in fact, I really don't know what applied science is. I think, at best, it's just applicable science, science that has some fairly obvious application to some problem in technology. Certainly, science benefits from technology, just as technology benefits from science, so we could get advanced equipment at various times. Of course, the rapid rise of digital measuring equipment is one example of that, although that didn't really come in until the seventies.

Riess: Did you have meetings, or tea with your equivalents in applied sciences?

Schawlow: Yes, there was a seminar. For a while, I organized a seminar in lasers and a lot of people came to it from companies around and that sort of thing. I guess Siegman probably took that over. I stopped going to it. We had our own group, people who were working for me. We would meet once a week and I would have various students talk about what they were doing and have discussions there. We did that even at Toronto, and certainly Charlie did that very successfully at Columbia.

Riess: But this was a way to get the applied science people together with the pure science people?

Schawlow: That first one was, to get various people together. But I think I sort of came to the conclusion that it didn't really have a lot to offer me, directly. Of course, in the beginning I was interested in everything that had the word laser or optical maser in it. I used to collect every scrap of newsprint, newspaper, or anything like. But then the field just grew so rapidly, you couldn't follow everything; you had to realize what it was you were doing and what you weren't doing.

So I sort of drew back into myself rather than trying to communicate with the others. What they were doing wasn't the

same thing I was doing and my work in the sixties was mostly on spectroscopy related to laser material. We did a little bit of work with lasers, but not an awful lot--using lasers for scientific stuff.

Riess: I have to ask you whether diamonds wouldn't have been just the most perfect thing in lasers.

Schawlow: Well, they don't work. They have some advantages, but pure diamond wouldn't emit light anywhere near the visible or have any absorption bands. It's quite transparent way out at the ultraviolet. Now, most gem diamonds have various impurities, so they will glow. Diamond, I think has been made to lase since then. Yes, I think Steve Rand at University of Michigan did get diamond to lase. But of course, they're very small and their optical properties differ from one kind to another. I think they just didn't have the right absorption and emission characteristics for lasing action.

Riess: But they're hard.

Schawlow: Yes, and that really became apparent, that it was important to have something rather hard or it wouldn't stand up to the thermal shock when you fired them.

It was not possible to grow diamonds at that time, and you couldn't put in the right kind of doping the way you can in sapphire. Artificial sapphires and rubies had been grown for a long time and they knew how to put various impurities in them.

"Science in Action" and Other Honors

Riess: Why were you chosen as the quintessential scientist for the "Science in Action" program? Tell me about that.

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Schawlow: I really don't know why they chose me for the scientist. It may be that I had a reputation for being entertaining, being a fairly good lecturer, and I had done something important. But I don't know. They didn't discuss it with me. And I don't know who made the decision, really.

"Science in Action" was a program produced, I think, by the California Academy of Sciences--you know, the museum in Golden Gate Park. Dr. Herrold was the organizer and he was the master of ceremonies. They had me on there in January, 1962, just a

few months after I came. That was the time Frank Imbusch and Linn Mollenauer, my two students, went with me and we had a crude laser to demonstrate.

I guess I found that I could break a blue balloon with it and thought I would demonstrate that. But the students put on the balloon--it was a sausage-shaped balloon which was standing upright--a hammer and sickle and Sputnik or something like that. I was rather horrified but I thought, "Oh well, maybe you won't be able to see that." Fortunately, it worked. If it hadn't worked, it might've been bad. But when I saw the kinescope later, they had zoomed right in on the thing and you could see it relatively clearly. But I didn't emphasize it in my talk.

That was made in the worst possible way for the performer because they broadcasted it live and did a kinescope, a film, which they then rebroadcast other places. So if you made a mistake, it would not only be shown live but would be repeated. But I didn't have any feedback on that. That was 1962. By late 1965--by that time the show had been running for some years and they had decided to have an independent producer produce it. He was the one who approached me and asked if I'd like to do this. I said okay. He was in charge of it. It didn't have any direct connection with the Academy of Sciences. They were nearly at the end of their run; I think they were going to finish in a few more shows.

Riess: Did it have commercial sponsorship and all of that?

Schawlow: No, I don't think so. It was on Channel 9 and some other educational tv places around. The guy who produced it did get an award for it from some national organization, but I never heard the details of exactly what the award was.

Riess: I thought it was perhaps like an earlier version of "Nova."

Schawlow: No.

[pause]

Riess: Also in the early sixties you received the Ballantine Medal.

Schawlow: And the Thomas Young Medal.

Yes, and then in '63 I was at a meeting and one of Charlie Townes' former students said, "You don't think you're going to get a Nobel Prize, do you?" I told him truthfully that I'd been nominated but I didn't know. Then in October of '64, the university newspeople came and said they'd gotten a tip that I

was going to get it and share it with Townes and Maiman the next day. So they came and took pictures in my class. I told the class that I didn't know what it was for, which was not true. I then told them truthfully that the last time they'd taken pictures in my class, it had appeared in the university's annual report as an example of expenditures. [laughter]

Riess: [laughs] You're good!

Schawlow: Well, it's true. But then I had a kind of sleepless night. Turned out they gave it to Townes--with [Nikolai] Basov and [Alexander] Prokhorov, the Russians--for the maser/laser principle. And you can't split it more than three ways.

After that I thought, well I'm not going to get one. I'd stopped worrying about it--hadn't been worrying about it much anyway--just go on and do my thing as best I can. When it came seventeen years later in '81 I was surprised to find that they had given me a Nobel Prize for contributions to laser spectroscopy.

Riess: Oh dear! Does this make sense, the first award?

Schawlow: Well, they can only include three, you see. As one of the Nobel committee people told me many years later, he thought they'd made a mistake including the laser in that. Because they could've given it just for the maser.

Riess: Including the laser really does include you.

Schawlow: Yes, yes. So when they gave it to me, in the announcement--they have some committee member make a little speech. He said that the step from the maser to the laser was made by Schawlow and Townes, something like that. So I don't know.

Also, in talking with one of the Nobel people, he said that it had been a close thing. It could be that the committee may have recommended one thing, but then the Academy may have overruled them. They can do that at the last minute. It isn't official until the Swedish Academy approves it. But I don't know.

Certainly the Russians had been lobbying hard. In fact, at the Quantum Electronics Conference in Paris of 1963, Basov came up to me and said, "I'd like to discuss with you and Townes who in this field should get the Nobel Prize."

Riess: Was this being funny?

Schawlow: No. He was dead serious. So I sort of joked and said, "Well, I'm pretty sure I know who Charlie Townes thinks should get the Nobel Prize"--meaning himself, of course, which he should. But I don't know just what all they did. The Russians do tend to lobby.

I couldn't object. The maser did come before the laser, and it was an important step, although what the Russians did was less than what Charlie had already done. But they got it in print first. Anyway, so I just forgot about getting a Nobel Prize. I remember telling people--several times people asked if I had a Nobel Prize, and I explained why it had been given for the maser/laser principle, that the maser had come first, and so I wasn't going to get one. I was pleasantly surprised when they did find a way to give me one. They did share it with Bloembergen, who had invented the tunable solid state maser which was the important one for radar and communications. He shared it with me and he was also overdue, I think.

They do a great job, I think, with the Nobel committees. They had this Nobel reunion in 1991 to mark the ninetieth anniversary of the Nobel Prize. I was talking there with one of the people who had been on the committee when I got mine, and I was saying I thought the committee had a great job. He said, "Oh well, we made some mistakes." [laughter]

Riess: It can't help but be somewhat political.

Schawlow: Well, I certainly never lobbied for it. I was really quite surprised.

Some Russian Physicists

Riess: The old view of Russia and the Cold War was such that when Charlie talked about meeting with Basov and Prokhorov I was shocked, or titillated, because the Russians were such "nonpeople" to the ordinary citizen of this country.

Schawlow: Yes, well Charlie tried to make friendly relations with them. I think he really believed that the scientist could do some good by talking with the Russians, because they could communicate on some level. And he was probably right on that.

We did have some Russian visitors. We actually had a couple of Russians work in our lab for a couple of months, I guess three months each. We were quite friendly, but I never talked about politics or anything like that with them.

Riess: But this country was fighting the Cold War. What were your feelings about that?

Schawlow: I never went to Russia. I heard so many horror stories about how unpleasant it was. At one point, I got an invitation from some ministry of machinery, or something like that, to go and give talks and I consulted the CIA to ask how I should respond to that. They said, "Write and ask them if you could visit some laboratories." And I heard nothing more from them after that. But then later there were scientific meetings held there. Of course, Basov and Prokhorov did come to the first Quantum Electronics Conference, and we met other Russians at other conferences--[V.S.] Letokhov and Chebotayev were among them.

Chebotayev was a very nice person. Everybody liked him and he was a very good scientist, too. After the Soviet breakup, when things got so very poor, support for science and everything in Russia, University of Arizona was trying to hire him. I think he had about decided to go there, but then he had a heart attack. In fact, he was there visiting and had a heart attack, and died. Not very old, fifty-ish I think.

Letokhov is quite a bright guy, very ambitious, and I think quite powerful in Russian science. More of an operator than Chebotayev was, but they're both very good scientists.

Riess: And what about the political system, what about communism? Is it the scourge that it was made out to be?

Schawlow: Well, I never had any doubt about that. I had no use for communism, even back in the thirties, I just couldn't see how anybody could fall for that.

It was interesting: at least one of our visitors told me that he had read some books here that he couldn't read at home. Maybe *Gulag Archipelago*, I'm not sure. I didn't try to draw him out on that because I felt that if I got him into trouble, I couldn't protect him. I think some of the scientists did kind of incite the people on the other side of the Iron Curtain, or the Bamboo Curtain to speak out, and they got their heads chopped off. Their scientific status could only protect them so far--not too far. I didn't want to urge anybody to do that. I just treated these Russians as individuals.

I think Charlie had made a real effort to establish good relations with them. Peter Franken at the University of Arizona did too. I gather since the breakup he's been very active in taking money over to help support some of the Russian scientists. He says he has travelled carrying big bags of

money with government permission because there's not any other good way to send it.

Riess: We were trying to get so many of them to come to this country.

Schawlow: Well, a lot of them did, but there are a limited number of jobs, we haven't got jobs for everybody. And I don't think we want to destroy Russian science. I certainly have learned a lot from Russian science and technology. For a long time they were very good in theory, they had a lot of ideas, but they didn't seem to have the ability to carry out the experiments as quickly as we could.

Riess: Is that because their bureaucratic structure is even worse than ours?

Schawlow: It is, and also they didn't have the industry. In fact, I asked Gorbachev about that when he visited here. A number of us were invited to sit around a table and he would answer questions--they [the questions] were screened ahead of time.

I asked him--recently there had been a girl from the Soviet Union who came to Stanford for an operation. She had a large blood tumor that was removed by a laser. From the beginning of lasers, as soon as there was anything at all, various people had ideas to make instruments. They'd start a little company to make them. They hoped to make money. Some of them did, some didn't. But it meant that all these instruments were very quickly available to us.

I asked, "Can you do that sort of thing in the Soviet Union?" Well, he sort of evaded that. He said, "Well, we hope to make things available quickly." But in fact, they built a lot more stuff at the laboratories because they did not have the instrument industry that grew up rapidly here. They had to have much larger support staffs and build things within the institutes. The different ministries were insulated from each other. I don't think that the Academy of Sciences people could talk to the people in the Department of Machine Building, or something like that.

The compartmentalization of Soviet science was a big drawback, I think. If they really put the money into something like the space program, then they'd build everything within that organization and could do very well. But for independent science--they did have much larger staffs than we did, but they didn't have all the marketplace to draw on that we had.

Riess: Did scientists there have the same kind of liaison with the government that Charlie describes when he was on the science advisory panel? Do they have that in the Soviet Union?

Schawlow: I'm not sure how they do it. They have this Academy of Sciences which is quite different from our Academy. It's very powerful, it runs a lot of institutes, has it's own budget, where our academy is strictly an honorary thing--it can do studies, but no more. "Le'-to-khov"--or "Le-tō'-kov" because in Russian it's "Le-tō'-kov," but he says that when he comes to the west it's "Le'-to-kov," because we tend to put the accent on the first syllable--he mentioned meeting with Brezhnev. So the favored scientist did have access to quite high officials, but I don't know the details.

I know Basov became very powerful. Apparently there was some power struggle between him and Prokhorov, and he became head of the Lebedev Institute, which is a very large institute for research on electronics and then lasers. After that, Prokhorov got his own institute, the General Physics Institute, and he had a large group. I noticed one year his name was on more than twenty papers; I'm sure he didn't do all of those himself, but he had a big group working. They both did some nice things. They're both very capable people, and a lot of the other Russian scientists are too.

Anyway, I think it was a good thing that people like Charlie feel that they need to keep in touch with the Russians and build bridges as much as they can. But I didn't feel it was something I could do.

Riess: That reminds me of the state of physics in Russia at the end of the Cold War. Have you brought Russian physicists to Stanford?

Schawlow: The department has. We have a theoretical astrophysicist, Andre Linde, who is very distinguished, and his wife who is a nuclear physicist. They have been added to our faculty. I don't know, there are probably others around the place too, but I don't know for sure. But I haven't been in a position to find jobs for anybody.

There are a number of Russian scientists around various places, but we have only the two. Ours is a very small department compared to most other major physics departments.

People and Projects

[Interview 7: November 14, 1996] ##

Optical Science

Riess: So much of your work has been taken up by people in the field of optical science. What do physicists think of the optical scientists?

Schawlow: The physics community thought of optics people as being lens grinders--[chuckle] a lot of them were. The Optical Society is a very diverse mixture of people, a lot of people interested in vision, and some in color imagery. And oh my, those people were fussy about terminology. You had to use the exact proper terms, and they didn't always agree on which ones they were.

Mary Warga was the executive secretary of the Optical Society. She had been a professor at University of Pittsburgh and had worked in spectroscopy, I think analytical spectroscopy, I'm not sure--that is, analyzing compounds, or alloys. However, she was full time with the Optical Society and when the first lasers operated she really went after them to get the laser people into the Optical Society. She came to Bell Labs and visited with a number of people there.

She got Maiman to give a talk at the first meeting that they held after his announcement--on very short notice, but she did it. Then they had another meeting in Pittsburgh in the following spring and a lot of invited papers on lasers. I gather some of the old line optics people got rather annoyed at that.

Riess: It's interesting, you say with a smile that they were lens grinders. At the same time the question of coherent light, must interest people in optics.

Schawlow: Well, of course they didn't have sources of coherent light before that.

Emil Wolf at the University of Rochester has done a lot of theoretical work on partial coherence of light. You can get it. After all, the famous Young experiment which sends light through two slits and has them interfere on a screen some distance beyond that, really requires that the light reaching the two slits be somewhat coherent. They've done that since Young's time which was the early 1800s. Every undergraduate physics course does it.

But the way we always did it was that we'd use a small source, have a narrow slit in front of the source, have a filter so that you'd get a narrow range of wavelengths. Then the light reaching the second pair of slits, which was maybe several meters away, would be only waves that are going almost entirely in one direction. So they would have a plane wavefront and they'd be coherent, nearly enough, across the two slits. So you'd get partial coherence.

With the laser we had a source of coherent light which was something quite different. And that stimulated a lot of work, a lot of physics questions in connection with lasers. It also suggested a lot--I didn't get into that much, but it did suggest interesting studies of materials related to laser material. There'd been work on that for many years in physics, and chemistry. Again, not a forefront sort of thing, but it interested me when I got interested in lasers. Actually the first nine or ten years that I was at Stanford, that was really the main focus of what we did, at least a lot of what we did.

For instance, I had found at Bell Labs that the extra lines, so-called satellite or neighbor lines, in the spectrum of ruby were caused by chromium ion pairs. Looking at the crystal structure, you could see that there were a number of different sites, that the neighbor could be in different directions. It could be right along the symmetry axis or off to one side in various directions.

Mollenauer, Imbusch, Emmett, McCall

Schawlow: I got Linn Mollenauer to work on trying to unravel that by applying stress to the crystal, just putting a weight, a piston pushing on the thing. We could see the line shifted in various ways. The direction in which you get the maximum shift would be along the direction of the particular pair.

Mollenauer was actually my first student and has done very well at Bell Labs. He worked for a while as assistant professor at Berkeley, but then he went to Bell Labs. He's done a lot of work on optical solutions which are very good for long-distance high speed communication over fiber optics. They're a serious competitor--that they may be the system of the future, although there are other competitors. But they've shown remarkable results.

Riess: You came to Stanford planning to work on spectroscopy.

Schawlow: Mostly that. I don't know, there wasn't anything very systematic. I attracted a lot of students. When I came I attracted students really too fast. I hate to say no to anybody.

I gave them various problems that occurred to me. We were kind of exploring. I should mention that somebody asked me, about the time I was coming here, how I was going to compete with Bell Labs when they had so many good people working on lasers. I said, "Well, it's simple. I won't compete. I'll do something different." That may have been part of the reason why I didn't really work on trying to find new laser materials and instead I worked on trying to clear up some of the physics questions that were suggested by lasers.

Riess: Did you finish talking about the chromium ion pairs?

Schawlow: That was one of the things we did. Gosh, my memory begins to fail me. I have a time remembering exactly what each student did now, some of them, although I can remember most of them. Frank Imbusch was the second student.

Mollenauer and Imbusch had been working with George Pake, but Pake was leaving--he went to be provost at Washington University--so they asked to work with me. Imbusch was from Ireland, and is back there now, at the University of Galway. He was very good at getting things done. Mollenauer was rather slow, but deep. He always saw things a level deeper than I had thought about them. But Imbusch was quick for getting things done, and so we did a number of "Oh, let's try this out" kind of experiments.

Riess: Would it happen in the lab or would you sit around?

Schawlow: We would have meetings every week, a group meeting to discuss things. I would talk to the students some, go around and visit them in the lab or they'd come and see me to discuss what they might do next. Mostly they had a lot of freedom to do pretty much whatever they wanted to.

I guess mostly I would ask questions and sometimes make a comment, a suggestion. I'd have these seminars, group meetings, and have the students talk. I would ask "dumb" questions once in a while to make sure that other students there understood what they were saying, and perhaps even to clarify their own thinking. I was really not ashamed to ask stupid questions because I knew that other students in the group were working on different things and they didn't know.

Then, one student came along, John Emmett, the red-headed guy in the movie ["Science in Action"]. He had come from Caltech and apparently had a sort of checkered reputation there. He'd done very well at the things he was interested in, and not bothered with other things. However he'd gotten through all right.

He was really a strange one in some ways. Once he sort of disappeared for some months. I didn't see him, so when he reappeared I asked him to give a talk at our group meeting. It turned out that he had his own machine shop at home and he was building parts for a big laser.

He knew more about flash lamps, I think, than anybody else in the world--the kind of flash lamps used for pumping lasers. In fact, Elliot Weinberg, who was our contact with the Office of Naval Research, put in some extra money to support Emmett's research, and Weinberg took Emmett with him to Europe to visit various laboratories where they were working on laser flashlamps.

He really liked to build things, Emmett did, and he built a big powerful ruby laser. It was rather expensive work, even with the Navy money it was very expensive, the things he did. He built a high-powered ruby laser which used a rod of ruby that was something like six inches long and three-quarters of an inch in diameter. I think they cost about two thousand dollars each. They're, of course, synthetic ruby.

The way he was using them they produce ultrashort pulses that would only last a few nanoseconds. I figured these rods would get destroyed by the high powered light flashes in maybe a thousand flashes. I think there are about a thousand flashes, each about two nanoseconds, so we'd get about two thousand nanoseconds out of this $2,000.

It was costing us about a billion dollars a second to run this thing and I told him that. He said, "Gee, boss, I realize that I've been here a couple of years and have only done a few microseconds of real work." I said, "I've been suspecting something of the sort." [chuckles]

It was very hard to get Emmett to actually measure anything. Elliot Weinberg wanted him to measure whether flashlamps were opaque to their own radiation in the reddish sort of region that was used particularly for pumping neodymium

gas lasers and neodymium YAG lasers.[1] Emmett had the apparatus, and set it all up--he could have done it better if he'd had another laser to probe the absorption of the discharge, but he used a short flash lamp, a very clever thing.

Weinberg came in on Saturdays to make Emmett sit down and actually take the measurements. That's why I put Weinberg's name as coauthor on a paper sponsored by NASA, even though he was working for the Office of Naval Research!

Emmett told me years later that he really didn't want to finish anything. He was afraid if he finished anything I'd make him leave, and he was just having too much fun there.

Riess: It makes me think of George Devlin, another clever scientist you worked with. Is that a question: whether or not it is important to make them into well-rounded physicists or whether that's even within their capacity?

Schawlow: Well, you try and get them to do what they can do. You make them as well-rounded as you can.

Emmett, after he left us, went to the Naval Research Lab in Washington and worked on high powered lasers for a while. Then when Livermore was starting to get into big lasers for nuclear fusion he went there and eventually became the associate director in charge of all their laser programs at Livermore. After some years he left there and started a business which I gather he sold and made a lot of money and is doing various consulting. A very clever guy, but it was sort of like having a tiger by the tail, a nice tiger, but--[chuckle]--a tiger. You really couldn't control him.

I wanted to explore various things. I got interested in the far infrared region which was still a big hole that hadn't been bridged really. We jumped from the microwave right to the visible and near visible. I had one student build a big far infrared spectrometer and I actually bought a gas laser using cyanide gas.

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[1] [from Interview 5] I probably ought to admit it, that was one of my big mistakes. Professor Dieke at Johns Hopkins had been working on spectra of rare earth ions and crystals, and he told me that I ought to try neodymium. I looked at the spectrum and thought, "Oh, that's awfully complicated, that doesn't look very promising." But other people did, and neodymium is still one of the best ions to use. You can't put it in sapphire, but you can in a lot of crystals--and in glass, too. But I didn't try it and others discovered that independently. [Schawlow]

That's a funny story. It turned out what they were using was methyl cyanide which is a liquid, and as such is not too poisonous. But if you're going to vaporize it as you would in gas lasers, you have to have good ventilation, which we did.

But it turned out that Emmett had been using acetonitrile as a liquid to measure the energy of his laser pulses. He put it in a flask with a tube coming up that measured the expansion when a laser pulse struck it. It turned out that acetonitrile was methyl cyanide--it's the same stuff.

We thought originally that this laser--the people who invented it thought it used the CN radical, and that would have a magnetic moment, so it could perhaps be tuned by applying a magnetic field. But by the time we really got under way, other studies had shown that it wasn't that, it was the HCN which was produced somehow in the discharge, another really nasty gas, but that is not a free radical and wouldn't be magnetically tunable. So nothing much came of that. We did some work on tuning the thing as much as you could by going to different transitions.

Bruce McCall was a student who worked with that, and McCall was an unusual one. He started out with me. He came from a fairly wealthy family, of auto parts manufacturers in the Detroit area. He was wrestling with the question of whether he should go instead to the business school. He was admitted to Harvard Business School and he went there and got an MBA, but he came back. He finished working on this cyanide laser.

He later started his own company, Molectron, which wasn't terribly successful, except that he eventually sold it at a good price because they had begun to develop a device for using lasers for treating stomach ulcers. Cooper Laboratories was sort of collecting laser medical companies, so they bought this at a good price.

Riess: This was still in the sixties or was this in the seventies?

Schawlow: It was probably in the seventies when they had this company.

Riess: Seems like something people might have been tempted to do a lot of, grabbing at something, turning it into a company, and into a profit.

Schawlow: Yes, but I don't think that was his intention. He did make infrared detectors, and the detector part was spun off by Cooper Labs. There is now still a Molectron detector company, but he has nothing to do with it. They made lasers of various kinds, but they didn't sell very many of them, I don't think.

And it was sort of just scraping along until they started on this medical thing, and that was salable.

Riess: When you have an idea, like the methyl cyanide, do you have to know where you're going to go with it?

Schawlow: Emmett was using it just as a liquid that had a fairly low heat capacity and would expand when the laser pulse hit it, just as a kind of thermometer. But when we bought that cyanide laser, I thought that it worked with the CN radical and that it could be tuned by magnetic field. That turned out to be wrong, so we just did a little more exploring of its properties and closed that off.

Titanium in Ruby Rods

Schawlow: We built a far infrared spectrometer. We used that for studying crystals, looking for lines from ions in crystals. It was related to the work that we'd been doing in the visible region. Some of these ruby lines were separated by intervals that would correspond to transitions in the far infrared region. We tried to see whether they really were from the same pair of ions or from different pairs, whether they just accidentally happened to be near each other.

In fact, we had originally reported that they were from the same one, but we were beginning to doubt that. And we did get a big rod of dark ruby, about six inches long, and looked through it, and the particular line we were concerned about was not there. So we looked at crystals with other transition metal ions, trying to see if it was an impurity, things in the iron series, like titanium. Well, we got a crystal of titanium and there was that line.

A few years later we had a visit from a man named Otto Deutschbein. Deutschbein must have been German originally. He had written his Ph.D. thesis in the early thirties. He had done a lot of work on the spectrum of ruby and other crystals related to it with these transition metal ions. We told him about this, that this line turns out to be titanium rather than chromium. And he said, "That's interesting." By that time, he was at the French Post Office, which is the big communications lab in France.

He said, "You know, it's interesting. Djevahirdjian in Switzerland has made a lot of ruby rods that are used in lasers in Europe." He had sent samples of his rods to the laboratory

at the French Post Office, and they had found titanium in them and they reported that to Djevahirdjian, and he said, "Oh, goodness! Don't tell anybody. That's my secret." The titanium helps the crystals grow better into the large crystals.

Riess: You might decide to just go through every material and come up with similar kinds of data about it.

Schawlow: It would have been better, actually. We had crystals of titanium-doped sapphire, and this turns out to be a very good laser material. Well, we didn't even try it as a laser material.

Riess: So that wouldn't be an approach?

Schawlow: It could have been, but we didn't. I don't know, I was very opportunistic, I just sort of tried various things. I really wasn't well focused, didn't plan, wasn't systematic. I just tried whatever happened to look interesting at the time.

Riess: There is something about the process here that makes me curious. Whenever you have an idea, you need to get money for it, and you need to write a proposal?

Schawlow: No. I would only work with the things that were vague enough that I could have a good bit of latitude to do what I wanted. Even when I did propose something fairly definite, if I did something different they would accept it in those days. So I just didn't write special proposals for each project. I never really had enough money, but I managed to scrape by on what we had. I had very few postdocs because I really felt I couldn't afford them. But we had some.

We did do some things on rare earth ions in crystals. In fact, I had a student working on crystals with praseodymium and lanthanum fluoride. I was consulting with Varian Associates, and they had somebody there who liked to grow crystals of lanthanum fluoride. He gave us some samples with various rare earth materials in them and we did various things with them.

Many years later, I had another student who was doing some work with that, which was suggested not by me but by a postdoc, Steve Rand. And I went to look up some information about this crystal and I was rather surprised to find that my name was on a paper back around 1963 about this very crystal. The work had been done though mostly by Bill Yen, who was a postdoc, and by that time was a professor--first at Wisconsin and then the University of Georgia.

Riess: Yes, he's a contributor to this book dedicated to you, isn't he? It says he joined the Schawlow group in the summer of 1962 as a research associate, "increasing the size of the team to four."[1] The four would have been Imbusch, Mollenauer--.

Schawlow: Somewhere around there I got Warren Moos from Michigan.

These people sort of were offered to me. At that time, I was still just kind of drawing on the money that was available in the microwave lab, not really worrying about budgets yet. Then I began to get my own contracts and had to worry about budgets.

Light-Controlled Chemical Reactions

Schawlow: There are two things I should mention. As you've probably learned from Charlie Townes, one of the things that stimulated my interest in how to get sources of shorter wavelengths, and also brought me to Columbia, was the Carbide and Carbon Chemical Fellowship, which had been started by Helmut Schulz.

Schulz had a vague idea that you could control chemical reactions by light of some wavelength between far infrared and visible. That was really the only application that we had vaguely in mind when we were working on the idea of a laser. So I thought I would like to do something on that.

I got one student, Bill Tiffany, to try and study a reaction that might be stimulated by a ruby laser, which was essentially the only laser that we had, really, in those days. We tried looking at reactions in bromine with ethylene, I think. We found that you could tune this laser by changing its temperature. As you scan across the spectrum, a lot of lines are drawn in the bromine, but when you're on a line you could get a reaction. If you're off a line, you wouldn't get a reaction. So it could be isotope-selective.

In the end we didn't get any separation. There were fast chain reactions that scrambled the isotopes before we could extract them. Actually, chemists knew about that sort of thing, but we didn't. So in the end, we did initiate the action selectively, but we couldn't complete it.

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[1] *Lasers, Spectroscopy and New Ideas, A Tribute to Arthur L. Schawlow*, Editors, W.M. Yen and M.D. Levenson, Springer Series in Optical Sciences, Volume 54, Springer-Verlag, 1987.

Riess: Did you consult with chemists?

Schawlow: Only to get the samples analyzed. I don't think there was anybody there [in the chemistry department] who was particularly interested in this sort of thing at the time. They did use a mass spectrograph to analyze what we were getting.

However, after Bill Tiffany finished, I couldn't find other students that wanted to work on chemistry--it wasn't physics, it was chemistry. I also began to have qualms. It would be okay to separate bromine isotopes, but if anybody found an easy way to separate uranium isotopes that would be a real disaster. So I decided I just wouldn't work on isotope separation of any kind, because I might have a good idea that made it easy, and that would be terrible.

Riess: You never published?

Schawlow: We published what did on the bromine, but we didn't do anything further. What I know now is that you really have to do those things fast. There's a lot of work done on laser isotope separation. Indeed, if they ever need to separate more uranium isotopes they would probably build a plant using lasers to separate it, rather than the diffusion or mass spectrographs that they used before. Livermore did a lot of work in that later. There was some done at Hanford, too.

It's okay for the government in their big secret labs to work on that sort of thing--anyway, I thought it best that I just not touch it. The way they do it is not easy. It's quite difficult. On the other hand, Dick Zare has done work separating chlorine isotopes. That's apparently very easy. If it were as easy to do in a garage, if it were that easy to separate uranium isotopes, that would be a disaster.

Riess: These issues and concerns make me thing about the Pugwash conferences. Did you attend them?

Schawlow: No, I never did. Never got involved, never got invited. I think we talked before about how I really kept out of government stuff, largely by refusing secrecy. I think, also, I was spending an awful lot of time with Artie and with my classes and my students and so on. I just didn't have any energy left over to do those things. Probably I didn't get invited because I wasn't involved with the government. If I'd been asked, it would have been hard to refuse. But fortunately I wasn't asked.

Consultancy at Varian

Riess: Also, consulting with Varian. Was that ongoing?

Schawlow: No, I gave it up after a while. I had become a director of Optics Technology, which was a struggling little company run by a man named Narinder Kapany. He was a Sikh who had gotten a Ph.D. at Imperial College, London. He was a man with a lot of ideas, but it was a badly run company I'm afraid, which I couldn't do much about. They had a lot of clever ideas, and every year he'd have a different product which had a different market. So they lost money and eventually went bankrupt.

But he wanted me to consult with them full-time at a time when things were prosperous. And I felt the Varian thing wasn't getting anywhere. They didn't have a real commitment to basic research. Several times they decided they were going to make gas lasers, and then decided they weren't, so I didn't feel that was really very interesting.

Riess: What do you do, as a consultant?

Schawlow: You just go over there and they tell you what they're doing. Maybe they ask some questions. Maybe you can answer them, maybe not.

Riess: You might have a number of consultancies?

Schawlow: Could have, as long as they didn't conflict. At one point, Hewlett-Packard wanted to start making microwave spectrographs and wanted me to consult. I felt that that wasn't fair because that might compete with something that Varian was doing. I mean, they're both in the instrument business. So I didn't take that one. But I could have done things that were not competing.

Riess: When you're on a board of a small scientific company, don't you end up being a scientist on that board?

Schawlow: Sometimes, yes. Sometimes you make technical comments and sometimes you come in and talk with people in the lab occasionally. But in principle, the board has to set policy. Kapany was a strong leader and I really don't think I did much good. I think I sort of wasted my time. Ended up making no money at all.

Riess: Did Stanford make policy about how physicists were or were not to be involved with the larger community?

Schawlow: The had some policy, I think largely driven by the engineering department where they had some professors that were running companies at the same time. I think they had a rule that you couldn't spend more than one day a week on the average consulting. I spent a good deal less than that.

I'm afraid that I don't think I really did anybody much good with my consulting--maybe helping them avoid making some mistakes. I don't know, their problems just didn't really turn me on very much.

The Hodgepodge of Projects, Ray Guns, Full House in The Lab

Riess: You were very engaged in what you were doing in those years, the sixties, at Stanford?

Schawlow: Yes, I was enjoying it, it was interesting. I didn't think the individual projects were terribly exciting to people in other branches of physics.

Much of our work was exploring the spectroscopic properties of transparent crystals containing rare earth ions. Many of the lasers existing then used these crystals, among them ruby. The spectra are the raw materials from which you may be able to make lasers or other devices. We didn't have widely tunable lasers yet, and so we worked mostly with high-resolution grating spectrographs.

Sometime in the late sixties, Roger MacFarlane joined us. He had obtained his Ph.D. in New Zealand, and knew much more than I did about the theory of these spectra. He was also a good experimentalist, too. After about two years with us, he went to the IBM research laboratory in San Jose, California and is still there. He has done very nice things through the years, and collaborated with us in the 1990s when we once again turned our attention to ions in solids.

One thing that did happen in our lab--I wasn't really the initiator, it was a man named Robert White, an assistant professor, who suggested that they look at manganese fluoride. Manganese fluoride is an anti-ferromagnetic material at low temperatures. That means that instead of all the electron spins being lined up parallel as they are in a ferromagnet, they are lined up anti-parallel. But there could be spin waves in this thing. White suggested that students look for spin wave side bands, and indeed, they found them. That was quite a nice thing and it surprised a lot of people.

Somehow, I felt that what we were doing was kind of a hodgepodge of stuff in the sixties--but each one was fun. I did get invited to give the Richtmyer Lecture at the joint meeting of the American Association of Physics Teachers and the American Physical Society in 1970, I think. I chose for that a title, "Is Spectroscopy Dead?" Laser spectroscopy hadn't really begun yet.

I remember asking various people what they thought--I asked colleagues if they had ideas. Felix Bloch came right to the point. He said, "What do you mean by dead?" I said, "Oh, turned over to chemists." [laughter] That had happened to microwave spectroscopy. No physicists were working on microwave spectroscopy after our book came out, I think. That pretty much killed it. Everybody thought, "Well, it's all done. All the physics is done." But the chemists were more interested in looking at a lot of different molecules with microwave spectroscopy.

Riess: Chemists, or physicists who are interested in chemistry?

Schawlow: Usually they were chemists who were interested in the physics of things.

So, anyway, this was a great honor. I didn't give a very good talk, and I never did get the manuscript written up, which I was supposed to do. I had the flu--I got the flu when I went to this meeting in Chicago. It turned out that Luis Alvarez had been president of the American Physical Society, and he was supposed to give his retiring presidential address. But he had the flu so bad that he couldn't give his address, so I was allowed to ramble on a little beyond my allotted time. [laughs] But I had the flu and I was not feeling well at all.

They said that Alvarez was there and was being attended by his famous father--you know, Walter Alvarez, a very famous doctor at the Mayo Clinic. Although what we had done was rather hodgepodge, people thought it added up to something.

[looking through a list of his publications] We were starting work on measuring the position and width of the spectral lines--with Imbusch, again, and some people at Bell Labs. I guess that was after Imbusch had gone to Bell Labs. He was there for a couple of years.

Riess: I imagine that all of this writing of papers took a huge amount of time.

Schawlow: It does, but in many cases a student or a postdoc would do some of the work.

I see there's one here about a portable demonstration laser that I wrote. Ken Sherwin had made my ruby ray gun and I was getting a lot of inquiries from high school kids who wanted to make a laser. I offered it to *Popular Science*, and just about the time I got the answer back, I got a letter from some woman in San Jose complaining I was giving dangerous toys to children. Of course, this was a toy housing I'd used, it wasn't at all a toy.

Then they [*Popular Science*] wrote me and said they had another article about making a laser, but they would buy my article for two hundred dollars and not publish it. I thought, "Well, maybe it's better not to publish that." They were offering, however, to send more detailed instructions on how to build a ruby laser. I gather that those instructions changed over the next few months so it became more and more what we had in our paper! [laughs]

Then I had the cute experiment about measuring the wavelength of light with a ruler, where you just have a laser beam skimming along the surface of the laser, being diffracted from the rulings at an almost glancing angle.

Riess: Where did you publish that?

Schawlow: That was in the *American Journal of Physics*, which is the journal of the American Association of Physics Teachers.

Riess: So that might be useful as a demonstration.

Schawlow: Oh yes. I think a lot of people have used that.

[looking through papers] Oh yes, we studied strontium titanate, which is a ferroelectric material, with chromium as an impurity. We tried to see if we could change the intensity of the fluorescence by putting on an electric field. We eventually got a small effect. (That was done with Stan Stokowski.)

The idea was that you deform the crystal enough--see, the chromium ion, if it were at a perfectly cubic surrounding, it wouldn't be able to have any electric dipole emission at all. But because it's not at a center of symmetry, it has an electric field which deforms the ion and makes it possible for it to emit. So the idea was to apply an external electric field to deform this rather deformable material, strontium titanate, and see if that would change the intensity. We did succeed in that.

Let's finish this [review of the students and postdocs] off. You know, we had all the students I could handle. At one point, I had ten students and I told them I'd never given a Ph.D. Well, after that they started coming out the pipeline. But in 1968 I think, I had some contact with Dick Slusher who was getting his Ph.D. at Berkeley with Erwin Hahn. He had a National Science Foundation postdoctoral fellowship. He wanted to come, and it wouldn't have cost me anything, but I talked him out of it. I was feeling rather despondent at that time. We didn't have any room, we didn't have any money to spare.

Riess: Here?

Schawlow: Yes.

Riess: You had your ten rooms?

Schawlow: Yes, but they were all full of students. Especially, we didn't have any money to start anything new. I suggested that if he wanted he could come, but maybe it wasn't too good an idea. So he went to Bell Labs instead and did very well.

He got the Schawlow Prize from the American Physical Society Laser Science Group a year ago, and I was there to tell the story of how I foolishly missed a chance to have him work with me. But I was just, well, feeling kind of depressed and not having any thrilling ideas, and really not having much freedom to do new things.

Riess: That's the part I don't understand, not having the freedom.

Schawlow: I didn't have the money, really, to start something that would require a lot of new equipment. I had good spectrographs of several different kinds, but--.

Riess: But this is the drying up of money time or what?

Schawlow: Yes. It was around that time that NASA decided they couldn't continue to support this work. They were under pressure to do things that were more closely related to their missions. So they said they were not going to be able to support me any longer. The Army Research Office had been giving me small amounts, $30,000 a year. It was a little later that they dropped out.

At any rate, I felt, "Well, I could go on doing the same sort of thing, but I couldn't really start anything very different." So I didn't encourage him to come although I would have taken him if he decided he really wanted to.

Fortunate Conjunction

Traveling

Schawlow: Then there's something, and I don't know whether I ought to say it or not, but I was on the Physics Advisory Panel of the National Science Foundation. They started a new program, offering equipment grants. And the next year's meeting, Wayne Gruner, head of the physics section, said, "Well, we haven't been getting many applications for those grants." I said, "Well you've got mine." He said, "Oh really? Do we?" And not very long after that I got the grant. That was a very fortuitous timing, because it came just about the time that Ted Hänsch came here.

Now, again, I was not really too interested in taking him on, but I got this letter from Peter Toschek, whom I had met. He wanted to know if I could take this man as a postdoc. I wrote back and said that I didn't have any money and he said, "Well, would you take him if he'd get a NATO fellowship?" I said, "Oh, all right." He did get that, and when he arrived, it was a very small fellowship. When we saw how good he was, we found another hundred dollars a month or so to help him.

Riess: You had met him before, hadn't you?

Schawlow: Well, he told me that, but I didn't remember that I had met him at a conference in Edinburgh. But I'm very bad about that sort of thing.

Riess: As a side note, it seems to me one of the pleasures of being a physicist was the far-flung conferences. Because of your responsibilities to Artie, did you miss out on that?

Schawlow: Well, not really. We felt we could go away for a week or two. By that time, Artie was living away from home. We went away for sabbatical in 1970, and that was bad, because while we were away the people in the house he was living in decided that they couldn't handle him any more. There were young girls, high school age or so, there who were afraid of him. He was big and strong, and he was having tantrums, though he wasn't hitting anybody. That was bad. If we'd been here, maybe we could have soothed them. But we had to find another place for him.

Riess: Where did you go on that sabbatical and what did you do?

Schawlow: We went to London and I actually was anti-commuting to Redding. My good friend George Series was there. I didn't really work on anything much; I think I did a little study of possibilities for x-ray lasers, but I didn't really reach any very valuable conclusions.

I think I foolishly--when you go to a country like that, if people know you're there they invite you to give a lot of talks. Doesn't seem like an awful lot for each one, but in the end it was too much.

Riess: Too much to get any physics done.

Schawlow: Yes. That's right.

Riess: That must have been when you met Ted Hänsch.

Schawlow: It may have been a year or two earlier, I'm not sure.

I remember very well--we flew to London and rented a car, which was a tiny Fiat, and drove up to Edinburgh. That was a nice adventure. With that car you really sort of had to stop every hour because it wasn't very comfortable. I think it was a deal that Pan Am had--the car rental was included in the excursion fare.

Riess: Did you have your daughters with you?

Schawlow: Not on that occasion, that was just a week or two. When we went to the sabbatical, yes indeed. They went to the American School in London. One of them, Edie, the younger one, was involved in two amusing stories there. She told someone, I don't know if it was another student or a teacher, that her father had invented the laser. The teacher said, "Oh, I didn't think so. Got to ask the science teacher."

The science teacher said, "Oh no. The laser was invented by a Mr. Laser, I think it was Samuel Laser." She was sort of crushed, but I managed to find a magazine or book that had the facts. [laughter]

She was eleven at the time. They had a long weekend for the American Thanksgiving, so we all went to Paris. She complained later, everybody else had a holiday but she had to go to Paris. In later years she thought that was funny.

[tape interruption]

Ted Hänsch and Edible and Tunable Lasers

Riess: Ted Hänsch came to Stanford in May, 1970?

Schawlow: Yes, he came before I went on my sabbatical and he did some wonderful things, including when I was away. He was very generous about putting my name on things when he'd really done most of it. He was just a genuinely nice person, good sense of humor, as well as being a wonderful physicist. He had good hands and could build things himself very quickly--which I could never do. Also, he was good at theory.

Riess: Did he arrive with something that he was working on?

Schawlow: He had worked on gas lasers for his Ph.D. thesis, but he was willing to work on anything that looked interesting here. I can't remember the exact sequence, but I think we got that equipment grant just before he arrived, and he helped us decide what it was we were going to buy and I decided to buy two lasers--you could then get them commercially. One was a nitrogen laser which gave short pulses, but they were a hundred kilowatts, a hundred thousand watts. They could pump all sorts of dyes--dye lasers had been discovered, but they hadn't been used for much.

One of the first things Ted did was to find a way to make this dye laser fairly monochromatic. One of the advantages of dye lasers was that they were tunable, but their output tended to cover a rather broad wavelength band. Whereas for spectroscopy you want them to be fairly monochromatic so you could tune them across spectral lines and see fine details. He was able to make a pulsed laser that was fairly monochromatic, and it was pumped by this nitrogen laser.

This nitrogen laser also was used--we had some fun playing with various dyes because almost any dye that glowed would lase, and even the gelatin filters, the photographic filters, would lase. So then I had one of the most fun experiments I did, about the last time I did anything with my own hands. I decided, well, if you can put dyes in gelatin, maybe ordinary Jello would lase. And it didn't. I tried all twelve flavors of Jello and they didn't fluoresce very well. I guess people don't like fluorescent foods. [laughing] Many of the dyes that fluoresce are poisonous anyway.

But I realized there was a dye that wasn't poisonous, namely fluorescein, because dentists paint that on your teeth to show up the plaque. So I put some fluorescein in some Knox gelatin and managed to get laser action in that. So we

published this and put in a phrase that this is the world's first edible laser material. [chuckles] That's often been quoted.

Riess: That's more comic strip material.

Schawlow: It is, but it turned out to be something very useful that we didn't realize. Even before we published--we weren't secretive, but somehow somebody at Bell Labs heard about this, maybe had a preprint, and they realized that photographic plates used gelatin and it could diffuse dyes into the photographic plates, so they could put patterns on there, like diffraction gratings, and could tune the laser with the pattern of lines on the photographic plate. Lines one behind the other would act as a grating, depending on the spacing of them. So they published that.

And then people with semiconductors began to put gratings of that kind in their semiconductor lasers to help tune them. Something quite serious came out of this fun experiment with the edible laser. You never know what will come from research.

Hänsch had this tunable pulsed laser, and I said to him, "If you want to get the interest of physicists, then you should work on the hydrogen atom." That's about all I did on it, but he then made a discharge tube--you could flow water through it and have a discharge which would produce hydrogen atoms, and observe the fine details of the hydrogen spectrum. That was a little later. I guess that was '71, probably after I came back from my sabbatical.

Riess: This work shed new light in some basic areas? Is that what you mean about getting the interest of physicists?

Schawlow: Well, yes, the theory of hydrogen atom--the details are based on the theory of quantum electrodynamics which includes detailed interaction of the atom with the electromagnetic field. That theory is a very good one. Now Hänsch has gone on, and others have too, to make really precise measurements on the hydrogen atom, and so far quantum electrodynamics is still okay. They haven't found anything wrong with it.

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Schawlow: From these first experiments he was able to measure the wavelength of the hydrogen atom line by a factor of ten or so, more precisely than could have been done before. The hydrogen atom line is very broad ordinarily, broadened by the Doppler motion. Hydrogen is a very light atom, so the atoms are moving rather fast, and that made the spectral lines kind of broad.

And they knew from radio frequency experiments what hidden structure lies within this broad line, they don't know the relative intensity components. To find the center of the line of the components was hard, but he was able to resolve it very completely and could make a more accurate measurements of the absolute wavelength. He has gone on far beyond that in his work in Germany.[1]

I think one of the reasons he left us was that--he wanted to continue to refine these measurements on hydrogen, and it was just so expensive that we simply could not get enough money for it from American sources. I gather we had one of the largest grants in the atomic field, but grants in atomic physics are generally not anywhere near as big as in nuclear physics. We really couldn't afford to do the things they've been able to do in Germany.

Riess: How long did he stay here?

Schawlow: He stayed with us about fifteen years. We made him an associate professor after two years of post-doc--he wasn't willing to take assistant professor--and then about a year or so later we had to give him tenure because other places like Harvard and Yale were trying to get him. He became a full professor quite young, because he was so in demand. We kept fighting off German offers, but eventually we couldn't. There were several things: Germany is home although his English is wonderful, he had a talent for languages as well as everything and it was only every couple of years that I might find a slight error in idioms or something like that. But they speak German in Germany, and they also have very good facilities.

Doppler-free Spectroscopy

Riess: And for your work it made a long-term difference to have Ted Hänsch there?

Schawlow: Oh yes. Yes. We switched directions entirely. We pretty much stopped working on solids and could do studies on gases.

Ted had found a way to get rid of the Doppler broadening by using two beams going in opposite directions from the same laser, separated by a beam splitter. The only atoms that would

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[1]Ted Hänsch is now a professor at the University of Munich and director of the Max Planck Institute for Quantum Optics.

interact with both of those would be atoms that were standing still, because otherwise they'd be Doppler-shifted differently for the two beams.

He applied that first to iodine vapor because he could use a krypton laser that we had bought. Iodine has lots of lines at all wavelengths so it was easy to get detailed spectra. Marc Levenson was a student who worked with him on that and he was maybe the best physicist I had of all my students. He did a very good job on that and then several other things, too.

So he had this method of Doppler-free spectroscopy which he then applied to the hydrogen with a pulsed dye laser. The argon or krypton lasers wouldn't tune very far, just within the width of the line. But as I say, well, the old saying: "If you can't get the mountain to come to Mohammed, well, take Mohammed to the mountain." [laughs] If you can't tune the laser to the line, well you get something that has lines everywhere.

That began to get me a little interested in molecular spectroscopy. For years I'd been telling people that a diatomic molecule is defined as a molecule with one atom too many [laughter]--if you get the second atom then things get much more complicated. You have vibration and rotation. Then I started thinking of ways you could selectively label a particular state, by saturating it. We began to do that and we found several different ways of doing that, using lasers to label states of molecules.

Riess: Label?

Schawlow: Label them, yes.

What we do is use one laser tuned to just one line in the spectrum and you chop this laser off and on. When it's on, it would saturate this line; that is, it would pump atoms out of the lower state so that there are fewer there, and all the absorption lines coming from that particular lower level would be weakened. So if you scan through it, you'd see those lines being modulated. They'd be alternately weakened and restored. Or you could--later on, we used pulsed lasers and did a two-step excitation. The first laser would put atoms into an upper level and then a second laser would go on up from there. So again, you'd have labelled this one particular level.

We were able to simplify a lot of spectra. We worked mostly on the sodium two, Na_2 molecule, which was complicated enough. Sodium is easy to vaporize, and it came at a reasonable wavelength for lasers in the visible, the yellow to orange red section of the visible. So we found a lot of new

levels that hadn't been recognized before. And although I really wasn't too interested in molecular spectra as such, I was interested in this technique of simplifying spectra.

Riess: What is the appeal? The simplification in itself?

Schawlow: Yes, it is. I'd always thought molecular spectra were just too horribly complicated for anybody, although people somehow did analyze them. The thought of making them more tractable, although the procedure is tedious, still, it was powerful and that was an appeal for me. So I had several students working on various aspects of that.

Brillouin Scattering: Marc Levenson

Schawlow: We did a little bit of work on the Brillouin scattering. When I had that equipment grant, I bought a krypton laser. It was a fairly expensive thing. The krypton laser appealed to me, if I was only going to buy one. It had a wide range of wavelengths. They could tune it to just a few lines here and there, but they pretty much covered the whole visible spectrum.

So we got this thing and people started asking, "Well, what are you going to do with it?" I thought, "Well, maybe I can look at the light scattering in bromine," which is a pretty opaque liquid in the visible. But this laser had one line out at 7900 angstroms which is really in the near infrared.

I asked Marc Levenson to do that and he did a great job. He not only stabilized the laser by putting a Fabry-Perot etalon in the thing, I think temperature controlled, so he made the laser quite narrow band, but then he built a scanning Fabry-Perot to scan the spectrum. He did get the Brillouin scattering. He could measure the velocity of sound at ultrasonic frequencies in the liquid.

But he noticed the shape of the curves from the interferometer were not quite right. There was a broad background which should have dropped nearly to zero. It had a background in between the peaks. He suspected that there was something else going on, so he looked at the spectrum with one of our spectrometers and found that there was indeed a broad background going out several hundred angstroms. He realized that this was due to hindered rotation of the molecules--they were interfering with each other in the liquid--and he

published that about the same time as someone else discovered that too. But it was something quite unexpected.

Levenson really was very good. He'd really think for himself. He did most of his thesis extending the work on iodine that Hänsch and he had started, on the iodine vapor. He measured how the splittings in hyperfine structure depended on the what particular vibrational state you were looking at, and found that the states near dissociation had a different magnetic splitting. A lot of sophisticated sort of stuff, but interesting and really quite exploratory.

Riess: Did any Nobel Prizes come out of this work?

Schawlow: No, I don't think so, although my Nobel Prize was given for contributions to the development of laser spectroscopy, so maybe it was some of this stuff, or may be the hydrogen work with Hänsch. They of course knew that I had played a part in the transition from maser to laser.

Riess: Did Hänsch come up for a Nobel Prize?

Schawlow: He doesn't have one yet. One of the problems, of course, was that a lot of the stuff that he did was done with me, and there were a lot of other people working on laser spectroscopy too.

R.R. Donnelley Co. Project in Switzerland

Schawlow: In 1974, I was asked by people from the R.R. Donnelley Company to consult on a project they were starting with a Swiss laser company. The aim was to see if they could develop a system using a large, rapidly pulsed laser to drill the holes in the copper plating of cylinders for gravure printing. The work was carried out at the plant of LASAG in Thun, with the collaboration of the laser group at the University of Berne. They had previously developed an automated machine for laser drilling of the holes in ruby watch bearings.

For this project, I visited the beautiful little town of Thun several times a year, along with several Donnelley representatives. Although rather far removed from my previous experience, the problems were fascinating and I learned a lot about laser machining.

At first some promising results were achieved, but eventually the task proved too difficult for the available lasers. Also, the Swiss franc rose sharply against the

American dollar, making the work too expensive to continue. Although the agreement called for transfer of any technology to the Donnelley Company, it became apparent that there was really nobody who could make use of it. The chairman, Charles W. Lake, realized this and decided that their basic technology needed to be strengthened. To do that, he formed a technical advisory committee to meet several times a year. I was asked to serve on it, along with some very good people including Tom Everhart who later became president of the California Institute of Technology.

At the meetings, some of their people would talk about particular projects, and we would ask questions, some of which must have seemed dumb to the experts. Mr. Lake, a truly brilliant engineer and manager, rarely asked the committee for advice but rather listened and then made his decisions. I think the meetings of the committee helped to clarify the thinking of those who made the presentations. Also, it helped the company to recruit some excellent young engineers. By the time that the committee was disbanded by later management around the end of the 1980s, they had a considerably broader technical staff.

Cooling With Laser Light and Other Good Ideas

Schawlow: In late 1974, we had the idea that you could cool atoms by using laser light, cool them down to very, very low temperatures and therefore narrow the spectral lines. We wrote a short paper that was published in *Optics Communication* in 1975. We didn't do anything experimentally because we were interested in hydrogen particularly--that has the widest lines because it's so light. There wasn't, and still isn't, really a suitable laser for cooling hydrogen. So we just published this note, and I didn't even think to mention it in my Nobel lecture, but it has become a rather important field of physics since then.

Steven Chu, who's now my colleague at Stanford, did the first experiments. Well, Letokhov in Russia, and I think John Hall at the Joint Institute for Laboratory Astrophysics at Boulder, did experiments on one-dimensional cooling of beams. But Chu did what we'd been talking about, three-dimensional cooling. He added some clever things to that that I hadn't thought of. One was--apparently he didn't know about our paper until after he had finished his work. He had the idea independently.

Riess: He was at Bell Labs then.

Schawlow: Yes, he was and he's a very bright guy too. So he had the idea--. We had calculated how long it would take to cool an atom, say, of sodium, because they can only absorb one photon every 10^{-8} seconds, which is a short time. Each time they would scatter a photon, they would only lose about one centimeter per second of velocity. And they start out with about three hundred thousand centimeters per second, the average thermal velocity. So it would take a while and they're moving fairly fast, so I thought you'd have to build an apparatus about a meter in every dimension to cool these things down.

But he instead used an argon laser to vaporize a little pulse of sodium vapor from a solid surface, and then just let the faster atoms escape, and the slower ones that remained he could then cool down to the very low temperatures. He started this field of optical cooling, and also of trapping atoms, which has become a big thing. This is one thing where we each came to the same conclusion about the same time, so I try to make the point that Hänsch really did come up with the idea of laser cooling independently.

(Steve Chu did win a Nobel Prize in physics this year [1997], sharing it with two other very good physicists who had made important advances in laser cooling. As soon as I could, I congratulated him, even though I had to tell him that he had spoiled my perfect record of never succeeding in nominating anyone for that prize. Of course many others probably nominated him, too.)

Riess: When Hänsch came, did you expand?

Schawlow: No, I didn't. But I gradually gave up space and funding to him, I really let him take over things more and more. I tended to do [my work] with equipment that he wasn't using anymore. I really gave him priority over everything. I'd kind of make do with things that I could scrounge. I didn't spend very much on myself.

Riess: Why did you behave that way?

Schawlow: Well, he just was so good and I didn't really want to get in his way.

I did some other things that were quite different. We did this work on the molecules which wasn't thrilling, but it was interesting. Later, the last few years before I retired, I thought, "Well, I'll do something--I've done enough that if it doesn't pan out, then it doesn't really matter to me so much."

I got some students to work on looking for very weak absorption lines in rare earth metals. Those things are almost opaque, but still, the rare earth ions act like they're almost independent from a number of studies, from neutron scattering and so on.

I had some very good students, Mike Jones and Dave Shortt, and they built a spectrograph that was very, very sensitive and could detect very small absorptions. They never did find any in a pure metal, but they found some metallic compounds--that behave metallically. We found lines even in one that was a superconductor. Neodymium cerium copper oxide. We were able to look at it both above and below the superconducting transition. It has a transition at thirty degrees Kelvin or so.

The way it was being done, Jones and Shortt just used a bright lamp to produce the absorption spectra and that produced a lot of heating, for example in helium which is then boiling vigorously. That's why you couldn't be really sure of the exact temperature of the sample. We couldn't really do what I would've liked to do, which was to go carefully through the transition temperatures--which you could have done if we'd gotten the lasers tuned to that, once we knew where the lines where. We couldn't use the laser to search for the lines because it would take forever to search for lines, like looking for a needle in a haystack. So we had to use a conventional spectrometer.

Tower of Babel

Riess: In the introduction to this book it says that tunable lasers were taken up by scientists who were both laser physicists and spectroscopists. Spectroscopy was a separate branch of physics? I don't understand at what point one elects to be A or B.

Schawlow: They probably drift into it. Laser physicists would be working on lasers primarily, and a spectroscopist might use spectrographs as they all had done before. And there always have been some. Spectroscopy was the hot field in the 1920s, and then it was considered a backwater in the thirties, the forties. However when they had lasers, that gave them a powerful new tool and they could do a lot more in spectroscopy.

Riess: When we talk about astrophysics or physical chemistry or laser physics or theoretical physics, these are discrete specialties but they all have to be taught in a university?

Schawlow: Some places have specialized courses in them. We didn't really. We just sort of thought if you signed up to work with a professor doing things in that field, then you have to read up on it, teach yourself, learn some of the techniques from his laboratory, and go on from there. But you do have a Tower of Babel effect that it is getting harder and harder to understand people in different branches of physics.

Riess: All with their own journals.

Schawlow: Yes, *Physical Review* used to be one journal, but now it has five sections I think. One of these is nuclear physics, another one is particle physics. I think there's even one on theoretical physics. Section A is atomic and general physics--I don't know, I used to get the whole thing, but they'd stretch from a volume of about this big for a year to *this* big. And very expensive, too, and you just couldn't store it.

Riess: Doesn't it mean that people get more out of touch with each other?

Schawlow: Yes. The only thing that brings them together is the things like *Science* and *Physical Review Letters* which has short articles from the various branches of physics. But even there, I find I can't really understand much of the things that are out of my field.

Riess: Do you use your computer as a way of keeping up with physics? In other words, do you get on to the web?

Schawlow: No, not really. The library has *Physics Abstracts* for the last few years and it's sometimes useful to search there, especially if you know the name of a person. The particle physicists, which is a very narrow field because they only have a few big accelerators, and are all working on similar problems, they're really desperately anxious to get the last word on something. both the theorists and experimentalists, and they post preprints on the web and people eagerly examine them, but I have never wanted preprints. When I see the article I want to do it once and not have to see an abstract and then later wait to get the full article.

Riess: Why are they so desperate?

Schawlow: It's a matter of getting something, an idea, that they can elaborate and publish something before somebody else gets the idea.

Riess: More so than in other fields of physics.

Schawlow: Yes. Very competitive. I think it's because the accelerator is so expensive, they can only have a few of them, so there are only a few problems being addressed at any one time, and a lot of theorists are chasing the same problems.

Riess: The science writers who are following physicists around, is it particle physics that they tend to follow?

Schawlow: Astrophysics seems to turn them on most, then particle physics. Not very often the optical physics.

More on Laser Cooling

Schawlow: One thing that has caught their attention in the last couple of years is that Carl Wieman, who is one of Ted Hänsch's students --now at the University of Colorado--has carried this laser cooling to the point that he, with Eric Cornell, were able to cool atoms down to the low temperature and of sufficient density that they got what they call Bose-Einstein condensation. That started with laser cooling--and I'm really not going through all the advances that other people made in extending laser cooling.

I guess I didn't explain how laser cooling works. It's very simple. The way we visualized it was that if an atom is moving and you have laser beams coming from all directions, from the six principal directions, that if the atom is moving toward the laser beam--the laser beams are tuned slightly below the resonance--if it's moving toward the laser beam, the atom sees the beam has shifted up into resonance, Doppler-shifted. So it'll scatter light, and every time it scatters a photon, it loses about a centimeter per second. On the other hand, when it's running away from the beam that's coming behind it, it doesn't see it because that's shifted farther down out of resonance. So this is a way of cooling free atoms without ever touching or making them condense.

But other people found that by using the internal modes of the atom, they can get cooling that goes much beyond what we had predicted. Then they can trap them as pioneered by Steve Chu. He used a magneto-optical trap. Then they use evaporative cooling, where they just lower the trap slightly and the faster atoms escape, leaving it cooled. That way, they get down to extremely low temperatures, micro-Kelvins, where Kelvins is one degree absolute. And that's where they were able to get this Bose-Einstein condensation. Very much more has been added to it than what we did, but we did start it. I

think that's my second most important paper, although I didn't think of it.

Riess: When was that?

Schawlow: It would be 1975. I worked on writing the paper when I was on sabbatical in London, in '74.

Riess: "Cooling of Gases by Laser Radiation"?

Schawlow: Yes. *Optics Communication*.

Riess: You have Ted Hänsch as the first author.

Schawlow: Yes. Courtesy. Well, actually, we could have done it either way.

There was one case where we were discussing it a little bit--you often go through a state of confusion before clarity emerges when you take on a new problem. It seems almost necessary. So we were sort of thinking, "Well, we could scatter light. Let's see, would you want the laser to be tuned above that? Or below?" We were a little confused. Overnight we both came to the same conclusion, to tune it below the line.

When we told people about it, we got two different reactions. One was, "Can't possibly work" because you're putting in energy and you're heating the thing. That wasn't a good reason because the laser has very little entropy, it's a very pure kind of light, it doesn't have a lot of randomness to it.

Other people said, "Oh yes, it's obvious." [laughs] When some people said it's wrong and others said it's obvious--we knew we had something pretty good.

Riess: I should think people would be very reluctant to say something can't work.

Schawlow: Oh, you'd be surprised. I remember people saying lasers weren't going to work, and they gave good reasons which are best forgotten.

Charlie Townes, of course, tells about how Rabi and Kusch tried to argue him out of the maser.

Riess: Did you and Ted Hänsch do any work on that in the lab?

Schawlow: No, no. We didn't even try to build or do laser cooling--just wrote this theoretical paper and left it at that, because we

really wanted to cool hydrogen and we couldn't do that because we didn't have a suitable laser for cooling it. So we just threw it out and let people see it. Run it up the flagpole and see who salutes, as they used to say on Madison Avenue.

VI ACCOMPLISHMENTS AND QUESTIONS

[Interview 8: November 26, 1996] ##

General Look at How Schawlow Works

Riess: When you were working on a problem, let's say when you were at Stanford, who did you bounce your ideas off? I mean, is that a process for you?

Schawlow: I had various students and postdocs and I guess I talked with all of them. We discussed things informally.

Riess: Would you use Charlie [Charles Townes], wherever he was?

Schawlow: No, I wouldn't use Charlie at all. No, he was doing different things, he was into astronomy then. And I really wanted to do my own thing, however insignificant that might be.

Riess: And maybe the case is that one doesn't need to.

Schawlow: Well, I was forty by the time I came here, I wasn't a kid anymore, I really was old enough that I should be standing up on my own feet.

Riess: I'm not implying that. I'm wondering about the intellectual process, whether it's an internal thing--"This could work," "But no, that won't work." Does it all go on in the head?

Schawlow: Yes, pretty much. But I did talk with students and I gave them a lot of freedom. I would sort of say, "This kind of looks interesting. Why don't you look into it?" And if they were good, they would find something that everybody hadn't thought about. But I would have pretty good instincts of things they could try.

Some of them were also fiercely independent, like John Emmett particularly. But mostly they would go in the direction I had pointed them. I was just interested in exploring a lot of different things, so different students I would discuss different things with. We would have our group meetings every week. They would be pretty informal and I'd try and get people talking.

Riess: When you went to international meetings, was that a very fertile time?

Schawlow: No, not really. It was sort of a waste of time. I guess I don't absorb things very well. I enjoyed going to them, but I don't really remember ever learning anything very clever.

[laughs] I remember the first international meeting I went to back in 1955 when I was working on superconductivity. The thing that intrigued me most was to find out about something called Dexion, which is a kind of oversized Meccano erector set. Well, people at Bell Labs were already using that, but I hadn't known it. Everybody at these meetings wants to tell you what he's doing.

We did have visitors who came by [Stanford], quite a few of them. It was a place that was sort of on the path when anybody came to the United States. It didn't seem to matter what part of the United States they were supposed to be visiting, they would somehow stop by Stanford. So we saw a lot of people, but I really don't think that they influenced me very much. I may have picked up little bits and pieces.

I don't think these ideas we had were very wonderful anyway, but they were all something new and that was my main purpose, to do things that were new and not worry too much about how important they were.

Riess: I need to be reminded that because you're a physicist does not mean you have a passion for every single aspect of physics.

Schawlow: Oh, physics is much too big. I mean, really the old Tower of Babel effect is certainly working there.

When I started out when I was a graduate student, I was interested in nuclear physics. I read pretty much what was available, and understood it pretty much, but, boy, that's gotten far beyond me. And particle physics I'd never gotten into. Even now in laser physics there are so many branches and so much elaborate theory that I've never been able to get into. It's discouraging.

Riess: Do you think that people have unrealistic expectations of physicists as problem solvers?

Schawlow: Well, we certainly have lots of problems to solve.

I guess when I look back I sort of regret that I didn't find *the* big problems in science, and do something about them. I just did what I could, whatever lay at hand. As long as it was something that hadn't been done before I was willing to explore it--even though I don't think anything I did really was of basic, fundamental importance like discovering quantum mechanics, relativity, or something like that, it wasn't in that league.

Still, there were a lot of interesting things we turned up, and some of them provided a lot of work for other people to do afterwards, to clean up.

Riess: If you say you regret that you didn't work on *the* big problems, do you have a hindsight about what those big problems were?

Schawlow: No. Really, I don't think I could have done anything but what I did, really. I didn't have the instinct, or the theoretical knowledge. Indeed, of course, by that time the big excitement in physics was going into particle physics. That was something that you had to devote your entire self to, become part of a big team working on a huge project.

When I came here I knew that SLAC was going to be built, and I hoped that somehow there'd be some way of getting involved with it. But it clearly wasn't possible, so I didn't really try. Anything they did was done to a deadline. You would get time for a run on one of the big machines, and you had to get everything ready for that. And of course there was the earlier stage where you had to go and persuade them that your project was worthy of time on the big machines.

It was a very competitive business and I really wasn't prepared for that. I didn't know the background or anything like that. It was really too formal for me.

Riess: Earlier you mentioned that you organized public seminars at Stanford which allowed people to come in from industry and other campus departments. I'd be interested in hearing all about that idea.

Schawlow: Well, it was when I first came in 1961. For a year or two I ran these seminars and then I guess other people took over the idea. It was a time, you know, when nobody knew anything much about lasers and there was a lot of excitement. So we had

people--I remember once we got Ted Maiman to come. Of course he had built the first ruby laser. He gave a good talk.

And there were people in the engineering department who were interested. There was Tony Siegman and his student Steve Harris. Tony was a professor already and he had been working on microwave masers, and then started working on lasers, and he had some students. I guess Harris came along later, and Bob Byer, who was Harris' student, was later still. They are both on the faculty, have been for years and years now. We're talking a long time ago. When was this? Thirty-five years ago.

I don't remember exactly how long I kept it up, but I think it gradually became a more departmental sort of thing, and some of the individual groups were strong enough to have their own seminars. There is still such a thing going on under the applied physics department. Once a week they have a seminar which is advertised both inside and outside the university, and I guess some people come to it from other places. That's aimed a little more toward laser engineering than I'm able to contribute to.

Riess: When you say outside the university, it's not that it's geared down to the public, but it's geared to industry.

Schawlow: People in industry. There were companies starting up. Like Spectra-Physics started up to make lasers and was quite successful at it. Varian had some interest, and Lockheed too. I don't remember just what companies were involved. A lot of small companies--Watkins-Johnson did a little work on lasers and optics technology--a number of other companies, some of which have disappeared. Anybody who was interested could come.

Riess: It sounds like an important thing to get going.

Schawlow: Burt McMurtry, I remember, was one of Siegman's students. He did a clever experiment. He wanted to detect microwave modulation on lasers, and he wanted a fast-responding photo tube. He realized that he could take a travelling wave tube, which was intended to amplify microwaves, and if he just shined the laser on the cathode of that tube it would amplify whatever pulses were on the laser. So he didn't have to build a tube. He took a travelling wave tube and shone a laser on the cathode.

He's done very well. He went and worked for a while at Sylvania, but then he got into venture capital and has done very well at that.

Riess: I think of putting together that seminar as a way of thinking larger, and that's my question here. How do you broaden your view?

Schawlow: I always read a lot of journals. I would subscribe to a number of journals--I didn't really have time to go to the library so I would get a lot of journals. For a while I'd keep them, but after a while I couldn't keep them. But I would skim through them every day as more would come in, and catalogs too, looking for ideas of equipment.

I went to the meetings. The Optical Society would have one. And then eventually the Quantum Electronics Conferences would have exhibits. You'd see some new apparatus and get some ideas of things that you might use. And I'm sure I did pick up some ideas there.

Prize-winning Work--Rydberg Constant

Riess: Three of your accomplishments are listed in the book on the Nobel Prize winners in physics: the observation of the complete hyperfine structure of a molecular iodine line; the first optical measurement of the Lamb shift in atomic hydrogen; and the most precise measurement of the Rydberg constant in hydrogen.[1]

Schawlow: I have to admit that Hänsch really did most of those things. I encouraged him and provided equipment for him, but the iodine thing was really done while I was away. I had, however, bought a krypton laser thinking that it would be useful for something or other. So it was there. I had Marc Levenson working with it to make it very monochromatic for some Brillouin scattering studies.

Hänsch did have the idea of getting rid of the Doppler broadening from the thermal motion by sending two beams in opposite directions through the cell containing the gas. Then he would chop one beam and then look at the other beam to see if it was modulated. If the beams were tuned either below or above the center of the absorption line, they wouldn't interact because they'd be seeing atoms going in different directions because of the Doppler shift.

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[1]*Nobel Prize Winners, Physics*, edited by Frank N. Magill, Salem Prize, Pasadena, 1989.

However, when they're tuned just to those atoms that were not moving at all, or perhaps moving a little sideways, they could interact with the same atoms, and the one beam that was chopped would saturate those atoms and decrease their absorption and so let more of the probe beam through, so it would modulate the probe beam. This was a very clever idea that Hänsch had.

Also a similar idea, about the same time, from Christian Bordé--it actually has roots in the things that had already been done in spectroscopy of laser gases, where they'd noticed the dip when they were tuned to the center of a line. Because there they have two beams going--this is for the gas inside the laser--they do have the two beams going in opposite directions. But what Hänsch introduced was using two beams externally and chopping one of them so that you could sense or detect the other.

Well, he had this thing, and he also had found a way to tune the pulsed lasers so that they were fairly monochromatic, a fairly narrow band. You could tune those anywhere in the visible. So I said, "Look, if you want people in physics to pay any attention to you, you should look at hydrogen." That's the one that people really think they understand, it's the simplest atom.

So he went to work and he did it, built a gas discharge chamber for producing atomic hydrogen and passed the two beams through that, and was able to resolve the fine structure in the hydrogen spectrum.

Well, at first he did that, we thought, "Maybe that'll permit us to measure the splitting." But it turned out that they were already well-measured from microwave studies, so what was left was to measure the absolute frequency of the line. Certainly after--your question before of "Who did I talk with?"--well, certainly I talked a lot with Hänsch after he came and discussed ideas with him.

So the thing you could do was measure the absolute wavelength. Now, even if you'd known where all these lines were under this Doppler-broadened spectrum, you couldn't really tell exactly where the center of the lines were because you didn't know the relative intensities of the components. So once they were resolved he could start measuring the absolute wavelength and therefore get a value for the Rydberg constant, which is one of the fundamental constants of physics. It measures the binding between electrons and nuclei in atoms. He did improve the accuracy of that by about a factor of ten or so.

Since then, he's gone on, and others have too, and they have improved the accuracy by maybe a factor of a million or so. That's a complicated business.

Quantum Electrodynamics

Riess: What is that kind of accuracy good for?

Schawlow: Only for basic physics, I think. Well, a hydrogen atom is something they think they can understand quite completely through quantum electric thermodynamics. Indeed they can calculate the energy levels with great precision in the splitting, in the Lamb shift and so. So one needs to verify that to see whether that really is exact. It's a test of quantum mechanics. So far it's passed every test.

The calculations have become extremely complex. They have to use more [Richard] Feynman diagrams than the ancient astronomers used epicycles. But there's a systematic procedure for doing these Feynman diagrams. Although it requires big computers and a lot of patience, still some theorists do go on calculating them, and so far they agree very well. In the latest measurements they can see an effect due to the size of the nucleus, which could be ignored in the earlier work because the nucleus is much smaller than the electron's orbit.

So far they haven't found anything wrong with quantum electrodynamics, which in a way is a little disappointing because you'd hope to discover something new and exciting. But it's essential to test these theories as well as you can, and they can test them much, much better than was ever believed possible in earlier years.

Riess: The search for something wrong opens another avenue.

Schawlow: That's the way physics goes, really. A lot of the time you hope something will not work. You have Michelson's experiment on the ether drift and it turns out there wasn't any. Then Lamb and [J.R.] Retherford in 1947 or so detected a Lamb shift between two levels in the hydrogen atom that were thought to have exactly the same energy, the 2S and 2P levels.

There'd been some hints of that before, even some measurements that had indicated it, but others had disagreed, so it was not clear until Lamb and Retherford used a radio frequency method that didn't have to worry about the Doppler broadening of the spectral lines. And of course that's what

Lamb got his Nobel Prize for. It was one of the things that inspired [Shinichiro] Tomonaga and Feynman and [Julian] Schwinger to develop quantum electrodynamics, for which they got their Nobel Prize.

Those quantum electrodynamic calculations have been refined very much. Interestingly enough, Paul Dirac, who developed the relativistic theory of quantum mechanics in 1928 or something like that, never liked quantum electrodynamics. I heard him talk about it at one of the Lindau meetings of the Nobel Prize winners, in 1982.

I happened to have a tape recorder with me at that meeting and I taped Dirac's talk and gave a copy to my friend George [W.] Series--I transcribed it and he got permission to publish it in the European *Journal of Physics*. Essentially Dirac said that quantum electrodynamics is not a real theory, it's just a prescription for calculating, but it's an awfully good prescription for calculating. [laughing]

One keeps hoping there will be some much simpler way of looking at what should be a simple thing with just one electron and one nucleus. But they have to take into account the interaction with the radiation field, polarization of the vacuum--it becomes extremely complicated to try to do exactly, but apparently they can, and so far neither that nor other precision experiments, like the ones that Dehmelt got his Nobel Prize for, have shown anything wrong with quantum electrodynamics.

They keep on pushing, and I'm sure that Hänsch and others will get another factor of ten or so and send the theorists back to their pencils and their computers.

Riess: Would you characterize this as the search for the secrets of the universe?

Schawlow: Yes, it is part of that, yes. It's part of the search for the laws that govern the universe. You test the ones you know and see if anything's wrong. If so, then you may have to get a totally different approach that looks quite different but somehow includes all the old results. An example of that, of course, is relativity reduces to Newtonian mechanics if the speed is not close to the speed of light. If it's much less than the speed of light, then Newtonian mechanics is very good, yet it looks quite different when you do relativity.

One hopes that maybe there'll be some new way of looking at things that'll make things simpler. But making them look simpler is not enough, they have to predict all the old

results, and now there are very many good results of quantum mechanics, and also some predict some new ones that differ from quantum mechanics. That's still an important search, but it takes a certain amount of courage to say that that's what you're going to do.

On the other hand, you can go ahead and measure some things which might possibly throw some light on it. But one has a feeling sometimes that it's sort of like the drunk who is looking for his lost quarter under the lamppost, "because that's where there's light" [laughter]--you didn't necessarily expect it there.

These things where we've made discoveries before--people, for instance, have tried the Michelson Morley experiment using lasers and increased the accuracy by many orders of magnitude, but the results are still the same. And so it is with quantum mechanics. Perhaps if a surprise is found it won't be found there, I mean, in doing the old experiments with better accuracy. But you don't know. So you do what you can.

Riess: Do you have some thoughts on the work of Stephen Hawking? Does he fit in anywhere here?

Schawlow: I've never had any interaction with him, I've never met him. He's a theorist, and he does interact with a number of other theorists. They have discussions and arguments, probably. But basically in the end I guess it's his own ideas that he writes up.

Riess: He has quite a public following, like Feynman had.

Schawlow: Yes, he's well known because he writes so well and because he's so handicapped. But there are others in cosmology, quite a few of them who--well, they publish obscure papers that are hard to read. They don't always agree with Hawking, and sometimes they're right, sometimes he is, or sometimes one doesn't know.

Scientific American published a debate between Hawking and Penrose a year or so ago about some aspects of cosmology. I wasn't really interested enough to try and decipher it very thoroughly. I think a lot of it is speculative.

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Schawlow: I know Hawking's work only secondhand through popular accounts, but I believe he did show that black holes could radiate away some energy because of quantum effects, quantum mechanical effects, which hadn't been thought about before. Otherwise,

black holes--anything that fell into them was going to stay there forever and had no way of getting out.

I think he has convinced people that there are quantum effects, that they do radiate something or other. Of course, there's a lot of radiation from the region around the black hole, a lot of material that's drawn into it and accelerates as it's going in. But it's a different frontier of physics.

And then of there are the particle physicists who feel that they have *the* frontier. That if only they can get some bigger machines they may find the Higgs boson which can explain why all the other particles have mass. Of course I don't know who explains why the Higgs boson has mass, but I don't understand that that well.

Riess: What you're doing is lining up a list of what we would call the sexy questions in physics.

Schawlow: Yes, yes. And I've never really worked on them, I sort of poke around the corners and see what I can find.

Riess: And yet the laser, at a certain point, was the sexy discovery.

Schawlow: Yes it was pretty sexy for a while, at least among the engineers. It also attracted a lot of theorists who wrote elaborate papers which I couldn't understand.

First of all, we thought of it in the semi-classical way, thinking of the light wave as being a classical wave to interact with quantum mechanical atoms and use the quantum mechanical process of stimulated emission. But this didn't satisfy people like Willis Lamb who wanted to quantize the field too. And you can do it, but it gets a lot more complicated.

I think it was in connection with that work that he proposed what's now known as the Lamb dip--not the sheep dip, the Lamb dip [laughter]--where if you tune gas lasers like helium-neon exactly onto the center of a line, then the output drops. That's because the two waves from the opposite directions are drawing on the same supply of atoms. This was certainly a predecessor of Hänsch's Doppler-free saturated absorption experiment.

Now, let's see, there was a third one that you mentioned.

Hyperfine Structure of Iodine

Riess: We talked about the Rydberg constant, the Lamb shift, and the first was the hyperfine structure of iodine.

Schawlow: Iodine, right, yes. Well, I went with some of these things. I had Marc Levenson measure the hyperfine structure of all the lines that he could reach. This was a case where he was using a gas laser that did produce a number of different wavelengths, maybe a half a dozen or so in the visible, but it wasn't continuously tunable. However, the lines were quite narrow when you could tune them.

So Levenson looked at those lines of iodine that he could reach and he studied the systematics of how did the hyperfine splittings change. There'd been some theorists who had suggested that the quadropole splitting, which is caused by the shape of the nucleus--not being spherical, they're sort of football-shaped--would change markedly as you got up toward the dissociation energy of the molecule, which he could approach. That didn't happen, so that was something he found.

Then there was a magnetic splitting also. That did get large as he got close to the dissociation, which he interpreted as a mixing in of another state that was near the dissociation level that had a different magnetic property. When they get close together they mix in a little bit of the properties of that other one. So we followed up on that.

His Ph.D. oral came just after Linus Pauling had come to Stanford. Pauling wanted to see what was going on in physics, so he volunteered to preside at a Ph.D. oral and Levenson was the first one, which actually was not so far from things that Pauling had done in molecular theory. It certainly was an extension of them. Pauling was quite polite and friendly, but I'm sure that must have made Levenson a little bit nervous because he was the great expert on molecular theory at that time, or had been.

Let's see, then I posed some alternative methods for really sensitive detection instead of using absorption. The trouble with iodine was that at the lowest pressure we could get by cooling it the lines were still pressure-broadened. That was not because we couldn't cool it more, but if we did there'd be not enough vapor to see, there wouldn't be enough absorption to see the changes in the absorption. So I thought of using the fluorescence because when it is excited it fluoresces. And we were able to go down a number of orders of magnitude.

About that time, I guess, continuous wave dye lasers were beginning to come in, and Bill Fairbank, Jr., who was the son of one of my colleagues, was working for me, and I suggested that he build a continuous wave dye laser. Well, the gain of the dye lasers, the continuous wave one, was not very high, and you couldn't put tuning elements in it the way you could in the pulsed dye lasers, where you had a lot of gain. So this thing was rather a Kluge.

Riess: Rather a Kluge?

Schawlow: Kluge--K-l-u-g-e. Haven't you ever heard of Kluge?

It was a complicated thing with external tuning elements outside of a laser cavity, and it was difficult to tune. But you could tune it to the sodium resonance, one of the bright yellow D-lines, and then use this fluorescence to get a relative measure of how much was there. He was able to cool it down to below zero Celsius, I think minus twenty or something like that, and measure the vapor pressure of the sodium at about a factor of a million lower than it had ever been measured before--as it went down in temperature.

So this was a very sensitive method--in fact, we realized that at the lowest temperatures there probably was only one atom at a time in the beam, that you'd accumulate light for some time. In fact, at those temperatures the mean-free path between collisions was greater than from here to the moon.

Riess: The mean free path?

Schawlow: Between collisions of sodium. There were so few sodium atoms that they just wouldn't ever collide. They'd collide with the walls of course, but not with each other.

Riess: Well, that's a very neat experiment.

Schawlow: Yes, I thought that was kind of cute. He built this thing, and it really wasn't good for much, so I sort of pulled the rabbit out of the hat by suggesting he measure the vapor density. And he did it. Of course I didn't do it.

Riess: Your responsibility in giving ideas to people just starting their careers--it can be a make or break thing, can't it?

Schawlow: Yes, I think so. And sometimes I would find students just couldn't do things the way I suggested and I'd have to give them something simpler, or get a new student to come in and help them.

Students work in different ways. Most of them are much stronger in formal theory than I was. I think I annoyed some of them because I'd do more hand waving because I was trying to understand the basic processes rather than the details of the theory.

The Apostolic Succession Phenomenon

Riess: In the process of putting a student together with an idea, do you have to have a grip on the student's psychology or his whole modus?

Schawlow: Well, you try. Sometimes you'd guess, and you wouldn't always succeed, as I say. Sometimes they couldn't work that way.

I had one student who just could not work by himself. He started out--as I often did, I'd have a beginning student work with an older one. I used to call it Apostolic succession. [chuckles] So John Holzrichter worked with John Emmett, and Holzrichter was a brilliant experimenter and has gone on to do nice things at Livermore. He was in charge of building their first big laser before fusion, and now he's in charge of their independent research--they have a certain amount of freedom to do things on their own.

When he was finishing up, I had Jeff Paisner start out to work with him. Holzrichter and Paisner did very nice things together, and I thought Paisner could just go on and do a bit more of the same. But nothing happened at all.

And then Serge Haroche came from Paris, a very brilliant guy, a wonderful person, and still a very good friend. Serge Haroche is now the head of the physics department at the Ecole Normal in Paris, which is one of the Grandes Ecoles, a very distinguished position. He had a bright idea of looking for what's now known as quantum beats, where you put a pulse of laser light on sodium vapor, tuned to the absorption line. But it would be a short pulse and the spectrum was broad enough so that it would excite several hyperfine components, sort of in phase. Then the thing afterwards would radiate--well, it was sort of like he lined up the atoms and then they precessed, like a searchlight that goes by you and you get alternations of lighter and darker.

Well, I got Paisner working with him, and things were going great, and I'm sure that Paisner made a real contribution.

Then Haroche left, and I said, "Well, you could do a little bit more here" and nothing happened.

National Ignition Facility Work, and Military Sponsorship

Schawlow: Finally Richard Wallenstein came from Germany and they did some nice work on quantum beats in molecules. He's a very good man, Jeff Paisner is, and he's done well at Livermore and published some nice work. He is now in charge of the design of the National Ignition Facility, which is going to be a super giant laser for fusion.

Riess: I remember you mentioning that, and I was thrown by the name.

Schawlow: It's a giant laser, or set of laser beams, that will be focused on a little pellet of heavy hydrogen. They will get enough energy so they hope that they get more out in the resultant explosion than they put in. The laser will heat it hot enough and compress it so that the heavy hydrogen combines to produce helium and release energy that way.

Riess: For a practical energy source?

Schawlow: They say that if you could tame it you could provide the world's needs for practically forever--there's enough heavy hydrogen in sea water.

But in fact now the sponsorship is military because they want to simulate hydrogen bomb explosions, and they can do that and really make measurements on them that they couldn't make on bombs, particularly because they're afraid that there might be a treaty banning all nuclear tests, which is I think quite possible. Then they only way they could do research on trying to understand and improve the hydrogen bomb would be with this simulation.

Riess: Tell me why understanding and improving the hydrogen bomb is an important way to go.

Schawlow: Look, I don't understand the military mind, at all.

However, it's certainly possible that if they could tame the thing--the trouble is that as the work has gone on the threshold has gotten higher and higher, so that they will have to put in something more than a million joules in one pulse. And the output will be something more than that, so it's a very

big explosion that they'll have to contain to convert it into usable energy.

They have some schemes, including having the thing in a cell whose walls are coated with liquid lithium that would absorb the neutrons from the blast and convert that into heat and then electrical power. But these things are still untried, and it isn't easy.

As I've said, it reminds me of the story of the king in the olden days who wanted to have some oak trees in front of the palace and told his prime minister, "Get a hundred men tomorrow and have them start planting a thousand oak trees in front of the palace." The prime minister says, "But Sire, why the hurry? Those oak trees won't be fully grown for a hundred years." The king said, "A hundred years? Have them start today." [laughter]

The possible payoff is enormous if you could tame nuclear fusion. Of course, this competes with the gaseous discharge work on nuclear fusion, the sort of thing that's been going on at Princeton. They both have difficulties.

Riess: If the military will pay, that's the way to get it paid for.

Schawlow: Well, it is, but I think the military really want that information. They want to know everything about hydrogen explosions, thermonuclear explosions.

Work and Publications with Students

Riess: I'd like to talk more about your students. You've already talked about many of them in the process. The first two graduate students to join the Schawlow group in 1961 were George Francis Imbusch and Linn Mollenauer, and you've talked about them.

Schawlow: We had a lot of fun together with Imbusch and Mollenauer. I think I've said before, Imbusch was very quick at getting things done.

Mollenauer was not quite so quick but he was a deep thinker and usually came up with something I hadn't thought about. He's done very well. He's been at Bell Labs for many years and he really was one of the first to show that optical solitons, solitary waves, could exist in glass fibers and that they would be a very good way to transmit information at high

speed because these things retain their shape, even if there's attenuation. And they can be replenished; if a signal gets weak, they can be reconstituted exactly the same as they were.

Riess: Perfect for Bell Labs.

Schawlow: Yes. Well, they haven't decided to put that system into work because this is a huge investment in these fibers, but still there and at other laboratories around the world it's being extensively investigated and looks like a real possibility for the very high speed communications.

Imbusch, despite his German-sounding name--I think his family came from Austria originally--his father was Irish and was a cabinet maker in Limerick. Imbusch went to University of Galway, where he could get a free education if he'd do it in Gaelic, in the Irish language. After finishing his Ph.D. he spent a couple years at Bell Labs, and they would have very much liked to keep him, but he went back to Ireland and has been a professor at Galway, and has continued to work on the spectra of ions and solids, and energy transfer among different ions. I think he's been a dean; he's certainly been an important official in the University, and in Irish physics in general.

Riess: Did Bell Labs ever underwrite work out here?

Schawlow: Well, they certainly never underwrote anything for me. I know when I was there, there was a feeling that, "Well, we're supporting science by providing new results from our own laboratories." They had given grants and fellowships, but I never had any direct contact with that. They never seemed very interested in what I was doing. As I say, I was going my own way, trying to stay out of the way of the thundering herd. I didn't want to get trampled on.

Riess: A student named Warren Moos "joined the fledgling laser spectroscopy group in 1961."

Schawlow: He was a postdoc who came from Michigan and he was interested in photochemistry and several other things. He actually had a student in engineering, Richard Soref, work for him on nonlinear optics.

Moos went to Johns Hopkins and became an assistant professor and has been a professor for many years. He switched to rocket astrophysics, where they send up rockets above the atmosphere and can photograph things in the ultraviolet and infrared, although only for a relatively brief period. I think he's done well at that, but I haven't followed him in detail.

Riess: Now, when we're talking about students, these are really graduate students. These are not postdocs.

Schawlow: No, I didn't have very many postdocs. Bill Yen was one, Moos was another.

Riess: Bill Yen came in 1962.

Schawlow: Yes. There were two students from Washington University that were somehow being pushed for postdoctoral jobs. One of them was Yen, the other one was Schwettman, Allan Schwettman, and he's still here. He worked for Fairbank on the superconducting accelerator, and he still continues to work on that even though Fairbank is long gone.

Riess: Did Bill Yen originally get his education in China?

Schawlow: I think not. His father was in the diplomatic service, and he didn't live in China very long. He said when he was about fourteen or so, he had to go back to China, to Shanghai. He grew up in Mexico City, mostly. His father was in the Nationalist diplomatic corps, and later was ambassador to Venezuela. I used to kid Yen about being the only person who spoke Chinese with a Mexican accent. He took some high school work in Shanghai. He said that was rough because he really had a lot of Chinese to learn and it's a difficult language.

But he came back to the United States and went to the University of Redlands I think, in California, and then to Washington University at St. Louis, where he worked on nuclear resonance.

Riess: And he was in the initial group with Imbusch and Mollenauer?

Schawlow: And Moos, yes.

Imbusch, I think, worked mostly on magnesium oxide, with chromium in it, which is another crystal that's a little different from the sapphire because the chromium ion is really in a site of cubic symmetry. Although chromium has a charge three, and the magnesium that replaces it has charge two, so there has to be some charge compensation somewhere else in the crystal.

Do you have the bibliography that I give you, the publication list? It might help me remember who did what.

[Riess passes bibliography to Schawlow]

Schawlow: Yes, here. We collaborated a little bit on energy levels in concentrated ruby with Paul Kisliuk and Mike Sturge at Bell Labs--Mike had come from England and had taken over my big spectrograph at Bell Labs. We studied temperature dependence of the width and position on the strong red lines in chromium and vanadium in magnesium oxide, again with some collaboration from Sturge at Bell Labs, and [D.E.] McCumber, who's a theorist. [Number 62 in publication list.]

Riess: This was in the early years?

Schawlow: Yes, 1964. Then Yen and [W.C.] Scott, who was a student, worked on praseodymium in lanthanum fluoride. [Number 66 in publication list.] We got into that partly because I was consulting with Varian. They were somewhat interested in getting into more fundamental research and they hired a crystal grower who liked to grow lanthanum fluoride crystals and could put various ions in it.

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Riess: Let's continue to review work you did with this group.

Schawlow: I probably shouldn't spend too much time on it, but you asked who some of these people were. Jake [J.Y.] Wong. "Far infrared spectra of V4+ and Co2+ single ions in corundum." [Number 78 in publication list.]

There were so many different things we did. We were trying to understand the splittings of these satellite lines in ruby and we had at one time tried to identify two of these lines coming from the same kind of chromium ion pairs, in which case there should be a far-infrared line connecting these two levels that show the splitting. Well, we thought we would check that out, but we were beginning to doubt it after we studied the thing a little more carefully. Jake Wong was the chief man on that. Mike Berggren helped with that too.

Then it was Ed Nelson who built a far infrared spectrograph for us. [Number 80 on publication list with E.D. Nelson and J.Y. Wong.] I got a huge rod of ruby, about six inches long, dark ruby, and about three-quarters of an inch in diameter. And there was no absorption at those wavelengths, but on the other hand we got a more moderate sized crystal of aluminum oxide containing some titanium and we saw the line. I told you that story earlier. [See p.239] That was work with Nelson and Wong. Nelson built the spectrometer.

Steve Johnson came in the late sixties, and he did some work on excited states in ruby and emerald. He's now at the

University of Utah working on biomedical imaging. I had gotten him to try and build a novel kind of spectrograph. Photographic plates have low quantum efficiency, but they do take all the light all the time. I thought a television type pickup tube would do that even better because it's more sensitive and gets all the light all the time.

Johnson worked for several years building such a spectrograph using an image orthocon, which was state of the art in those days but unfortunately not a great choice for this because it's kind of a finicky thing, not as stable as one would like for a spectrometer. Now, it's very common that people use what they call optical multichannel analyzers, which usually use an array of diodes to take all the light all the time and read it out in scans.

Johnson was very stubborn. At one time we had some money left over and I wanted to buy a commercial tv camera setup and he wanted to build his own. He spent several years doing that, but he learned a lot about imaging, and so he's gone into biomedical imaging ever since.

Another student I had was Stan [E.] Stokowski, who had done an undergraduate thesis with Charlie Townes at MIT, the only one who's ever worked for both of us. I had him doing some studies of line shifts of chromium in strontium titanate, which is a ferroelectric crystal. It's a crystal where the electric field moves the ions around rather easily so you get a large susceptibility. We actually were able to finally see a change in the intensity of the lines as well as the positions when you applied an electric field. [Number 91 in publication list.]

Riess: Much of this sounds like chemistry to me.

Schawlow: It was close. I was a member of the division of chemical physics of the American Physical Society. This sort of stuff was done in the Electrochemical Society too, although I never did get involved with that.

Riess: Peter Toschek?

Schawlow: He was just a visitor, a nice guy. Hänsch worked with him for his Ph.D. thesis. He insisted he wasn't Toschek's student, Toschek was a postdoc there, but they both learned lasers together. Neither of them had done anything with lasers before.

Larry [S.] Wall did work on stress-induced phase transitions in strontium titanate. [Number 101.] I had a lot of students. I had forty altogether.

Chinese Physics Graduates

Riess: You had a number of Chinese students. How was their orientation different from American students? Can you make any generalizations?

Schawlow: Wong had his undergraduate education in this country, at Princeton. He was from Hong Kong and certainly fluent in English.

Riess: And Zugeng Wong?

Schawlow: He was just a visitor [1982-1983], Wong Zugeng.

I visited Shanghai in 1979. That was the first exchange where the Chinese Academy and the National Academy of Sciences agreed to exchange a certain number of lecturers. Each one would go to a different place and they'd bring students from all over China to hear the lectures. So I went to Shanghai Normal University--later it became East China Normal University. That was supposed to be a teacher's college but it had considerable research going on.

There was a professor there named I-shan Cheng who had gotten a Ph.D. at Ohio State in molecular spectroscopy in the 1940s. They weren't giving Ph.D.s at Chinese universities at that time. But they had a number of people doing research, generally under his direction, and he asked if we could have some of them come visit and work in our laboratory. They supplied the money for support for them so I didn't have to pay anything. So I said okay.

And there was also Xia Hui-Rong. Xia is the family name, but she was the wife of Wong Zugeng. She was a good physicist, and she died just a few weeks ago in a bicycle accident on the campus of East China Normal University. She was here for a year, and then she was at the University of Colorado for another year, I think. Her husband, Wong Zugeng, was also a pretty good physicist. I think he became head of the physics department there.

Anyway, she was in a bicycle accident. They don't wear helmets in China, and she somehow hit her head, was in a coma for a week, and died.

Riess: Then there's Zhang Pei-Lin?

Schawlow: Zhang Pei-Lin. He also was not a student, he was a visitor in 1983. He was from the Institute of Physics in Beijing and he was quite a good man too.

There was another Wong in there, Wong Zhao-Young, who was from Fudan University in Shanghai. He was only able to stay for nine months, so he didn't get as much done as the others did. He later became head of the physics department there, but then much to my surprise he moved to Hong Kong and became a member of the physics department at one of the universities, Baptist University, I believe.

I hate to go on print saying that I just can't remember people. And I can't remember a lot of people. It's terrible at times.

Riess: Can you make a generalization about the approach to physics of the Chinese, or the training?

Schawlow: I think the general thing I felt when I visited China in '79, the first time, was that they were very capable and had built just about every kind of laser that had ever been in print, but they didn't have any idea what to do with them. They really didn't have very creative ideas.

The people in Shanghai under Professor Cheng were trying to measure atmospheric pollution using two carbon dioxide lasers, one of which was tuned to an absorption line of a pollutant and the other was tuned off it. And that's a very good way to do it, and they were actually measuring some pollution from smoke stacks. But most of the other people I saw really didn't seem to have any very good ideas. I'm sure it's much better now.

The other thing I found is that they were trained very narrowly. They wouldn't know anything at all about nuclear physics--if they were going into laser physics and optical physics that would be all they'd know. And they'd know that pretty well, what was in the books they would know. But they wouldn't know anything at all about other branches of physics.

Riess: Did you feel that they looked to you as a leader more than other students might have, that the reason they didn't have ideas was because somebody else was always supposed to have ideas? Or they didn't know what they were looking for because somebody else usually told them?

Schawlow: Well, this was when I visited China. The ones who came here, yes, they developed ideas as they went along. Again, I would kind of aim them in some direction and let them go, and then they thought of things, they developed ideas.

Particularly Yan Guang-Yao. He came in that group in 1979. Professor Cheng, who had been very badly treated during the Cultural Revolution, I think sort of looked after Yan, who wasn't a Communist--I think the rest of them were--and because of that he was sort of low man on the totem pole around the university. But Cheng particularly suggested that we take him, and he was really the best of the bunch as far as producing his own ideas, and carrying out experiments too, although the others were okay.

In 1984 when I visited again I didn't have any postdocs and was sort of looking--it would be nice to have somebody--so I invited Yan to come back, and he came with his wife and son. Now he has no intention of moving back to China and he has his green card--I don't think he's a citizen yet. He worked on his Ph.D. for quite a long time.

He was here first just as a visitor. Then when we could we made him a graduate student, when he could do that without having to go back, and he worked there for quite a while. As soon as he could finish his Ph.D. without having to return he finished it. He'd written twenty papers by that time. Then--well, he was close to fifty and his English accent was pretty bad so it would have been hard for him to find a teaching job, but a job opened up running the lecture hall demonstrations at Stanford and he took that on and is doing a good job there. It's not really a research job, but it does require some knowledge of physics and apparatus, which he supplies very well.

Riess: The dead horse that I'm beating--the world view of someone educated in China is not so different that they don't look at questions of physics in a very different way?

Schawlow: I don't think so, no, not the people I knew. They seemed very normal.

I think now, of course, laser physics in China, and spectroscopy, are doing some original things. They have some crystal growers who have developed some special crystals for harmonic generation and mixing of different wavelengths that are some of the best in the world, producing materials that are sold everywhere.

I didn't have any students who were directly from mainland China. We did start admitting a few in the eighties. We had to keep the numbers down because we're not a very big department and we could easily have filled the place up with Chinese students. I think, though, that the ones who came did pretty well. They had strong theoretical grounding. It would

have been hard to sort them out, but T.D. Lee had arranged for examinations to classify these people and that was a big help.

Riess: What do you mean "sort them out"?

Schawlow: Well, to find out which ones were really good.

Riess: You mean at the point where they're applying?

Schawlow: Yes.

Riess: Where was T.D. Lee?

Schawlow: He's a professor at Columbia, from China originally. He got a Nobel Prize in the 1950s for discovering the nonconservation of parity. Both he and C.N. Yang, who shared the prize with him, they've both come from China and they've done a lot to try and help Chinese physics.

Summing up the Seventies

Riess: When Ted Hänsch came in 1970 the original whole balance--it's not like a balance of power, but something shifted.

Schawlow: Yes, sure, the direction of things.

We had some money for the first time. We had that equipment grant and tunable lasers had just been discovered, and he improved them considerably. But we could, for the first time, do some laser spectroscopy. Up until then we'd just been mostly studying the properties of materials related to lasers, we hadn't really been doing work on lasers so much except what Emmett did, or building lasers for special projects. So we switched over, really cut down working on solids and I think Gary Klauminzer was probably the last one to work on ions in crystals.

Then I sort of started following up on some of the things that Hänsch had started. Well, some things were my own. I had worked on Brillouin scattering, and also the intermodulated fluorescence, which was the way to get sensitive detection of weak lines which has been used by some other people too.

Riess: In fact, the set of questions that you had initially asked as a graduate students were beginning to be answered at the end of your research.

Schawlow: Yes, certainly getting rid of the Doppler broadening, that was pretty well under way with the saturation of intermodulated fluorescence and so on, and other methods of polarization and intermodulation.

But it's the old story--a lot of things that were terribly difficult to do at one time, like when I was a student, become easy, but they're done. [laughs] So you have to keep on looking for other things.

Riess: When Ted Hänsch came did your role vis-à-vis students change?

Schawlow: Well, he had his students and I had mine. And I didn't have much to do with his students. I guess at first even before he became associate professor there were some of them that were formally reporting to me but actually being supervised by Ted.

It is true that toward the end I was really letting him have all the resources I could and really making do with things that he wasn't interested in, equipment that he was tired of. So he did cause some constraints on space and money, but still he was so good that I just really wanted him to have every opportunity that he could. And of course we were fighting to keep him because other places were trying to hire him away, Harvard and Yale among them, and Heidelberg, and then finally Munich got him.

Riess: What was the financial situation in those years? You had the equipment then.

Schawlow: I got the equipment grant just about the time he came, but I never got another one.

We did have what the National Science Foundation claimed was the biggest grant in their atomic physics program. They were used to fifty thousand dollar grants and ours was probably about three hundred thousand. But it sure wasn't enough for all the things we wanted to do. We had to pay huge overhead on any salaries or any supplies. We had to pay employee benefits of something like twenty-five percent, and overhead on salaries after benefits, including the benefits, something like sixty percent.

I think it was true that if I hired a person it would cost me just for his salary twice as much as I was paying him. So that made it very expensive. It was one reason why I stopped having any postdocs. If somebody came with their own money, that was all right, but I couldn't afford to hire them.

After I retired, a man from the Office of Naval Research who had been helping us said that he wanted me to have seventy-five thousand a year so that I could hire a postdoc. Well, I figured if I hired a postdoc, I couldn't pay less than about thirty thousand for salary, and with overhead I think it would be over sixty thousand. That would leave very little money for equipment or supplies, and I just wasn't able to do it.

Riess: Was that an area where you did battle with Stanford?

Schawlow: No. I'm afraid I just took what I could get. I felt it was hopeless. Other people were trying to fight it, not I. The university wanted all the money they could get their hands on.

Then of course they got into trouble with the government over charging too much. The Office of Naval Research had a representative at Stanford who could approve our payments, and usually these were people who'd go along quite nicely with whatever we wanted to do. But then they got a guy who wanted to make trouble and he caused a lot of trouble and he found a few skeletons in the Stanford closet, nothing to do with me, but they then cut their reimbursement rate for overhead substantially. I don't think it's ever gotten up to what they had before though it's still pretty high.

News of the Nobel Prize--Putting the Money to Work for Artie

Riess: Let's go now to the happy subject of the Nobel Prize. Tell me about it, and also tell me what you did with your prize money.

Schawlow: Well, first thing I heard of it was that I had this phone call at four o'clock in the morning from a radio reporter. He wanted to know first of all what had I done to get this Nobel Prize, and I couldn't tell because I had published a hundred and sixty-seven papers and I didn't know which combination they were honoring me for. In fact, it was some time quite late in the day before I got the actual citation. [laughter]

Nowadays and before that, I think usually there's a phone call from the Swedish Academy. But I didn't get one. I did get a telegram from the Swedish ambassador I think, but it was days later that I got anything direct from the Academy.

Riess: There is a picture of you and Aurelia on the telephone in your kitchen. I take it that was posed.

Schawlow: Well, yes and no. I guess so. Anyway, that was early in the morning, and of course the phone calls started coming. Then the *Palo Alto Times*, I guess, wanted to send a photographer. So Aurelia insisted that I take the phone off the hook and get dressed.

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Schawlow: Yes. This reporter also asked me what I was going to do with the money. I told him I had an autistic son and that we were working with some others to try and set up a group home for autistic people. Well, actually, I did give five thousand dollars to a Peninsula Children's Center which was trying to plan something, but what they were planning just wasn't suitable, so we dropped out of that. They felt that you could just have a program for a few years, and autistic people are not going to be cured in a few years.

So then we found a group home for Artie, and we offered to pay for an extra staff member. We paid them, I think, $2500 a month or something like that for some months. I think we spent over twenty thousand dollars on that.

Well, of course we spent a few thousand dollars on the trip, because I took my wife and daughters and they needed six evening gowns. [laughter] They borrowed fur coats from various friends, so they didn't have to buy a fur coat which you couldn't use in Palo Alto. We didn't go the cheapest way. We stopped in London both ways, and in New York. We spent about seven thousand dollars on those expenses. So that accounts for about thirty-four thousand, and I'm sure the rest of it all went for things that Artie needed. I've spent hundreds of thousands on things for him.

Riess: You used that as a public opportunity to talk about autism.

Schawlow: Yes, I did, and it was appreciated by the Autistic Society. They gave me a plaque.

A wonderful thing came out of it. When we went to Stockholm, we met Karin Stensland Junker. She was the mother of an autistic girl and had written a book about her daughter. Then she got a Ph.D. in clinical psychology working on that. She told us about a young man who had come to her office and he couldn't talk--he was twenty-four years old--but he could type on the keyboard that looked about the size of a calculator, but it typed letters instead of numbers and it printed them out on a paper tape.

She asked him, "Could I have some of your tapes?" And he replied, in Swedish of course, "No." She said, "Why not?" He said, "You can't read it when the sun shines." Well, it fades in sunlight, it was a thermal printing.

I thought, "Gosh, if Art could do that!" He might be able to understand that, but we had no way of telling.

Then when we came home, I was giving talks all over the country to various Autistic Society people. And in Memphis I told somebody about this and he said, "That sounds like a Canon Communicator," and gave me an address in Seattle. It turned out that they were just modifying it for special handicaps, like for people who needed to operate it with a stick in the mouth. But they told me where the American distributor was and it turned out that was a company that was about a mile away from my home.

I went over there, and they would've lent me one, but I knew it was going to be more difficult so I bought one for six hundred dollars. And it was a flop. He just hit XXX and ZZZ and so on.

Well, we've told the full story in that article that we wrote about him. We tried various things. Finally it was a couple of years later, almost two years later, that he began to actually communicate with us.

Riess: So first he resisted it?

Schawlow: Yes. But then we tried other things, like trying to get him to pick out letters and put them into blocks where they would fit.

Then we got this Texas Instruments Touch and Tell, where the synthesized voice asks, "Show me the red letter 'R'," something like that, and then if you get it then they say, "That's good," or something like that, and move on to the next.

The first week he just didn't seem to know what it was all about. Next week, he did all right off, he knew the whole alphabet. Then we tried cards with pictures and with words, matching words to pictures. He could do that pretty well. Then we had him picking out words from a magazine page and he could do that too.

Then we met a speech therapist who showed us how to use a communication board where you put the words on. You can make choices, like for snacks or job tasks. You point to a word. We thought he might need pictures, but he didn't. With just the words, he could do it. So he obviously could read some. I

guess we'd had another teacher for a short while who began to show us that he could recognize letters. That happened along there. I forget exactly where that came in.

Then we had this first laptop computer, the Epson HX-20. I programmed it to show a word with a dash under each letter. Nothing would happen unless you pressed the right key, then the letter would appear. And at the end of the word, if he got the word complete, it would print it out on a strip printer that was built in like an adding machine printer. He loved that. He would tear off these things and stuff his pockets with these tapes until he had used up all the tape.

But this was still not communicating. So then one day I thought, "Well, I'll let you choose what kind of pizza you want." We were at the park and we'd usually go for pizza after that. I put down cheese, sausage, and pepperoni. He chose sausage by pointing to it. I said, "Let's confirm that by typing it out." And he typed it out with my hand on his.

Then it was just a week or two later that we were in the ice cream parlor, and he waved for something over in one direction. We acted dumb and said, "Let's go to the car and get the communicator. You can tell us." He typed out "shoes" and there was a shoe store there, so we bought him shoes, and that was a big breakthrough.

Riess: Your hand on his, of course, is the controversial part of the whole business.

Schawlow: Yes, I know. But that was the way to get his hand on the keyboard. And he still seems to want it. He doesn't usually want to point at anything without a hand on his. Very occasionally he forgets that he needs that and will do something. There have been some studies on how to achieve independence.

It's been hard for us because we'd only see him every few weeks for an hour or two and we wanted to get the communication. But I really wish we could work on independence because we're not always going to be around. Fortunately some other people have been able to pick it up and can get something out of it. Some, particularly Martha Leary, and Aurelia when she was alive, are very good, they can get a lot out of him. Others--well, when he really wants something, he can tell them.

Riess: He probably really wants you and that's the way of staying connected.

Schawlow: I guess so, but he doesn't tell me a lot. He's not very communicative.

Riess: Here you were coming back from your Nobel Prize event and speaking around the country on autism. Receiving the prize also meant the beginning of another flood of speaking activities.

Schawlow: Yes, it was bad. I had been doing so many things, I got the flu quite bad in January of 1982. Then I got it again the following year. Too much travelling and being run down. Since then I've been taking flu shots and I've only had it once. But I find that flu shots and pneumonia shots are not totally effective.

Current Work

Riess: Now, today, what are the questions you're still wishing, if you had time in your lab, you could answer?

Schawlow: I got interested in trying to see these rare earth ions, which have fairly sharp lines even in solids because they're somewhat shielded from their neighbors, I wanted to see what they could tell us about metals, or conducting semi-metals. So I had students search for these lines. In fact, I just got the proof of an article that I've written for a memorial issue of *Physica*, the Swedish physics magazine, a memorial issue for George Series.

I start off by saying that he had done some wonderful things on the details of spectral lines, but there were other things where you had to look. I said. "There's an old recipe for rabbit stew that starts, 'First, catch a rabbit.'" [laughter] In this case, we didn't know where these lines would be, we had to search. Unfortunately, nobody knows how to search over a wide range using lasers. They're more like too sharp a searchlight when you need a flood light.

I would have liked to have taken a laser and studied some of these lines as they go through the superconducting transition. The superconductor theorists like Phil Anderson say this is not very interesting because these ions are not directly involved in the superconductivity. They have to be there somehow, but just the copper oxide layer is where the superconductivity is. That's what they say.

Well, I don't think this is the most important thing but it's interesting, it's a puzzle.

Riess: As it becomes simply less convenient to simply be in your lab, from parking to the other things that are going on in your life, do you find yourself becoming more of a theorist?

Schawlow: No, I'll never be a theorist. A theorist is one who can do mathematical calculations. I can think about the physics, the theory in broad terms. That's all I can do. I'm not doing much of that, but I am doing a little of that. I'm trying to plow through where other people have plowed, and it's not likely that I'll discover anything worthwhile, but I'm still intrigued by the puzzles and try to think about them.

Riess: Do you use your computer to get onto the physics websites?

Schawlow: No, not at all. I don't know whether my kind of physics would be on there. Probably would. I've found some people post their papers on the net. I haven't tried. I should try for lasers and nonlocality and things like that.

Riess: I think you should because if anyone can get plugged in, it's you.

Schawlow: I guess so. You can certainly spend an infinite amount of time with computers.

[pause]

Schawlow: I've always told my students that to discover something new, you never have to know everything about a subject, you never can. All you have to do is recognize one thing that's not known.

Riess: Charlie Townes said in the introduction to the book in your honor said that you had a role in the American Physics Society and other organizations in shaping policy for the world of physicists.[1]

Schawlow: Well, I don't I really had too much to do with it. I was on the various boards, director of the Optical Society and the Physical Society, and I was president of each of them. On the big committees maybe you can nudge things slightly in some direction--actually the executive officers, or whatever they're

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[1]p. vii, *Lasers, Spectroscopy and New Ideas, A Tribute to Arthur L. Schawlow*, Springer Series in Optical Sciences, 1987.

called, executive secretaries, are the people who really run those things.

Riess: He must be referring to something.

Schawlow: I can't think of anything terribly important, just to try and keep them on the right path.

I did help to get the Optical Society more deeply into serious physics. There was one time they had a joint meeting of the Optical Society and the American Physical Society and I organized a really high level session on "What is light?" and got some really top people to give some talks. So, I think I sort of helped to raise the tone of the Optical Society, although I certainly didn't do it alone. There were a lot of other good people.

The Physical Society? I don't know, I gave them whatever advice I could give, but I don't *really* think that I changed the direction appreciably.

Riess: I thought perhaps it might have been vis-a-vis issues in military or war.

Schawlow: No, I avoided them. I really was bad, I didn't. Most other presidents have pontificated on such subjects, but not me.

Thinking in Classical Pictures

Riess: You have certainly a reputation--and I can tell from this oral history--as a terrific public speaker. That's a great gift.

Schawlow: I don't know. I gradually gained confidence.

It partly comes back to the way I think. I realize that even when I was a student that if I had any real ability, it was that I could look at a subject and say, "Now, what's really the important thing here? What's it all about? Never mind the details." And that's a good thing to do when you're trying to write a presentation, or a paper, or give a talk. If you can grasp what it's all about, then you can maybe make it clear and give the illusion to people that they understand it.

I guess I told you I've some horrible experiences--well, they turned out all right. Like the first time I was invited to give a talk at the American Physical Society and I was on the program following Feynman. [laughter]

Riess: You really wish to communicate what you are doing, too.

Schawlow: I think so. As I say, I think gradually as you gain confidence, as you have some success, then you're willing to stick your neck out and make bolder statements and predictions.

Riess: And somewhere I've written down, "Schawlow has little patience with abstract theory or tedious mathematical derivations."

Schawlow: That's for sure. As I've sometimes said, I think in fuzzy pictures. [laughing] I'm not an artist, I can't draw anything, but I do think in pictures. They are probably not as clear or sharp as a person with more artistic ability could do, but I like to picture things.

In some ways, I'm sort of out of step with the world because I guess I think more in classical pictures. I find quantum theory very puzzling. Well, everybody agrees it's puzzling now! But the orthodox view is that there's no use trying to think of concrete pictures for things you can't measure--like what happens between here and there, when light is emitted and when it's absorbed. But I keep trying.

Riess: Does fuzzy logic make it more acceptable to think in fuzzy pictures?

Schawlow: No. Fuzzy logic just means that instead of having things always off or on, you can adjust them. This is the sort of thing we've always done as humans. Like when you turn your radio set on, you set the volume at the level you find convenient and not always full on or full off. I don't think there's much more to fuzzy logic than that, except that they're able to do it in a systematic computer way.

Riess: Do you find chaos theory helpful in thinking about the world?

Schawlow: No. The general idea certainly is true that some things--the image of a butterfly starting a storm--it's true that a lot of things are easily tipped by a slight initiation one way or the other. But I don't think that really interacts much with anything I think about.

A Few Last Stories to Tell

Riess: I want to ask you a question that I don't usually ask, but I'd like to get it on the record. It's awkward to ask.

Schawlow: That's all right.

Riess: What would you like to have gotten out of having done the oral history?

Schawlow: Well, I thought what I wanted to do was to have a summary of what I've done so that I could start writing my autobiography and have the facts down there and try put them into perhaps a little different shape, with maybe a few more jokes. [laughs] I mean, I've seen some funny things. But strangely enough, I'm feeling rather discouraged. I feel that now I've gone through this stuff, I don't think it's very interesting and I don't know how I can make myself face it.

Riess: I think I've spent too much time trying to cover issues in physics while you are wishing to tell a more lighthearted version of your life.

Schawlow: Yes, well, that could easily be done later. I don't know, I don't think I have the ability to write it, but I'm not sure. If I ever can get some time to think, I'll have to think about that. But there just seems to be an endless number of things that have to be done.

For instance, I got a letter from a lady in Florida who wants to know if I know a school district in California where they allow facilitated communication. Well, I don't, but I can't really say simply that I don't. I'll have to do some digging and see if I can find something, find somebody that might know something.

Riess: The autism research has been a secondary field of your life.

Schawlow: Yes, it has. I've had a curiously semi-detached view of things. What works for me and for Artie is all I care about, really. But of course you can't really help one person without helping others. Like he couldn't live by himself, he has to have a group home, and when I help the group home I help him. I don't know. Well, I'll call a few people, see if I can get leads on that one.[1]

But there just seems to me an awful lot of stuff. For instance, I spend a lot of time sorting out my records and CDs, and then unfortunately getting new ones.

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[1]July 1997, Arthur Schawlow notes that he did find such a school district in California.

Riess: About getting the jokes in, and the lightness, it's hard to testify to one's own wit and humor.

Schawlow: Yes, well I have a few stale jokes that I keep using over and over again. Or I don't have jokes, really. I have a spiel that I use when I break balloons.

There have been some funny things that have happened that I would like to include if I think of them as I go along, and I probably have included some. Not necessarily things that I did. There are some funny things that I've thought up and which I treasure and sort out. I'll crack jokes and sometimes they'll fall flat, in which case I won't reuse them.

Riess: In the classroom?

Schawlow: Sometimes. Sometimes in meetings. It's often good to lighten the meetings. When things get too serious, it helps to have a little bit of a joke.

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Schawlow: Things just occur to me on the spur of the moment. For instance, there was this talk about "death rays." So I made a slide from a picture in the encyclopedia of knights in shining armor and I called it "our laser countermeasures." The shiny metal would reflect most of the laser light. [laughter]

I was being interviewed by a reporter and I said that as soon as there were any lasers the science fiction writers, "or newspaper reporters, as they're sometimes known," thought this was all for weaponry. [laughter] These things just kind of come out of the situation.

[tape interruption]

Schawlow: [Schawlow turns to his music collection] I have a machine that allows you to scan sheet music into it and transpose it and print it out in a different key. For clarinet, you know, you can take piano things and transpose them. If I ever do play the clarinet again. Right now it's just kind of speculation.

Riess: Do you think you will get back to the clarinet? It might be good for your lungs.

Schawlow: I don't really know. That's what the physical therapist said, I really ought to do it, for my lungs. I do a lot of just kind of sitting, staring at newspapers and magazines, things like that. I don't have a lot of energy. But I'm getting better.

[Schawlow puts on a CD] I got this record--I used to go to New York several times a year and I would go, always, to Jimmy Ryan's, where usually Joe Muranyi was playing clarinet. He's Hungarian descent and speaks the Hungarian language and in recent years has been going back and forth to Hungary where he's a big hero as a jazzman who played with Louis Armstrong.

[tape interruption]

Schawlow: [Schawlow reviews with Riess the spelling of the names of some Chinese physicists] Before we went to China we took a short course in Chinese in night school run by Foothill Junior College in Palo Alto. In China, everywhere we were we went to a Friendship Store. They have souvenirs and things. I would tell my wife "Wode chyan bugou"--my money's not enough. [laughter]

The course was taught by a nice old Chinese gentleman, Dr. P.F. Tao, who had gotten a Ph.D. in Berlin many years before. I liked that course so much that I thought, two years later, I'd go back and take a little more.

Anyway, then we came across the word "chang" meaning "to sing," and also the word "chang" meaning "often." So I asked Mrs. Xia, who was then visiting in our lab, "How do you say, 'We often sing Chinese songs?'" And she said, "Women tsang tsang jungwo ger." I told the teacher this and he said, "Oh yes, that's the Shanghai dialect." [laughter] Apparently the dialect all the way through the middle of China is something like that because people in Chungking were doing that too.

When we were in China it was interesting listening to people on the street. We could occasionally make out a word. I really had very little vocabulary and we had not studied characters at all. In this night school Chinese class there were several people of Chinese origin, but they spoke Cantonese and they wanted to learn to speak Mandarin.

That second attempt to learn Chinese came in the autumn of 1981. In October I learned that I had to make the trip to Stockholm for the Nobel Prize and I had many new things to get done. So I went back to the class the next week and told Dr. Tao that I couldn't continue with the course. He said, "We understand, and we're greatly honored to have you. We would like to put on a banquet in your honor."

So, two or three weeks later they had a banquet at a local Chinese restaurant. It was attended by that class and another, and there was an orchestra of students playing Chinese musical instruments. A student reporter for the Foothill Junior

College newspaper was present at the event. A couple of weeks after that, an article appeared with the heading, "Foothill Dropout Honored for Nobel Prize." [laughter]

The main trouble with learning to speak a foreign language is that when you say something, then they answer you, and then you're lost. You can carefully compose a sentence, but you never know what's going to happen.

Did I tell you about my encounter with Italian?

We were in Florence. My wife, who had studied some Italian--I had not, although I had listened to a record of Italian phrases--insisted that I buy the tickets to Rome. So I went up to the counter--I had carefully prepared--and I said, "Due bigletti per Roma, primo classe, con posti reservati"--"two tickets to Rome first class with reserved seats." I had the time written down on a piece of paper.

He said, "Si, oggi?" And I had no idea what he meant. Then he said, "Today?" [laughter] A perfect example of where you can get thrown by lack of vocabulary.

Riess: Professor Schawlow, here is where the interview ends.[1] Are you content with our linguistic discussion as the final note? Or do you want to sum up in some way, as if you knew that you were having a last say?

Schawlow: [written addition] Well, I always told my students that there are three rules for writing: (1) have something to say (that's the hardest part), (2) say it, and (3) stop.

However, looking back, I have had some success and a lot of satisfaction in life. It has been hard work, and I have stupidly overlooked some good things that I might have found. Perhaps my focus was too narrow at times, but I felt that I had to concentrate on what seemed most important and yet attainable. The only fact that saved me was that others overlook things, and so, as I told my students, there are still lots of simple and beautiful things to be discovered. I really believe that.

I did have some wonderful research collaborators and students, and they helped to make up for my deficiencies. Science is cumulative and many very able people have taken up some of my work, and carried it far beyond anything I imagined.

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[1]This final question to Arthur Schawlow was added and answered in the editing stage, after the interviews were concluded.

Thus, while some of my results are properly forgotten, others have gone so far as to make me look a lot more prescient than I ever was. In all, physics has been intriguing, at times frustrating and compelling, but very worthwhile. I can't imagine doing anything else with my life.

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TAPE GUIDE--Arthur Schawlow

APPENDIX

Publications by ARTHUR L. SCHAWLOW

1. Nuclear Moments of Silver (M.F. Crawford, ALS, W.M. Gray, and F.M. Kelly), Phys. Rev. 75, 1112 (1949) (letter).

2. Transmission and Reflection Coefficients of Aluminum Films for Interferometry (M.F. Crawford, W.M. Gray, ALS, and F.M. Kelly), J. Opt. Soc. Am. 39, 888 (1949).

3. Electron-Nuclear Potential Fields from Hyperfine Structure (M.F. Crawford and ALS), Phys. Rev. 76, 1310, (1949).

4. Nuclear Moments of ^{25}Mg (M.F. Crawford, F.M. Kelly, ALS, and W.M. Gray), Phys. Rev. 76, 1527 (1949) (letter).

5. Hyperfine Structure and Nuclear Moments of ^{207}Pb (ALS, J.N.P. Hume, and M.F. Crawford), Phys. Rev. 76, 1876 (1949) (letter).

6. Isotope Shift in the Resonance Lines of Zinc (M.F. Crawford, W.M. Gray, F.M. Kelly, and ALS), Can. J. Res., A, 28, 138 (1950).

7. An Atomic Beam Source and Spectrograph for Hyperfine Structure. Nuclear Moments of Silver (M.F. Crawford, ALS, F.M. Kelly, and W.M. Gray), Can. J. Res., A, 28, 558 (1950).

8. Nuclear Magnetic Moments and Similarity between Neutron and Proton States in the Nucleus (ALS and C.H. Townes), Phys. Rev. 82, 268 (1951) (letter).

9. Significance of the Results of Microwave Spectroscopy for Nuclear Theory, Ann. New York Acad. Sci. 35, 955 (1952).

10. Microwave Spectrum and Structure of ReO_3Cl (E. Amble, S.L. Miller, ALS, and C.H. Townes), J. Chem. Phys. 20, 192 (1952).

11. Charge Distribution in Nuclei from X-Ray Fine Structure (ALS and C.H. Townes),Science, 115, 284 (1952).

12. Quadrupole Coupling Ratio of the Chlorine Isotopes (T.C. Wang, C.H. Townes, ALS, and A.N. Holden), Phys. Rev. 86, 809 (1952) (letter).

13. A Microwave Spectrum of the Free OH Radical (T.M. Sanders, Jr., ALS, G.C. Dousmanis, and C.H. Townes), J. Chem. Phys. 22, 245 (1954).

14. Examination of Methods for Detecting OH (T.M. Sanders, Jr., ALS, G.C. Dousmanis, and C.H. Townes), J. Chem. Phys. 22, 245 (1954).

15. Hyperfine Structure in the Spectrum of $N_{14}H_3$. I. Experimental Results (G.R. Gunther-Mohr, R.L. White, ALS, W.E. Good, and D.K. Coles), Phys. Rev. 94, 1184 (1954).

16. Nuclear Quadrupole Resonances in Solid Bromine and Iodine Compounds, J. Chem. Phys. 22, 1211 (1954).

17. Structure of the Intermediate State in Superconductors (ALS, B.T. Matthias, H.W. Lewis, and G.E. Devlin), Phys. Rev. Letters 95, 1344 (1954).

18. Effect on X-Ray Fine Structure of Deviations from a Coulomb Field near the Nucleus (ALS and C.H. Townes), Phys. Rev. 100, 1273 (1955).

19. Microwave Spectroscopy (C.H. Townes and ALS), McGraw-HillBook Company, New York (1955). (Reprinted by Dover Publication 1975).

20. Structure of the Intermediate State in Superconductors, in Proceedings of the Conference de Physique des Basses Temperatures, CNRS, Paris 1955.

21. Crystal Structure and Quadrupole Coupling of Cyanogen Bromide, BrCN (S. Geller and ALS), J. Chem. Phys. 23, 779 (1955).

22. Structure of the Intermediate State in Superconductors, Phys. Rev. 101, 573 (1956).

23. Intermediate State Of Superconductors: Influence of Crystal Structure (ALS and G.E. Devlin), Phys. Rev. 110, 1011 (1958).

24. Structure of the Intermediate State of Superconductors (ALS and G.E. Devlin), in Proceedings of the Fifth International Conference on Low Temperature Physics and Chemistry, Madison, Wisconsin, 1957, J.R. Dillinger, ed., University of Wisconsin Press (1958), p. 311.

25. Penetration of Magnetic Fields through Superconducting Films, Phys. Rev. Letters 109, 1856 (1958).

26. Infrared and Optical Masers (ALS and C.H. Townes), Phys. Rev. 112, 1940 (1958).

27. Effect of the Energy Gap on the Penetration Depth of Superconductors (ALS and G.E. Devlin), Phys. Rev. 113, 120 (1959).

28. Structure-Sensitivity of the High-Frequency NMR in Powdered Anntiferromagnetic MnF_2 (J.L. Davis, G.E. Devlin, V. Jaccarino, and ALS), J. Phys. Chem. Solids 10, 106 (1959).

29. Superconductivity, in Experimental Methods in Physics, Academic Press, New York (1959), Vol. 6, p. 71.

30. Intermediate State of Hard Superconductors (ALS, G.E. Devlin, and J.K. Hulm), Phys. Rev. 116, 626 (1959).

31. Electronic Spectra of Exchange-Coupled Ion Pairs in Crystals (ALS, D.L. Wood, and A.M. Clogston), Phys. Rev. Letters 3, 271 (1959).

32. Optical Detection of Paramagnetic Resonance in an Excited State of Cr^{3+} in Al_2O_3 (S. Geschwind, R.J. Collins, and ALS), Phys. Rev. Letters 3, 545 (1959).

33. Self-Absorption and Trapping of Sharp-Line Resonance Radiation in Ruby (F. Varsanyi, D.L. Wood, and ALS), Phys. Rev. Letters 3, 544 (1959).

34. Optical Detection of Paramagnetic Resonance in Crystals at Low Temperature (J. Brosset. S. Geschwind, and ALS), Phys. Rev. Letters 3, 548 (1959).

35. Infrared and Optical Masers, in Quantum Electronics, Columbia University Press, New York (1960), p. 553.

36. Coherence, Narrowing, Directionality, and Relaxation Oscillations in the Light Emission from Ruby (R.J. Collins, D.F. Nelson, ALS, W.Bond, C.G.B. Garett, and W. Kaiser), Phys. Rev. Letters 3 (1960).

37. Infrared and Optical Masers, Bell Laboratories Record 38, 403 (1960).

37A. Infrared and Optical Masers, J. Am. Soc. Naval Engrs. 73, 45 (1961).

38. Optical Masers, Northeast Electronics Research & Engineering Record, 158 (1960).

39. Zeeman Effect of the Purely Cubic Field Fluorescence Line $MgO:Cr^{3+}$ Crystals (S. Sugano, ALS, and F. Varsanyi), Phys. Rev. 120, 2045 (1960).

40. Corbino Disk (D.A. Kleinman and ALS), J. Appl. Phys. 31, 276 (1960).

41. Simultaneous Optical Maser Action in Two Ruby Satellite Lines (ALS and G.E. Devlin), Phys. Rev. Letters 6, 96 (1961).

42. Nuclear Quadrupole Resonance in an Antiferromagnet (J.C. Burgiel, V. Jaccarino, and ALS), Phys. Rev. 122, 429 (1961).

43. Strain-Induced Effects on the Degenerate Spectral Line of Chromium in MgO Crystals (ALS, A.H. Piksis, and S. Sugano), Phys. Rev. 122, 1469 (1961).

44. Infrared and Optical Masers, Sol. St. J. 2, 21 (1961).

45. Optical Masers, Sci. Am. 204, 52 (1961).

45A. Optical Masers, Usp. Fiz. Nauk. 75, 569 (1961).

45B. Optical Masers, in Akceleratory, Reaktory, Lasery, Panstwowe Wydawnictwo Naudowe, Warsaw (1964). (Reprinted).

46. Fine Structure and Properties of Chromium Fluorescence in Aluminum and Magnesium Oxide, in Advances in Quantum Electronics, J.R. Singer, ed., Columbia University Press, New York (1961), p.50.

47. Composite Rod Optical Maser (G.E. Devlin, J. McKenna, A.D. May, and ALS), Appl. Opt. 1, 11 (1962).

48. Fine-line Spectra of Chromium Ions in Crystals, J. Appl. Phys. Suppl. 33, 395 (1962).

49. Masers, in Collier's Encyclopedia (1962), p. 493.

49A. Masers, Electro-Technology, J. Soc. Elec. Engr., Electronics and Radar Development Establishment, Bangalore, 6, 199 (1962). (Reprinted).

50. Lasers, Encyclopedia of Science and Technology, McGraw-Hill Book Company, New York (revised-1969), p. 406 a-d.

50A. Optical Masers (P. Kisliuk and ALS), in McGraw-Hill Encyclopedia of Science and Technology Yearbook, McGraw-Hill Book Company, New York (1962). p. 380.

51. Optical Masers, in Proceedings of 1961 London Conference on Optical Instruments and Techniques, K.J. Habell, ed., Chapman and Hall, London (1962). p. 431.

52. Coherent Light for Communications, in Proceedings of the Symposium on Tracking and Command of Aerospace Vehicles, Institute of Aerospace Sciences, New York (1962).

53. Tilted Plate Interferometry with Large Plate Separations (H.W. Moos, G.F. Imbusch, L.F. Mollenauer, and ALS), Appl. Opt. 2, 817 (1963).

54. Optical Masers (Lasers), (ALS and H.W. Moos), in McGraw-Hill Encyclopedia of Science and Technology Yearbook, McGraw-Hill Book Company, New York (1963).

55. Crystals and Light, Stanford Research Inst. J. 6, 3 (1962).

56. Widths and Positions of Sharp Optical Lines in Solids, in Quantum Electronics III, Proceedings of the Third International Conference, Paris, 1963, P. Grivet and N. Bloembergen, eds.,Columbia University Press, New York (1964), p. 645.

57. Energy Levels in Concentrated Ruby (P. Kisliuk, ALS, and M.D. Sturge), in Quantum Electronics III, Proceedings of the Third International Conference, Paris, 1963, P. Grivet and N. Bloembergen, eds., Columbia University Press, New York (1964), p. 51.

58. The High Gain Laser as a Wavelength Standard (L.F. Mollenauer, G.F. Imbusch, H.W. Moos, ALS, and A.D. May), in Proceedings of the Symposium on Optical Masers, Polytechnic Institute of Brooklyn, April 16-19, 1963, J. Fox, ed., Polytechnic Press, Brooklyn, New York (1963), p. 51.

59. Enhanced Ultraviolet Output from Double-Pulsed Flash Lamps (J.L. Emmett and ALS), Appl. Phys. Letters 2, 204 (1963).

60. Advances in Optical Masers, Sci. Am. 209, 34 (1963).

60A. Advances in Optical Masers, Usp. Fiz. Nauk 81, 745 (1963). (Reprinted)

60B. Advances in Optical Masers, abridged translation "Le Applicazioni Pratiche del Laser" (Sapere, no. 650, p. 102 ((1964).

61. Optical and Infrared Masers, Contemp. Phys. 5, 81 (1963).

62. Temperature Dependence of the Width and Position of the ^2E-$^4A_2$ Fluorescent Lines of Cr^{3+} and V^{2+} in MgO (G.F. Imbusch, W.M. Yen, ALS, D.E. McCumber, and M.D. Sturgge), Phys. Rev. 133, A1029 (1964).

63. Optically Pumped Masers and Solid State Masers, in Proceedings of the International School of Physics "Enrico Fermi," Course XXXI, Quantum Electronics and Coherent Light, Varenna, Italy, 1963, P.A. Miles, ed., Academic Press, New York (1964).

64. Lasers and Coherent Light, Phys. Today 17, 28 (1964).

65. Direct Measurement of Xenon Flashtube Opacity (J.L. Emmett, ALS, E.H. Weinberg), J. Appl. Phys. 35, 2601 (1964).

66. Phonon-Induced Relaxation in Excited Optical States of Trivalent Praseodymium in LaF_3 (W.M. Yen, W.C. Scott , and ALS), Phys. Rev. 136, A271 (1964).

67. Isotope Shifts in the R Lines of Chromium in Ruby and MgO (G.F. Imbusch W.M. Yen, ALS, G.E. Devlin, and J.P. Remeika), Phys. Rev. 136, A271 (1964).

68. A Portable Demonstration Laser (K.H. Sherwin and ALS), July, 1964, (Not for publication).

68A. How the Ruby Laser Works, Popular Science, November 1964, taken in part from "A Portable Demonstration Laser."

69. Fluorescence of $MgO:Cr^{3+}$ Ions in Non-Cubic Sites (G.F. Imbusch, ALS, A.D. May, and S. Sugano), Phys. Rev. 140, A830 (1965).

70. Lasers, Science 149, 13 (1965).

71. Lasers (H.G. Freie and ALS), in Advanced Optical Techniques, A.C.S. van Heel, ed., North-Holland Publishing Company (1967), p. 467.

72. Measuring the Wavelength of Light with a Ruler, Am. J. Phys. 33, 922, (1965).

73. Observation of a Spin Wave Sideband in the Optical Spectrum of MnF_2 (R.L. Greene, D.D. Sell, W.M. Yen, ALS, and R.M. White), Phys. Rev. Letters 15, 656 (1965).

74. Lasers, in the Symposium in International Ophthalmology Clinics (A Quarterly Book Series), Little, Brown and Company (1966), 6, No. 2, p. 241.

75. Effect of Ultraviolet Pumping on Ruby Laser Output (R.L. Greene, J.L. Emmett, and ALS), Appl. Opt. 5, 350 (1966).

76. Magnetic Effects in the Optical Spectrum of MnF_2 (D.D. Sell, R.L. Greene, W.M. Yen, ALS, and R.M. White) in Proceedings of the Conference on Magnetism and Magnetic Materials, 1965, in J. Appl. Phys. 37, 1229 (1966).

77. Beam of the Future, in Science Year, The World Book Science Annual 1965, Field Enterprises Education Corporation (1965), p.167.

78. Far Infrared Spectra of V^{4+} and Co^{2+} Single Ions in Corundum (J. Y. Wong, M. J. Berggren, and ALS)), in Proceedings of the Conference on Optical Properties of Ions in Crystals, 1966, H. M. Crosswhite and H. W. Moos, eds., Interscience Publishers, New York (1967), p. 383.

79. Far Infrared Spectra of $Al_2O_3:Ti^{3+}$ (E. D. Nelson, J. Y. Wong, and ALS), Phys. Rev. 156, 298 (1967).

80. Far Infrared Spectra of $Al_2O_3:Cr^{3+}$ and $Al_2O_3:Ti^{3+}$ (E. D. Nelson, J. Y. Wong, and ALS), in Proceedings of the Conference on Optical Properties of Ions in Crystals, 1966, H. M. Crosswhite and H. W. Moos, eds.,Interscience Publishers, New York (1967), p. 375.

81. Thermal Shifts in the Energy Levels of $LaF_3:Nd^{3+}$ (S.A. Johnson, H.G. Freie, ALS, and W.M. Yen), J. Opt. Soc. Am. 57, 734 (1967).

82. Spontaneous Emission from a Helium-Neon Laser as a Convenient Wavelength Standard (J.L. Rapier, H.H. Heimple, and ALS), Am. J. Phys. 35, 890 (1967).

83. Selective Laser Photocatalysis of Bromine Reactions (W.B. Tiffany, H.W. Moos, and ALS), Sci. 157, 40 (1967).

84. Piezospectroscopic Studies of Exchange-Coupled Cr^{3+} Ion Pairs in Ruby (L.F. Mollenauer and ALS), Phys. Rev. 168, 309 (1968).

85. Lasers and Coherent Light, Am. Sci. 55, 197 (1967).

86. Transverse Stimulated Emission in Liquids (J.L. Emmett and ALS), Phys. Rev. 170, 358 (1968).

87. Far Infrared Spectrum of $Al_2O_3:V^{4+}$ (J.Y. Wong, M.J. Berggren, and ALS), J. Chem. Phys. 49, 835 (1968).

88. Spectroscopic Studies of $SrTiO_3$ Using Impurity Ion Probes (S.E. Stokowski and ALS), Phys. Rev. 178, 457 (1969).

89. Laser Light, Sci. Am. 219, 120 (1968).

90. Dielectric-Related Optical Line Shifts in $SrTiO_3:Cr^{3+}$ (S.E. Stokowski and ALS), Phys. Rev. 178, 464 (1969).

91. Electric-Field Effects on the Spectrum of Chromium in Strontium Titanate (S.E. Stokowski and ALS), Phys. Rev. Letters 21, 965 (1968).

92. Lasers and Light, readings from Scientific American, with Introductions by A.L. Schawlow, W.H. Freeman and Company, Incorporated, San Francisco (1969).

93. Plasma Refractive Effects in HCN Lasers (B.W. McCaul and ALS), in Proceedings of the Second Conference on Lasers, Annals of the New York Academy of Sciences 168, 3 (1970), p. 697.

94. Lasers and Their Light, in Laser Photocoagulation and Retinal Angiography, by H.C. Zweng, H.L. Little, and R.B. Peabody, The C.V. Mosby Company, Saint Louis (1969), p. 3.

95. Design and Analysis of Flashlamp Systems for Pumping Organic Dye Lasers (J.F. Holzrichter and ALS), in Proceedings of the Second Conference on Lasers, Annals of the New York Academy of Sciences 168, 3 (1970), p. 703.

96. Is Your Research Moral? Physics Today 22, 118 (1969).

97. Lasers: The Old Dream and the New Reality, preface for book on Laser Applications, by W.V. Smith, Artech House, Incorporated (1970).

98. Polarized Fluorescence Study of Cr^{3+} Through a Stress-Induced Phase Transition in $SrTiO_3$ (T.S. Chang, J.F. Holzrichter, G.F. Imbusch, and ALS), Sol. St. Comm. 8, 1179 (1970).

99. Direct Observation of Single-Domain $SrTiO_3$ (T.S. Chang, J.F. Holzrichter, G.F. Imbusch, and ALS), Appl. Phys. Letters 17, 6 (1970).

100. Depolarized Light Scattering from Liquid Bromine (M.D. Levenson and ALS), Opt. Comm. 2, 4 (1970).

101. Cubic to Trigonal Stress Induced Phase Transition in $SrTiO_3$ (L.S. Wall, M. Rokni, and ALS), Sol. St. Comm. 9, 573 (1971).

102. Laser Action of Dyes in Gelatin (T.W. Hänsch, M. Pernier, and ALS), IEEE J. Quant. Electr. QE-7, 45 (1971).

103. Dispersion in C_6H_6 and C_6D_6 (M.S. Sorem and ALS), Phys. Letters 33A, 268 (1970).

104. Image Amplification by Dye Lasers (T.W. Hänsch, F. Varsanyi, and ALS), Appl. Phys. Letters 18, 108 (1971).

105. Complete Hyperfine Structure of a Molecular Iodine Line (T.W. Hänsch, M.D. Levenson, and ALS), Phys. Rev. Letters 26, 946 (1971).

106. Magnetization Induced by Optical Pumping in Antiferromagnetic MnF_2 (J.F. Holzrichter, R.M. Macfarlane, and ALS), Phys. Rev. Letters 26, 652 (1971).

107. An Image Orthicon Spectrograph with Computer Control (S.A. Johnson, W.M. Fairbank Jr., and ALS), Appl. Opt. 10, 2259 (1971).

108. High Resolution Saturation Spectroscopy of the Sodium D Lines with a Pulsed Tunable Dye Laser (T.W. Hänsch, I.S. Shahin and ALS), Phys. Rev. Letters 27, 707 (1971).

109. From Maser to Laser, in The Impact of Basic Research on Technology, B. Kursunoglu and A. Perlmutter, eds., Plenum Press (1973), p. 113.

110. Saturation Spectroscopy of Molecular Iondine Using the 5017A Argon Laser Line (M.S. Sorem, M.D. Levenson, and ALS), Phys. Letters 37A, 33 (1971).

111. Spectroscopy with Tunable Lasers in the Visible Region, in Proceedings of the Esfahan Symposium on Fundamental and Applied Laser Physics, Esfahan, Iran, August/September 1971.

112. Measurements of the Kinetic Energy of Free Positronium Formed in MgO (S. M. Curry and ALS), Phys. Letters 37A, 5 (1971).

113. Lasers: The Light Fantastic, in Physical Science Today, Communications Research Machines, Incorporated (1973), p. 523.

114. Optical Resolution of the Lamb Shift in Atomic Hydrogen by Laser Saturation Spectroscopy (T. W. Hänsch, I. S. Shahin, and ALS), Nature 235, 63(1972).

115. Hyperfine Interaction in Molecular Iodine (M. D. Levenson and ALS), Phys. Rev. A6, 10 (1972).

116. Saturation Spectroscopy in Molecular Iodine by Intermodulated Fluorescence (M. S. Sorem and ALS), Opt. Comm. 5, 148 (1972).

117. Ultrasensitive Response of a CW Dye Laser to Selective Extinction (T. W. Hänsch, ALS, P. E. Toschek), IEEE J. Quant. Electr. QE8, 802 (1972).

118. Nuclear Quadrupole Coupling of the $^1\Sigma^{+g}$ and $2\Pi^+_{Du}$ States of Molecular Iodine (M. S. Sorem, T. W. Hänsch, and ALS), Chem. Phys. Letters 17, 300(1972).

119. Simple Dye Laser Repetitively Pumped by a Xenon Ion Laser (T. W. Hänsch, ALS, and P. Toschek), IEEE J. Quant. Electr. QE-9, 553 (1973).

120. Lasers - Present and Future, in Proceedings of the Royal Institution, 1973, England, February 16, 1973.

121. Hyperfine Quantum Beats Observed in Cs Vapor under Pulsed Dye Laser Excitation (S. Haroche, J. A. Paisner, and ALS), Phys. Rev. Letters 30, 948 (1973).

122. Measuring the Diameter of a Hair by Diffraction (S. M. Curry and ALS), Am. J. Phys. 42, 412 (1974).

123. The Better To See..., in The Greatest Adventure, E. H. Kone and H. J. Jordan, eds., Rockefeller University Press (1974), p. 176.

124. Two-photon Spectroscopy of Na 3s - 4d Without Doppler Broadening using a CW Dye Laser (T. W. Hänsch, K. C. Harvey, G. Meisel, and ALS), Opt. Comm. 11, 50 (1974).

125. Lasers and Masers, in Encyclopedia Britannica (1974), p. 686.

126. Observation of Zeeman Quantum Beats in Molecular Iodine (R. Wallenstein, J. A. Paisner, and ALS), Phys. Rev. Letters 32, 1333 (1974).

127. Isotope Separation by Selective Unimolecular Photoisomerization (J. I. Brauman, T. J. O'Leary, and ALS), Opt. Comm. 12, 223 (1974).

128. Absolute Measurement of Very Low Sodium Vapor Densities Using Laser Resonance Fluorescence (W. M. Fairbank, Jr., T. W. Hänsch, and ALS), J. Opt. Soc. Am. 65, 199 (1975).

129. Excited State Absorption in Ruby, Emerld, and MgO: Cr^{3+} (W. M. Fairbank, Jr., G. K. Klauminzer, and ALS), Phys. Rev. 11, B60 (1975).

130. Cooling of Gases by Laser Radiation (T. W. Hänsch and ALS) Opt. Comm. 13, 68 (1975).

131. Measurement of the Stark Effect in Sodium by Two-Photon Spectroscopy (K. C. Harvey, R. T. Hawkins, G. Meisel, and ALS), Phys. Rev. Letters 34, 1073 (1975).

132. Masers and Lasers, IEEE Electr. Devices, ED-23, 773 (1976).

133. Identification of Absorption Lines by Modulated Lower-Level Population: Spectrum of Na_2 (M. E. Kaminsky, R. T. Hawkins, F. V. Kowalski, and ALS), Phys. Rev. Letters 36, 671 (1976).

134. Simplification of Spectra by Polarization Labeling (R. Teets, R. Feinberg, T. W. Hänsch, and ALS), Phys. Rev. Letters 37, 683 (1976).

135. Digital Wavemeter for C.W. Lasers (F. V. Kowalski, R. T. Hawkins, and ALS), J. Opt. Soc. Am. 66, 965 (1976).

136. Lasers, in Science Technology, and the Modern Navy, Thirtieth Anniversary 1946-1976, Office of Naval Research, Arlington, Virginia (1976).

137. Ground State Relaxation Measurements by Laser-Induced Depopulation (R. Feinberg, R.E. Teets, J. Rubbmark, and ALS), J. Chem. Phys. 66, 4330 (1977).

138. Stark Effect Study of Excited States in Sodium Using Two-Photon Spectroscopy (R.T. Hawkins, W.T.Hill, F.V.Kowalski, ALS, and S.Svanberg), Phys. Rev. A15, 967 (1977).

139. Lasers, Light and Matter, Frederic Ives Medal Address, J. Opt. Soc. Am. 67, 140 (1977).

140. Laser Interactions with Materials, in Proceedings of the LASER-77 Opto-Electronic Conference, June 21-24, 1977, Munich, Germany (in press).

141. Saturated-Interference Spectroscopy (F.V.Kowalski, W.T.Hill, and ALS), Optics Letters 2, 112 (1978).

142. An Improved Wavemeter for CW Lasers (F.V.Kowalski, R.E.Teets, W.Demtroder, and ALS), J. Opt. Soc. Am. 68, 1611 (1978).

143. Twenty Years of Laser Physics (ALS), in Proceedings of LASER-78 International Conference Mount Royal Hotel: London, 9-10 March (1978).

144. Laser Spectroscopy of Atoms and Molecules, Science 202, (1978), pp. 141-7.

145. The Spectrum of Atomic Hydrogen (T.W.Hänsch, G.W.Series, and ALS), Scientific American 240, No.3, 94(1979)

146. Polarization Labeling Spectroscopy of NO_2, (R.E.Teets, N.W.Carlson, and ALS), J. Molec. Spectro., 78, 415 (1979).

147. Doppler-Free Intermodulated Opto-Galvanic Spectroscopy (J.E.Lawler, A.I.Ferguson, J.E.M.Goldsmith, D.J.Jackson, and ALS), Physical Review Letters, 42, 1046 (1979).

148. Precision Interferometer Calibration Technique for Wavelength Measurements: Iodine Wavelengths at 633 nm and H (J.E.M.Goldsmith, E.W.Weber, F.V.Kowalski, and ALS), Appl. Opt. 18, 1983 (1979).

149. Identification of Excited States in Na_2 by Two-Step Polarization Labeling (N.W.Carlson, F.V.Kowalski, R.E.Teets, and ALS), Opt. Comm. 29, 302 (1979).

150. Lasers: The Practical and the Possible, Stanford Magazine 7, 24 (1979).

151. Doppler-Free Two-Photon Optogalvanic Spectroscopy (J.E.M.Goldsmith, A.I.Ferguson, J.E.Lawler, and ALS), Optics Letters 4, 230 (1979).

152. Doppler-Free Optogalvanic Spectroscopy (J.E.Lawler, A.I.Ferguson, J.E.M.Goldsmith, D.J.Jackson, and ALS), in Proceedings of the Fourth International Conference on Laser Spectroscopy, June 11-14, 1979, Garching, West Germany.

153. Some Methods of Laser Spectroscopy, in Proceedings of the International Conference on the Physics of Electronics and Atomic Collisions, IPEAC 1979, N.Oda and K.Takayanagi, eds., Aug. 29-Sept. 4, 1979, Kyoto, Japan.

154. The Laser Revolution, in Proceedings of the Fourth National Quantum Electronics Conference, Sept. 19-21, 1979, Edinburgh, Scotland.

155. Sensitive Intracavity Absorption At Reduced Pressures (W. T. Hill III, R.A. Abreu, T.W. Hänsch and A.L.S.), Optics Comm. 32, 96 (1980).

156. Superradiance Triggering Spectroscopy (N.W. Carlson, D.J. Jackson, A.L.S., M. Gross and S. Haroche), Optics Comm. 32, 350 (1980).

157. Identification of Rydberg States in Na_2, by Two-Step Polarization Labeling (N.W. Carlson, A.J. Taylor, and A.L.S.), Phys. Rev. Lett. 45, 18 (1980).

158. Polarization Intermodulated Excitation (POLINEX) Spectroscopy of Helium and Neon (T.W. Hänsch, D.R. Lyons, ALS, A. Siegel, A-Y. Wang, and G-Y. Yan, Opt. Comm. 37, 87 (1981).

159. Two-Step Polarization Labeling of Excited States of Na_2 (N.W. Carlson, A.J. Taylor, K.M. Jones, and ALS), Phys. Rev. A24, 822 (1981)

160. Doppler-Free Radiofrequency Optogalvanic Spectroscopy (D.R. Lyons, ALS, and G-Y. Yan), Opt. Comm. 38, 35 (1981).

161. A Study of the Excited $^1\Sigma^+_g$ States in Na_2 (A.J. Taylor, K.M. Jones, and ALS), Opt. Comm. 39, 47 (1981).

162. Two-Photon Line Shapes with Near-Resonant Enhancement (H-R. Xia, G-Y. Yan, and ALS), Opt. Comm. 39, 153 (1981).

163. Selective Spectrum Simplification by Laser Level Labeling (N.W. Carlson, K.M. Jones, G.P. Morgan, ALS, A.J. Taylor, H-R. Xia, and G-Y Yan), in Laser Spectroscopy V, Proceedings of the V Conference on Laser Spectroscopy, A.R. McKellar, T. Oka, and B.P. Stoicheff, eds., Springer-Verlag, New York (1981), p. 51.

164. Polarization Intermodulated Excitation (POLINEX) Spectroscopy of Excited Atoms (Ph. Dabkiewicz, T.W. Hänsch, D.R. Lyons, ALS, A. Siegel, Z-Y. Wang, and G-Y. Yan), in Laser Spectroscopy V, Proceedings of the V Conference on Laser Spectroscopy, A.R. McKellar, T. Oka, and B. P. Stoicheff, eds. Springer-Verlag, New York (1981), p. 178.

165. Spectral Structure of Continuous-Wave Two-Photon Transitions in Na (G.P. Morgan, H-R. Xia, and ALS), J. Opt. Soc. Am. 72, 315 (1982).

166. A Multiple-Wedge Wavemeter for Pulsed Lasers, (L.-S. Lee, and ALS), Optics Letters 6, 610 (1981).

167. Simplifying Spectra by Laser Level Labeling, in the Proceedings of the International Colloquium on Molecular Spectroscopy, Stockholm, Sweden, May 11-15, 1981, also Physica Sripta 25, 333 (1982).

168. Spectroscopy in a New Light, Nobel Prize Lecture, in Les Prix Nobel, Nobel Foundation, Stockholm, 1982,

168A Spectroscopy in a New Light, Reviews of Modern Physics 54, 687 (1982)

168B Spectroscopy in a New Light, Science 217, 9 (1982)

168C Fortechritte in der Laserspektroskopie, Naturwissenschaftliche Rundschau, 36, 247 (1983).

168D Spectroscopy in a New Light, Uspekhi Fizicheskii Nauk (USSR) 138, 205 (1982).

168E. Spectroscopy in a New Light, Postepy Fizyki (Poland) Tom 34, Zeszyt 4 (1983).

168F. Spectroscopy in a New Light, Czechoslovak Journal of Physics, Section A, Volume 33 (1983).

169. Ultraviolet Sum-Frequency Generation Utilizing Optical Pair Interactions in Solids (S.C.Rand, L.-S.Lee and ALS), Opt. Comm. 42, 179 (1982).

170. Spectroscopy: Present and Prospects, in New Techniques of Optical and Infrared Spectroscopy, The Royal Society, London (1982), p. 219.

170A. Spectroscopy: Present and Prospects, Phil. Trans. Roy. Soc. London, Series A, 307, 685 (1982).

171. Lasers and Physics: A Pretty Good Hint, Physics Today 35, 46 (1982).

172. Concluding Remarks, in Atomic Physics 8, I.Lindgren, A.Rosen, and S.Svanberg, eds, Plenum Press, New York (1983), p. 565.

173. A Scanning Pulsed Polarization Spectrometer Applied to Na_2, (A.J.Taylor, K.M.Jones, and ALS), J. Opt. Soc. Am. 73, 994 (1983).

174. Lasers and Their Uses (Charles H. Davis Lecture Series, Fall 1983), sponsored by Naval Studies Board, National Research Council, Academy Press, Washington (1983).

175. Advances in Laser Spectroscopy, Interdisciplinary Science Reviews, Volume 9, Number 1, 59 (1984).

176. Lasers in Historical Perspective, IEEE J. Quant. Electr., Centennial Issue (1984).

177. Cascade Stimulated Emission in the Sodium Dimer (Z-G. Wang, Y-C.Wang, G.P.Morgan, and ALS), Opt. Comm. 48, 398 (1984).

178. Cooperative Energy Transfer Among Pr^{3+} Ions in LaF_3 (L.-S.Lee, S.C.Rand, and ALS, Phys. Rev. B 29, 6901 1984).

179. Generation of Coherent UV Radiation by Optical Wave-Mixing Processes in Atomic Potassium, (P.-L.Zhang, Y.-C.Wang, and ALS), J. Opt. Soc. Am. B1, 9 (1984).

180. Laser Spectroscopy, Past, Present, and Perhaps Future, in Proceedings of the International Conference on Lasers '82, STS Press, McLean, Virginia (1983), p. 1.

181. Three-Photon-Excited Fluorescence Detection of Atomic Hydrogen in an Atmospheric-Pressure Flame (M.Alden, ALS, S.Svanberg, W.Wendt, and P.-L.Zhang), Optics Letters 9, 211 (1984).

182. Two-Photon Resonant Optical Processes in Atomic Potassium (P.-L.Zhang and ALS), Canadian J. of Physics, 62, 1187 (1984)

183. Laser Emission from Double-Minimum State $(2)^1\Sigma^+u$ to $(2)^1\Sigma^+_g$ by Optical Pumping in Sodium Dimer (Z.-G.Wang, H.-R.Xia, L.-S.Ma, Y.-Q.Lin, I.-S.Cheng, and ALS) in Proceedings of the International Conference on Lasers, LASERS '84, STS Press, McLean, Virginia (1985).

184. Comment of the Asymmetry Observed in Intracavity Absorption Line Profiles (W.T.Hill III, T.W.Hänsch, and ALS), accepted, Applied Optics, September, 1985.

185. Lasers and Mankind, Franklin Lectures in Science and Humanities, Auburn University, Alabama, May 6, 1985.

186. Laser Spectroscopy Using Beam Overlap Modulation (T.P.Duffey, D.Kammen, ALS, S.Svanberg, H.-R.Xia, G.-G.Xiao, and G.-Y.Yan), Optics Letters, 10, 597 (1985).

187. 25 Years of Lasers, Encyclopedia Britannica Yearbook of Science and the Future, 1985.

188. High-Contrast Doppler-Free Transmission Spectroscopy, (S.Svanberg, G.-Y.Yan, T.P.Duffey, and ALS), Optics Letters, 11, 138 (1985).

189. Two-Photon Resonances in a Sodium-Potassium Mixed Alkali Vapor (G.P.Morgan and ALS), J. Opt. Soc. Am. B, 1033 (1986).

190. Saturation Spectroscopy for Optically Thick atomic Samples, (S. Svanberg, G.-Y.Yan, T.P. Duffey, W.-M. Du, T.W. Hänsch and ALS), J. Opt. Soc. Am. B, 462 (1987).

191. Principles of Lasers, in Proceedings of Impacts of Physics on the Frontiers of Medicine I. Lasers, Lake Buena Vista, Florida, Dec. 8-10, 1986

192. Constants of the $5s^1\Sigma^+_g$ State of Na_2, From Two-Photon Spectroscopy, (G.-Y. Yan, T.P. Duffey, W.-M. Du, and ALS), J. Opt. Soc. Am. B4, 1829 (1987)

193. Intracavity Absorption Detection of Magnetic-dipole Transitions in $^{18}O_2$, and the determination of the $b^1\Sigma^+_g$ (v=2) State Rotational Constants, (W.T. Hill III and ALS), J. Opt. Soc. Am. B5, 745 (1988)

194. Atoms, Molecules and Light, in Modern Physics in America: A Michelson-Morley Centennial symposium, William Fickinger and Kenneth Kowalski, eds., American Institute of Physics AIP Conference Proceedings 169, American Institute of Physics, New York (1988), p.26

195. The Potential of the $(5)^1\Sigma^+_g$ State of Na_2, by Two-Step Excitation Spectroscopy, (G.-Y. Yan, B.W. Sterling, and ALS), J. Opt. Soc. Am. B5, 2305 (1988)

196. Doppler-free UV Excitation Spectra of $C^1{}_u$-$X^1\Sigma^+_g$ in Na_2, by Modulated Population Spectroscopy, (G.-Y. Yan, B.W. Sterling, T. Kalka and ALS), J. Opt. Soc. Am. B6, 1975 (1989)

197. Experimental Observation of the $(3)^1\Sigma_u^+$ State of Na_2, by Deperturbation of the $C^1{}_u$-$X^1\Sigma^+_g$ System, (G.-Y. Yan and ALS), J. Opt. Soc. Am. B6, 2309 (1989)

198. First Observation of Perturbations on the C^1_u State of Na_2, by CW UV Modulated Population Spectroscopy, (G.-Y. Yan, B.W. Sterling and ALS), Laser Spectroscopy IX, Proceedings of the Ninth International Conference on Laser Spectroscopy, M.S. Feld, J.E. Thomas, and A. Mooradian, eds., Academic Press, New York, pp 402-404 (1989)

199. Relaxation Oscillator Detection of Optogalvanic Spectra, (G.-Y. Yan, K.-I Fujii, and ALS), Optics Letters 15, 142 (1990)

200. A New Method for Detecting Optogalvanic Effect and Plasma Oscillation, (G.Y. Yan, K.-I. Fujii, and ALS), Proceedings of the Fourth International Conference on Laser Aided Plasma Diagnostics, Fukuoka, Japan, pp 415-420 (1989)

201. Discovering Science (ALS) in A Voyage of Discovery: Messages from Nobel Laureates, Israel Halperin, editor

201a. Discovering Science (ALS) in book for third world scientists, D.K. Nachtigall, editor

202. Sharp Optical Lines in Rare Earth Barium Copper Oxides (D.W. Shortt, M.L. Jones and ALS), Phys. Rev. B42,132 (1990)

203. Felix Bloch (ALS) in Yearbook of American Philosophical Society 1989, 174 (1990)

204. Detection of Sharp Absorption Lines in Very Thin Nd_3 Films, (D.W. Shortt, M.L. Jones, A.L. Schawlow, R.M. Macfarlane and R.F.C. Farrow), J. Opt. Soc. Am. B8, 923 (1991)

205. The Beginnings of Lasers, in Proceedings of Tropical Laser 90, International Laser Therapy Association, T. Ohshiro, Ed. (1990)

206. Optical Line Spectra in Metallic $(Nd,Ce)_2,CuO_{4-x}$, (M.L. Jones, D.W. Shortt, B.W. Sterling, ALS and R.M. Macfarlane), submitted to Physical Review B (1991)

207. Measurement of Diode Laser Characteristics Affecting Its Tunability with an External Grating, G.-Y. Yan and A.L. Schawlow, J. Opt. Soc. Am. B9, 2122 (1992)

208. Laser, Licht und Materie, Naturwissenschaftliche Rundschau 45, 211 (1992)

209. Ultrasensitive Absorption Spectroscopy of Lanthanide Solids, (M.L. Jones, D.W. Shortt, B.W. Sterling and A.L.S.), submitted for Festschrift for Stanley S. Hanna (1992)

210. Laser Technology for the Next Century, A.L. Schawlow in Proceedings of Laser Advanced Materials Processing, Nagaoka, Japan, June 8-12, 1992 A. Matsuoka, editor

211. Perspectives On Laser Spectroscopy, in Proceedings of Enrico Fermi Summer School, Varenna, Italy, June 29-July 10, 1992, Frontiers in Laser Spectroscopy, T.W. Hänsch and M. Ingiusco, editors, North Holland (1994) page 1.

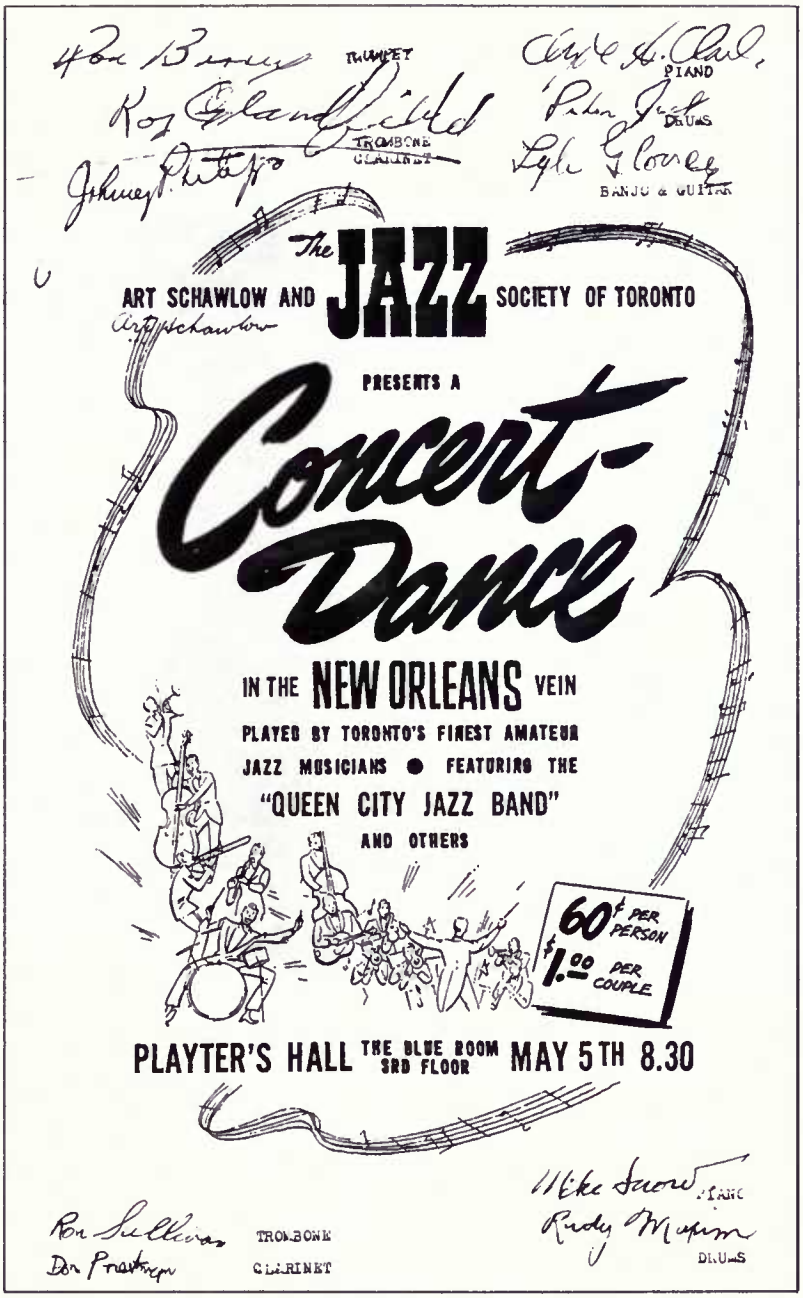
212. Absorption Spectroscopic Measurement of Atomic Density in Laser-Induced Vapor Plume, T.P. Duffey, T.J. McNeela, J. Mazumder, and A.L.S., Appl. Phys. Lett. 63, 2339 (1993)

213. Spectral Lineshapes for First-Surface Reflection from Solids, B.W. Sterling, A.L.S., and M. Jones, submitted to J.O.S.A. B, 1994

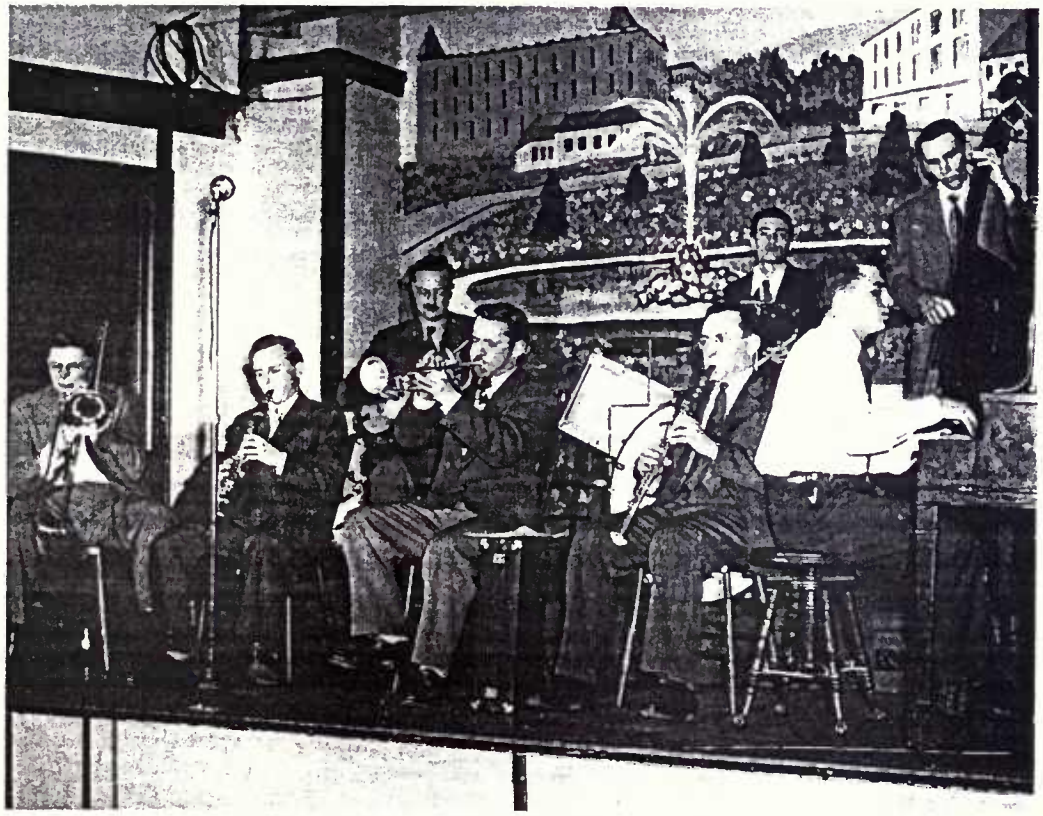
214. Lasers in Perspective, A.L. Schawlow in Proceedings of the 6th International Symposium on Advanced Nuclear Energy Research, Mito, Japan (Japan Atomic Energy Research Institute, 1995), page 3

215. Fifty Years of Physics and Physicists, A.L. Schawlow, Physics in Canada, 1995

216. Absorption spectroscopic measurements of plume density and temperature in production of nanocrystalline $NbAl_3$ by laser ablation deposition, T.P. Duffey, T.G. McNeela, T. Yamamoto, J. Mazumder and A.L. Schawlow, Phys. Rev. B, 14652 (1995)



Poster autographed by the musicians, May 5, 1948



Delta Jazz Band, Lansdowne Assembly Hall, December 2, 1948

Ron Sullivan, Johnny Mitchell, F.L. Priestly, Bob Donnelly
Art Schawlow, Jim Johnson, Barry Habberman, Ken Glandfield

DELTA JAZZ BAND
"The Jazz Band Ball"
Lansdowne Assembly Hall
Thursday, December 2, 1948
8:45 - 9:15 pm

PERSONNEL:

Bob Donnelly	-	Trumpet
Ron Sullivan	-	Trombone
Johnny Mitchell	-	Clarinet
Art Schawlow	-	Clarinet
Barry Habberman	-	Piano
F. L. Priestly	-	Banjo
Ken Glandfield	-	Bass
Jim Johnson	-	Drums

PROGRAM:

You've Gotta See Mama Ev'ry Night (vocal Donnelly)
Ja-Da
Tin Roof Blues
Darktown Strutters' Ball
Slow Blues
Just A Closer Walk With Thee

QUEEN CITY JAZZ BAND
"The Jazz Band Ball"
Lansdowne Assembly Hall
Thursday, December 2, 1948
9:15 - 10:15 pm

PERSONNEL:

Frank Mowat	-	Trumpet
Bud Hill	-	Trombone
Johnny Philips	-	Clarinet
Clyde Clark	-	Piano
Lyle Glover	-	Banjo
Harvey Hurlbut	-	Bass
Jack Beattie	-	Drums

PROGRAM:

Red Light Rag [theme]
Cut It Loose (My Bucket's Got A Hole In It)
Working Man Blues
I Thought I Heard Buddy Bolden Say
Dallas Blues
Squeeze Me
[1] Ballin' The Jack
[1] Muskrat Ramble
[1] Tin Roof Blues
Canal Street Blues
[1] Nobody Knows You When You're Down And Out
Red Light Rag [theme]

FOOTNOTE:
[1] This tune was recorded, and sold as a ten-inch acetate record, by Warner & Marrifield Recording Service, Toronto.

2. Live Appearances

My first introduction to the jazz fraternity in Toronto came on Thursday, March 6, 1947, when the Jazz Society of Toronto held a concert of recorded jazz, and invited the general public. They presented a well-balanced program of twenty-eight records covering the dixieland, New Orleans, blues, Chicago, and revival schools of jazz. Although I had been collecting records for five years, the variety of styles came as a revelation to me. I can still remember sitting on those wood chairs before the meeting began, reading the handbill, and wondering what NORK and OOJB meant!

The Queen City Jazz Band, led by pianist Clyde Clark, was the first amateur jazz band I ran across in Toronto. It had been playing for several years. In fact, at three private recording sessions in 1946, 1947, and 1948, they had recorded twelve sides which were issued as custom-made acetate records (2).

My first meeting with the band occurred at a dance presented by Art Schawlow and the Jazz Society of Toronto on May 5, 1948. It was, for me, merely the first of many dances in various halls around Toronto, including Centre Island (a resort area offshore in Toronto harbour).

A memory comes back to me of one of these sessions - a warm summer evening, a refreshing breeze blowing in from the lake, an open-air dance floor called The Lido Deck on the main street of Centre Island, and up on the bandstand the Queen City Jazz Band with all seven men playing their hearts out.

I didn't dance at these sessions - I just listened, spellbound, and wrote down the names of the musicians and the tune titles. When I returned home, I typed up the lists and filed them away. I did this for all the sessions described in this paper. So there's no faulty memory here; you can rely on the accuracy.

The first time the band was recorded at a dance was on July 21, 1948, when Art Schawlow and Wilf Goldstick set up a tape recorder and tried to record most of the music. After the dance we all drove over to Wilf's place to hear the results. Alas, the tapes were unusable. It's a pity; I remember that the closing number, Canal Street Blues, was a long version, with time for a solo by each member of the band and each guest who was sitting in. Note that Bud Hill played string bass that night. When I expressed my surprise to Bud, he said, "Heck, <u>any</u> trombone player can play bass."

In September of 1948 Ken Dean left the Queen City Jazz Band and formed his own band, Ken Dean's Hot Seven. Now there were <u>two</u> amateur bands to follow! For a teen-ager like me with a deepening appreciation of jazz, that wasn't at all hard to take. The dance at the Todmorden Memorial Hall on August 13, 1948, was the first appearance of the band.

In October, Bob Brimson played a dance at Coliton's Auto Livery with a sextet that I presume he put together for the occasion. He used four new members of the Queen City Jazz Band, plus Bud Hill's brother Ed on clarinet. The band played stock arrangements, interspersed with hot jazz tunes. The singer, Jean Nesbitt, was Bob Brimson's girl friend. When she sang her dreamy vocals, two young bucks from the audience stood in front of her and swayed from side to side. Bob was ready to punch them out, but Jean felt they were sincere, and was flattered. I was sitting on the floor beside Bud Hill at one point and the arrangement must have been boring, because Bud leaned over to me and offered to let me blow his trombone part. I told him I hadn't a clue how to play trombone, but I don't think Bud would have minded.

IMPACT OF BASIC RESEARCH ON TECHNOLOGY

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FROM MASER TO LASER

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In some ways, lasers seem to be the realization of one of mankind's oldest dreams of technological power. Starting with the burning glass, which was known to the ancient Greeks, it was natural to imagine an all-destroying ray of overpoweringly intense light. Francis Bacon, in his 1627 New Atlantis, imagined that the inhabitants of this utopia had "all multiplication of light, which we carry to great distance, and make so sharp, as to discern small points and lines." In War of the Worlds, H.G. Wells' 1898 novel, Martians nearly conquered the earth with a sword of light. In 1923, the Russian novelist Alexei Tolstoi wrote The Hyperboloid of Engineer Garin. Then, in the 1930's the Buck Rogers comic strip often made use of a disintegrator gun.

Yet many old dreams, which have more or less come true in this century, are realized only more or less. Men dreamed of flying like birds and now they do fly, but it is not at all like birds. Similarly, most lasers deliver far less than the destructive death rays of science fiction but their light has properties, such as monochromaticity and coherence, which go far beyond the old dreams.

Rays of any kind were far from the minds of Charles H. Townes and myself when, in 1957, we began to think seriously about the possibility of optical masers. Rather, we were thinking of what was already a classic problem in pure technology: to find something which

would act like a radio tube and generate shorter radio waves. "Daedalus" has pointed out in New Scientist (December 22, 1965) that there is a body of research which seeks to find ways to do things for their own sake. There may well be no immediate application in sight, but such pure technology "like pure science, often has to masquerade as the applied variety in order to get funds." Some problems in pure technology may appear as frivolous as "the development of a square gramophone record played with such a perfect quadrilateral-linear motion that corner effects are imperceptible." But others play a serious part in the development of technology. Even though their applications are not immediately foreseeable, they do parallel or extend lines of enquiry which have been fruitful in the past.

Throughout the twentieth century, scientists and engineers have sought to extend radio techniques to shorter wavelengths. As a boy in the 1930's I had read in the Radio Amateur's Handbook that after World War I, the amateurs "couldn't go up [in wavelength], but we could go down. What about those wavelengths below 200 meters? The engineering world said they were worthless --but then, they'd said that about 200 meters, too." After preliminary tests and "some months of careful preparation, two way amateur communication across the Atlantic finally became an actuality when Schnell, 1MO, and Reinarty, IXAM, worked for several hours with 8AB, Deloy in France, all three stations using a wavelength of about 110 meters." Still shorter waves, with lengths ranging from 10 to 80 meters, were found to make possible world-wide communications.

In the 1930's, amateurs and others found ways to use very high frequency waves whose lengths were shorter than about ten meters. These waves did not travel much more than line-of-sight-distances, but they were found to be suitable for reliable, broad-band broadcasting such as for television or stereo music. With inventions like klystrons and cavity magnetrons it became possible to explore the properties of waves of centimeter lengths. These waves were not suitable for broadcasting since they could be stopped by almost any obstacle. However, their short wavelength made them useful for high-definition radar and for relaying broadband communications.

From all this, it seemed overwhelmingly probable

if some way could be found to generate shorter wavelengths, there would be uses for them. Some of the uses would be obvious, like communications, but there was a good chance that the unforeseen uses would be even more exciting. There were, of course, ways of producing shorter electromagnetic waves from many kinds of hot bodies. Such sources, like the sun and electric lamps, could be quite bright, but they lacked several of the desirable properties of electronic oscillators. The output was always a rather broad band of frequencies. Since the excited atoms or molecules radiated spontaneously and independently of each other, their output did not have spatial coherence. Moreover in the infrared, and especially for the longer infrared wavelengths, spontaneous emission was relatively slow so that the power emitted was small.

One of the requirements for building an electronic oscillator to generate such short electromagnetic waves is the resonators to tune it. For microwaves, which have lengths ranging from millimeters to centimeters, tuning is usually achieved with some kind of cavity resonator whose dimensions are comparable to the wavelength. When the desired wavelengths are a small fraction of a millimeter, construction of cavity resonators becomes a very difficult task. But nature has provided us with many kinds of atoms and molecules with natural resonances throughout the infrared and optical wavelength regions. Even when I was an undergraduate student, in the late 1930's, it seemed to me that there ought to be some way to use these in amplifiers or generators of infrared waves. But I did not know enough quantum physics to even begin trying to find a way to do it. Very likely others had similar vague ideas and, indeed, the formal similarity between atomic absorptions and resonances of tuned circuits had long been recognized.

The connection between radio waves and atoms was again emphasized by the growth of radiofrequency and microwave spectroscopy in the years after World War II. I was then a graduate student at the University of Toronto, having interrupted my studies for war work teaching at the University and then microwave antenna engineering in a radar factory. At the University of Toronto, we did not have the facilities for the glamorous fields like nuclear physics and radiofrequency resonances. So, I was happy to work, under Professor Malcolm F. Crawford, on hyperfine structure in the

spectra of atoms. With another graduate student, Frederick M. Kelly, I constructed an atomic beam light source to give spectral lines sharp enough so that their hyperfine structure could be analyzed. Another graduate student, William M. Gray, constructed a spectrograph and a Fabry-Perot interferometer to use with our source. Thus I became highly familiar with this interferometer, consisting of two parallel, partially-transmitting mirrors facing each other. This instrument had been studied in undergraduate optics classes, but even though most of the work with the interferometer was done by the others in our group, I did learn more about it during our research. When I began to think of resonators for light waves a decade later it seemed natural to start with the Fabry-Perot structure of two mirrors facing each other.

In the postwar years, it seemed to me that the most exciting physics research was at Columbia University. I.I. Rabi was still active, and W. Lamb and P. Kusch had recently made discoveries which were immediately recognized as important and later brought them Nobel Prizes. I wrote to Rabi, and he suggested that I apply for a postdoctoral fellowship to work with Associate Professor C.H. Townes. This fellowship was provided by the Carbide and Carbon Chemicals Corporation, a division of Union Carbide, to support research on the applications of microwave spectroscopy to organic chemistry. I had neither knowledge of nor interest in organic chemistry, but microwave spectroscopy was an attractive new field. I must also confess that I had not heard of Charles Townes, although I soon found that he had recently published a number of discoveries. At any rate, I applied for and was awarded the fellowship.

After coming to Columbia University, I learned that although microwave spectroscopy can be used to determine the structure of organic molecules and for analysis, that was not the only reason for Carbide and Carbon Chemicals' sponsorship. As early as August, 1945, Dr. H.W. Schulz, a member of their research staff, had written a memorandum to propose a new type of catalysis "to employ electromagnetic radiation of a specific frequency to effect activation of reacting molecules by induced resonance." In this memorandum he stated that "The pertinent frequency range would cover the long and short wave radio bands as well as the infrared, visible and ultra-violet spectra. A literature search indicates that this principle has previously been employed only in

the case of photocatalysis." As his study proceeded, Dr. Schulz came to realize that resonance catalysis would need the tunability and power of radio generators, but at a shorter wavelength than was available from existing oscillators. After various alternatives were considered, it was decided to support long range research aimed in this general direction at a major university.

At Columbia University, there was a Radiation Laboratory group in the physics department, continuing a program from the wartime days on magnetrons to generate millimeter length radio waves. Also there was Townes, who had recently come from Bell Telephone Laboratories and was making pioneering studies of the interaction between microwaves and molecules. The laboratory was supported by a Joint Services contract from the U.S. Army, Navy and Air Force, with the general aim of exploring the microwave region of the spectrum and extending it to shorter wavelengths. Dr. Harold Zahl of the Army Signal Corps and Paul S. Johnson of the Air Force Office of Scientific Research were among those active in the sponsorship of this program. Captain Johnson also organized a millimeter wave study committee and asked Townes to be its chairman. As Townes has recounted, it was on the morning of a meeting of this committee that he conceived the idea of the maser.

Thus during my stay at Columbia there was widespread recognition that it was interesting to find better ways to generate wavelengths shorter than those produced by existing electronic devices. But nobody had a good idea of how to do it, and so Townes' group concentrated on exploring the structures of molecules and their interaction with microwave radiation. This turned out to be the right decision, for a detailed understanding of the ammonia molecules was just what Townes needed to invent the maser in the Spring of 1951. In this ammonia maser, a beam of ammonia molecules would pass through a suitable electric field which would accept those in excited states and reject the unexcited, absorbing molecules. The excited molecules could be stimulated to emit microwave radiation inside a cavity resonator, a metal box having dimensions comparable with the wavelength.

Townes told me about his idea in May or June of 1951, and it seemed promising. I would have liked to work on it, but my time at Columbia University was coming to an end and I had accepted a job in solid state physics

at Bell Telephone Laboratories.

My work took me quite far away from problems of generating electromagnetic radiation. I kept in touch with Townes, because we were writing a book on Microwave Spectroscopy and I spent nearly every Saturday at Columbia University. Thus I heard from time to time about the problems and progress of the work on the maser and was delighted when it first operated in 1954. At about that time, interest in masers began to pick up at Bell Telephone Laboratories and two years later, G. Feher, H.E.D. Scovil and H. Seidel built the first three-level solid state microwave maser following a proposal by Nicolaas Bloembergen of Harvard University. While the original ammonia maser had been primarily useful as a frequency standard or as a sensitive detector for studies of the ammonia molecules, the solid state maser was something that could actually be used for communications and radar. It had a broader band width and could be tuned by changing the strength of a magnetic field. Not long afterwards, C. Kikuchi of the University of Michigan showed that ruby was a good material for such masers. Joseph Geusic, who came to Bell Labs from Ohio State University about that time, where he had done his thesis with J.G. Daunt and had for the first time measured the mivrowave resonances in ruby, was one of those who became active in designing and perfecting ruby masers.

I did not participate in any of this except as a spectator, being busy with research on superconductivity and, for a time, nuclear quadrupole resonance. I also taught twice a three month course in solid state physics for the engineers in the program which Bell Laboratories had established for new engineers coming from college with Bachelor's or Master's degrees.

When parametric amplifiers were rediscovered by Harry Suhl, we thought perhaps this might somehow be a clue to producing shorter wavelengths and I spent a little time learning about them. I even built an audio-frequency parametric amplifier too. It may well have been the first one at Bell Labs since the work of Peterson some years earlier, which had by then been nearly forgotten and was not known to me.

By 1957, I was coming to think that the time was right for a serious investigation as to whether one could build some kind of an infrared maser. Naturally,

I was thinking primarily about the wavelength region just a little shorter than could be obtained by radio tubes. Townes had hoped initially that his ammonia maser would oscillate at a wavelength of a half millimeter, but in the final device the output was at one and a quarter centimeters wavelength, which is well within the region spanned by existing microwave tubes. I remember attending a conference on low temperature physics at the University of Wisconsin in August 1957 and chatting there with Michael Tinkham, who was then at the University of California and had been doing some far-infrared spectroscopy. This kind of spectroscopy was very difficult, because the existing light sources were extremely weak and so I suggested that it really ought to be possible to build some kind of a maser to produce a stronger source. Tinkham mentioned that iron in crystals had energy levels in a right wavelength region, but neither of us did anything more about it at that time.

A few weeks later, about October of 1957, Charles Townes visited Bell Labs and we had lunch. Townes had been consulting with the Laboratories for about a year, but his contacts were with the maser people and I had not had any serious discussions with him. He told me then that he was interested in trying to see whether an infrared or optical maser could be constructed, and he thought it might be possible to jump over the far infrared region and go to the near infrared or perhaps even visible portion of the spectrum. He had made some notes and said that he would give me a copy. We agreed that it might be worthwhile for us to collaborate on this study and so we began.

We both realized that the three-level and four-level pumping schemes, used in microwave masers, could be used with incoherent light as a pump if we could get enough power from the incoherent light. Indeed, Townes had envisioned optical pumping of masers as early as 1954, and had mentioned the method in his basic maser patent. Just as in the microwave maser the ammonia molecules are excited independently and enter the resonator quite individually, so we could excite individual atoms or molecules in any kind of a maser at random. The synchronization would be achieved by a wave stored in a resonator.

However, excited atoms lose their stored energy even if they are not stimulated. In solids at

radiofrequencies the energy is lost by transfer to the crystal vibrations where it becomes heat, but in the visible region spontaneous radiation may be more important. It was not at all obvious whether one could get enough excited atoms despite spontaneous emission, and the only way to answer this question seemed to be to study the properties of some fairly simple substances which might be calculable. Although solids and liquids are known to emit strongly, gases are simpler and better understood and the simplest gases are those consisting of individual atoms. Townes thought he saw a suitable system in thallium vapor and had described it in the notes he gave me.

The thallium atoms would be excited from the ground state (6p) to a higher one (either 6d or 8s) by ultraviolet light from a thallium lamp. Such lamps were in use in Kusch's laboratory at Columbia University for experiments on optical excitation of thallium atoms in an atomic beam resonance experiment. Townes had discussed with Gordon Gould, a student of Kusch's who was working on the atomic beam experiment, the properties of thallium lamps to find out how much power could be expected from them. Atoms excited to the 6d or 8s level would, according to Townes' scheme, rapidly radiate part of their stored energy and drop to the 7p level which would be the upper level for maser action. From there they could be stimulated to make transitions to a 7s level which would normally be empty.

After looking at this, I saw a flaw in it, in that the rate of spontaneous transition was greater out of the 7s to the 6p than from the 6d or 8s into the 7p. This means that the 7p state, which was to hold atoms to be stimulated, would empty faster than it filled. Laser action might not be impossible under those circumstances, but it would be difficult, and it pointed out a general problem with this sort of a cascade operation in atoms. It is rather usual, although there are exceptions, that the various excited states have progressively longer lifetimes as you go up except for the ground state whose lifetime is essentially infinite.

However, if Townes' thallium scheme was not immediately workable it did make an important point. It would be easier to do a theoretical analysis for transitions of a maser to emit radiation in or near the visible than it would be for the submillimeter region, where so little was known experimentally. It might ever

be that it would be actually easier to build one in the near-visible region because the spacings between energy levels in that region are large enough so that thermal excitations do not quench excited atoms as quickly as they do for levels with the smaller spacings corresponding to the far infrared. So, we searched for suitable energy levels and transitions in some atoms which might be excited to emit radiation in this portion of the spectrum.

In this quest, we had a good deal of information to guide us, although not all the questions we wanted had been asked. The energy levels of many atoms were tabulated in the volumes prepared by Charlotte Moore of the National Bureau of Standards. Some transition probabilities were given in the Landolt-Börnstein Tables, in a Table edited by L. Biermann. These would give us a start and would give references to more complete information in original papers.

How many excited atoms would we need? Townes had the maser equation which he modified by letting spontaneous emission loss replace the other kinds of losses which had been dominant for microwave masers. The equations are given in the paper which we published in 1958, but essentially what he did was to imagine light waves traveling in a box which could be thought of, for the derivation, as a rectangular box with reflecting walls. Light would be lost only at the walls, and, knowing that light waves travel at the velocity of light, one could easily calculate the average time between wall reflections. Then from that you could get the ratio of energy lost to energy stored for an electromagnetic wave in the box, provided you know the reflection loss each time the wave reaches a wall. The rate of stimulated emission of energy from the excited atoms depends on the intensity of the stored wave, as do the losses. One needs then to calculate how many excited atoms are needed to overcome the losses. Now the excited atoms will radiate in a short time which might range anywhere from billionths of a second to perhaps thousands of a second and must be replaced on the average once each lifetime. Thus we can calculate the number of excited atoms needed per second to just make up the losses in this resonator. If we had more than that we can increase the losses by opening the hole or making the walls partly reflecting so that we can take out some of the energy generated.

In the microwave regions, the strength of the

interaction of the molecules with the stored electromagnetic wave is usually measured by the dipole moment of the molecule. One can give an effective dipole moment for an optically excited atom, but it is more usual to use the quantity known as the oscillator strength, f, which is related to the dipole moment. This is the quantity most commonly tabulated in places such as the Landolt-Börnstein Tables and more extensive compilations which have appeared since then. The oscillator strength, f, indicates the effective number of electrons available for the particular atomic resonances and may range from one down to a very small fraction of one, or in a few exceptional cases it can be greater than one but not commonly. It can be measured, for example, if you know how many atoms there are in the ground state by determining the strength of the absorption of light within the band which the atom can absorb. Measurement for excited state is more difficult because it is not easy to know just how many atoms there are in each of the excited states but some measurements have been made.

Probability of stimulation by a given wave is proportional to oscillator strength, and so also is the gain for a given number of excited atoms. Thus to get a large gain without exciting very many atoms, we would wish to have a large oscillator strength. However, since the oscillator strength measures the interaction between the atom and an electromagnetic wave, the rate of spontaneous emission is also proportional to the oscillator strength. That is, the greater the oscillator strength the shorter the lifetime of the excited state and the faster we have to replace the excited atoms. It turned out, then, that it did not really matter what the oscillator strength was for the particular transition. If it was high, we would need only a few atoms but we would have to replace them frequently. If it was low, we would need many atoms, but would not have to replace them as often. Thus the oscillator strength would not matter at all, if atoms lost their excitation only by emitting the desired radiation. But if there are competing processes, it is helpful if the desired one has a large oscillator strength.

Another factor, important for the gain of a particular atomic resonance, is the width of the spectral line. The probability of stimulated emission, and hence the gain from a given number of excited atoms is inversely proportional to the width of the spectral

line. Fortunately, for a gas at low pressure, the linewidth is known to be given by the Doppler effect from the thermal motions of the atoms. This is easily calculable. In solids and liquids the linewidths are much more variable. When, later, we began to think seriously about these materials, we had to make our own measurements of linewidths.

We concentrated our study on the simplest atoms, the alkali metals. While the hydrogen atom's spectrum is perhaps even simpler and more theoretically calculable, hydrogen exists in the form of molecules which have to be disassociated and the efficiency of the dissociation would introduce additional uncertainty. The alkalis have only one electron outside a closed shell and so can be thought of as nearly one-electron atoms. Their energy levels are well known and the metals are not hard to vaporize. Moreover, alkali vapor lamps are commercially available by a number of companies and indeed sodium vapor has been widely used for street lighting. I chose to look most carefully at potassium for a rather trivial reason. Both the first and second members of the principal series of potassium vapor lie in the visible region. That is, one could pump potassium atoms from the ground 4s state up to the 6p with visible 4047Å light from a potassium vapor lamp and then monitor the progress of these atoms back to the ground state by looking at the red line emitted when atoms drop from the 5p to the 4s state. In the other alkalis, one or the other of these transitions lies in the infrared or ultraviolet. These are obviously not very important considerations, but I had essentially no optical equipment at all at the time and was thinking whether one could begin experiments easily and cheaply. Moreover, it did seem that any conclusions from one atom would be pretty much applicable to the others.

I bought some commercial Osram alkali vapor lamps and one of my colleagues, Robert J. Collins, measured the power output of some of these lamps for me. Collins had done his thesis research in spectroscopy and was by that time a Bell Laboratories physicist working on some infrared spectroscopic studies. He found that each of the lamps generated to 0.08 milliwatts in the 4047Å line. Of course this was only one small lamp and was not designed for maximum power. One could imagine buying large arrays of such lamps or, if necessary, building them. But the 0.08 milliwatts, if we could use all of it, would be sufficient to excite quite a large number

of atoms in a potassium vapor cell. So as our calculations progressed, with the aid of tables of measured oscillator strengths published years before, it began to look that you could indeed get enough excited atoms to obtain measurable amplification in the excited state.

During this time we had not been paying too much attention to the resonator which we would need to complete the maser oscillator. I had in mind from the beginning something like the Fabry-Perot interferometer I had used in my thesis studies. I realized, without ever having looked very carefully at the theory of this interferometer, that it was a sort of resonator in that it would transmit some wavelengths and reject others. Such an interferometer might typically have had mirrors with diameters of perhaps 7 cm and spacing of perhaps that much or less. Somehow it must have been implicit in our thinking that the absence of the side walls did not really matter too much. However, as we began to feel satisfied that it was possible to get sufficient excitation our attention turned more toward the properties of the resonator. The number of modes of oscillation of such a resonator, having dimensions tens of thousands of times larger than the wavelength, was enormous even in the limited range of frequencies which the atoms could amplify.

All physicists learn how to calculate the number of modes of waves in a large volume somewhere around the end of undergraduate or the beginning of graduate studies. This kind of calculation is important, for example, for estimating the spontaneous emission lifetime of excited atoms and for the derivation of the well known law for the intensity for emission from a heated black body. The same kind of counting up of modes is used in the Debye theory of specific heat, where the thermal motions of the atoms in a solid are considered to be entirely equivalent to a superposition of all possible random thermal waves of wavelengths from very long ones to those whose wavelength is just twice the spacing between the atoms. I have been through this as an undergraduate, but my memory had been particularly refreshed when I taught the Debye theory of specific heat to the engineers at Bell Telephone Laboratories. However, at first I simply looked at the number of modes and then began to think what the output of the optical maser might be like if we had one.

Martin Peter, another colleague at Bell Laboratories,

was particularly insistent that we should find some way to reduce this enormous number of possible modes. Otherwise, he felt the optical maser, if it did oscillate, would jump rapidly from one mode to the other and not produce any very recognizable kind of oscillations. The coherence of the radiation would be continually interrupted by jumps from one mode to a different one. Townes had recognized the importance of this multimode problem, and it had kept him for a long time from proceeding with short-wave masers. When we began our work together, he believed both that it was important to damp out other modes and assure that there was good mode control, but that even though he could see no system which would do this completely, one should go ahead in any case, thinning out the modes as much as ideas would permit. He expected that the oscillator would oscillate momentarily on a single or a limited set of modes because of nonlinearities, but that it would also jump fairly rapidly between different modes. He believed that one could easily determine that the system had gone unstable and was working, and that the propertie even with a complex set of modes would be recognizable and interesting. Very possibly, if Townes and Peter had discussed the question directly, they would have reached some sort of agreement. None of us doubted that some good method of mode selection was highly desirable.

I began to think of these modes in terms of the waves of the Debye picture, that is waves traveling in different directions inside the resonator and having different wavelengths. The range of wavelengths was limited by the bandwidth of the amplifying atoms. Now, to reduce the number of directions that would be acceptable in the instrument was not so easy. I thought for awhile that perhaps there might be certain directions in which the light could come out of the box, as there is in a Fabry-Perot resonator, and that the output might be an array of beams like the Laue spots of the x-ray diffraction camera. I thought at one time of replacing the walls of the box by diffraction gratings ruled so that they would only reflect light well for a particular angle of incidence and that only waves coming in this right direction would be properly reflected.

Having advanced this far, around the beginning of February 1958 I wrote down my ideas about optical masers in my notebook. Of course many very wise scientists will tell you that any scientist worth his salt carefully records all observations, calculations and concepts

in his laboratory notebook. However, I fear that I do not qualify, because in seven years at Bell Telephone I had not yet filled one notebook. Indeed my experience had been that the only valuable calculations and data were those that I took on scraps of paper. Whenever I thought I had things in sufficiently good order to record them in the notebook it turned out that I had overlooked something and that that particular work was essentially worthless. However, I did write down a number of pages of thoughts about optical masers. They included some calculations on potassium and the reordering of the equations and some of the ideas about possible structures, even though I was not at all confident that the problem of mode selection was solved. Even though I had never tried to patent anything, I asked Solomon L. Miller to read and witness these notes, on January 29, 1958. Miller had been one of Townes' graduate students when I was at Columbia University and had a laboratory near mine at Bell Telephone Laboratories. He was certainly well able to understand the discussion in my notes and so indicated when he signed them. I was a bit startled, just a few days later, to learn that Miller had left Bell Labs to go to IBM. Perhaps that had something to do with my never writing any more ideas in my notebook.

But indeed I did get a good idea very soon after writing these notes. I realized that if we took literally the Debye picture in which the various modes were waves having different lengths going in different directions, it could suggest a way to select one, or at most a few of these modes. If a wave started from somewhere near one wall of the resonator, it would reach a different place on the far wall, depending on its direction. If, therefore, most of the far wall were eliminated so that only a small patch remained, the wave would only be reflected if it were going in the right direction to reach that small patch of wall.

Thus we could reduce the large box to just two small mirrors facing each other at the ends of a long column of excited atoms. This arrangement would serve as a good resonator for waves which travel nearly straight along the axis joining the mirrors. A wave with any other direction would soon move sideways enough to miss the small end mirror, and thus would be lost.

It was clear to me then that this resonator could not hold any wave unless its direction of propagation

was inclined to the axis by less than the angle subtended by one mirror at the position of the other. Townes pointed out that this structure would be considerably more selective than that. Waves were expected to bounce many times back and forth through the amplifying medium. Only a wave traveling quite exactly along the axis would remain in the amplifying medium long enough to attain a high intensity by stimulating emission.

This simple reasoning convinced us that we had found a structure which would really strongly favor the growth of a few selected modes. It was also apparent that the output through one of the partially-reflecting mirrors would be a highly directed beam, more or less approximating a plane wave. We were also satisfied that we knew of at least one substance in which we would be able to excite enough atoms for optical maser action. However, it would take an uncertain time to build one, and unexpected experimental problems might well be encountered. We were aware that, during the three years which it took to construct the ammonia maser, some of the ideas had been discovered and published by others. There were many more workers in the field by 1958. Although we were not aware of any direct competition and did not particularly try to hurry, it seemed best to publish our conclusions without waiting for experimental verification. During the spring months we worked mostly on writing the manuscript.

Before submitting the paper for publication, we were required to circulate the manuscript to our colleagues at Bell Telephone Laboratories for technical comments and to the patent department to see if it involved a patentable invention. Several people, particularly some of those most expert in microwave waveguide theory, were skeptical of the reality of our modes and the proposed method of mode selection. They wanted to see a more complete calculation with rather precisely defined boundary conditions, which was done only later, in 1960, by A.G. Fox and T. Li. We did, however, add some paragraphs to our paper, in the hope of making the mode frequency and selection argument more complete and clear. There was some worry that there might be some modes of the resonator with longitudinal field components, as are found in microwave resonators. However, in a resonator like ours, the wave travels many thousands of wavelengths from one mirror to the other, and must behave much like a wave in free space, and so must be largely a transverse wave.

The patent department was, at first, quite uninterested in the idea. I suppose that it appeared to be remote from the needs of the telephone industry and perhaps they did not believe it would work or that if it did it would be very useful. However, largely at Townes' insistence they did prepare and file an application for a patent. It was issued rather speedily, in March, 1960. Our paper was submitted for publication, in August of 1958, and was published in the Physical Review in the December 15 issue of that year.

The paper did arouse a considerable amount of interest and a number of laboratories began searching for possible materials and methods for optical masers. Townes, in his own group at Columbia, began efforts to construct a potassium optical maser, working particularly through two graduate students Herman Z. Cummins and later Isaac Abella. They were joined for a time by Dr. Oliver S. Heavens who is now Professor of Physics at York University in York, England and who was even then a world renowned expert on highly reflecting mirrors.

We were of course aware of other possible materials for optical masers. One of these was cesium vapor. Cesium had the additional advantage that it could be pumped by a strong spectral line from a helium lamp, which happened to coincide with one of the cesium atom's absorption wavelengths. This coincidence had been noted in 1930 by C. Boeckner and mentioned in A.C.G. Mitchell and M.W. Zemansky's book Resonance Radiation and Excited Atoms (Cambridge University Press, 1934). We noted in our paper that a cesium infrared maser could thus be pumped by a helium lamp. This kind of a laser was constructed and successfully operated by S. Jacobs, G. Gould, and P. Rabinowitz in 1961. Thus by 1958 we knew a number of gases suitable for optical maser action, although we could not be sure which would be easiest.

Being at Bell Laboratories, I had been pretty thoroughly indoctrinated to believe that anything that you can do in a gas can be done in a solid and can be done better in a solid. I therefore began to explore the possibility of solid optical maser materials. Albert Clogston, who was my immediate boss at Bell Laboratories, had encouraged my interest in optical masers and now encouraged me to, if I wished, drop superconductivity entirely and begin studies of possible optical maser materials. On the other hand, nobody ever

suggested that we try and organize a group to build an optical maser. Anything I did I would have to do myself. There was a nearly invariable custom in the physical research department that each man was to be an individual scientist, and not an assistant to anyone else.

About the optical properties of solids, indeed my ignorance was quite total. However, even before our paper was published I began to learn a little bit. One thing that impressed me was that some materials such as ruby had broad absorption bands and narrow emission lines. Thus we were able to say in our 1958 paper that "The problem of populating the upper state does not have as obvious a solution in the solid case as in the gas. Lamps do not exist which give just the right radiation for pumping. However, there may be even more elegant solutions. Thus it may be feasible to pump to a state above one which is metastable. Atoms will then decay to the metastable state (possibly by nonradiative processes involving the crystal lattice) and accumulate until there are enough for maser action. This kind of accumulation is most likely to occur when there is a substantial empty gap below the excited level."

When writing that, ruby seemed like a tantalizing possibility because it did glow so brightly almost no matter how you excited it. Several people about that time had become interested in the optical emission from ruby including Saturo Sugano and Y. Tanabe and their associates in Japan, Irwin Wieder at the Westinghouse Research Laboratories, and Stanley Geschwind at Bell Laboratories. But ruby seemed also to present a very serious difficulty. The transition for the only strong fluorescence lines were absorbed by unexcited atoms in the same material. That is, they were resonance lines and so the atoms could both absorb and emit the same wavelength. Thus one would start out with the disadvantage that initially all the atoms would be absorbing, so that half of them would have to be excited before any amplification at all could be obtained. Without doing any calculations this really seemed like too much of an obstacle to overcome, although there might be some way because of the broad absorption bands for pumping. Several people made measurements on the fluorescence efficiency and strangely enough they all gave estimates ranging from 1 to 10 percent. I now think they were all trying to be conservative, but if we had done a calculation and believed their figures we

would have confirmed our prejudice that it was not possible to obtain optical maser action in the resonance lines of ruby. But I thought that perhaps there might yet be some way, such as by splitting the energy levels in a large magnetic field so that one sublevel at least would be empty at low temperatures.

How was it that so many people at nearly the same time began to study the optical properties of ruby? Well, the one reason was that there had been an advance in the understanding of crystals related to paramagnetic resonance studies which could carry over into the optical spectra. The dominant influence in my case and perhaps in some others, was that ruby was being used in microwave masers. It was possible to visit Joe Geusic or others at Bell Labs and find a drawer full of rubies from which you could easily borrow samples. So much was my practice to borrow samples that I remember a year or so later that George Devlin marked on one of the spectra "ruby ALS bought!," because up to then I had been mostly borrowing.

One advantage of all this interest in ruby was that crystals had been ordered and were available and with various concentrations of chromium in aluminum oxide. Although I did not think that ruby was going to be any use for an optical maser material, it was interesting because it seemed to have a fairly simple spectrum, which according to the theory should have had just two emission lines at low temperature along with some bands. Indeed at the very lowest temperatures it should have only one emission line. In actual fact, as had been shown many years ago by a number of experiments and most thoroughly by Otto Deutschbein in 1932 and by S.F. Jacobs and G.H. Dieke in 1956, there were very many lines in the spectrum of ruby. I thought naively that these extra lines might be due to the interaction of the chromium ion with the crystal lattice vibrations and that if we studied them it would give us some information about the crystals. It also seemed interesting to look at the chromium spectrum not only in aluminum oxide but in the related gallium oxide which Joe Remeika could grow as small crystals with various concentrations.

I had at that time a Gaertner wavelength spectrometer, a very simple student-type instrument which I had bought when I first went to Bell Labs thinking that it could be used for measuring thickness of thin metal films. Darwin Wood was in charge of spectrochemical

analysis and he could make available some time on his spectrographs and indeed collaborated with us on some of our early studies.

I have mentioned George Devlin, who was my technician during most of my years at Bell Labs. I had hired him even though he had very little formal training and really a rather poor high school education. He had, however, been a champion model airplane builder and it was evident that he had an attractive personality and a quick mind. It turned out to be a very rewarding association. For Devlin, although he was almost completely nonmathematical, had a real physical insight as well as skill in designing, building and operating equipment. Perhaps one of his most valuable characteristics, was that he did not have any great preconceptions as to what the data should be. Several times, he pointed out to me small effects which I would have dismissed as noise but which he insisted were real and turned out to be interesting. One of these was that when we were looking at various samples of gallium oxide crystals with various concentrations of chromium, he noticed that the satellite lines, that is the extra lines to the red of the strong R-line, were different in different samples. That was all I needed and I immediately jumped to the conclusion that these lines were not due to the crystal vibrations but rather to pairs of chromium ions. The probability of a given ion having a near neighbor in a particular crystal ion position was going to be proportional to the concentration of the ions in the crystal. At low concentrations such close neighbors would be very unusual and at high concentrations they would be common. This of course would also be the explanation for the extra lines in ruby. Darwin Wood and I investigated this point more quantitatively, collaborating with Albert Clogston on the theoretical aspects, and we published a note on the pair spectra in the summer of 1959.

However, one of the most interesting features of this pair spectrum was that it could produce a large splitting in the ground state of the chromium ions which the crystal field alone could not do. Thus instead of having a single or a very narrowly split ground state the splitting could be large enough that at low temperatures some of the higher levels of the ground state would be empty. This was what we thought we needed for an optical maser. We really felt that if we were to get optical maser action we had to give ourselves

every advantage and that is why we had not seriously considered the theoretical possibility that we could empty out the ground state by pumping. I presented this dark red ruby as a possible laser material, which would oscillate in the satellite line at 7009 or 7041Å or perhaps both, at the First International Quantum Electronics Conference in September of 1959. In emphasizing the advantages of the broad pumping bands in ruby and the four-level system made possible by the exchange coupling in dark ruby, I said briefly that the R lines were not suitable for laser action. Of course, it turned out later that both sets of lines can be made to lase.

The proceedings of that conference, including my talk, were published very quickly and issued in February of 1960, by the Columbia University Press. This is one of the very few occasions when I remember exactly when I wrote a paper. I had promised a manuscript by the time of the meeting, but as so often happens other things had prevented it. Therefore, I stayed at home for the first two days of the three-day meeting, wrote the paper and then came and delivered my talk on the third day.

In that particular paper, I also described rather concretely the structure of an optical maser in the following words -- "The structure of a solid-state maser could be especially simple. In essence, it would be just a rod with one end totally reflecting and the other end nearly so. The sides would be left clear to admit pumping radiation."

Well, if we knew the material and the structure, why not do it? Now, when we know that construction of a laser can be so easy, it is hard to give a convincing answer to that question. But when we did not know how to assess the difficulties and since it had never been done, we believed that they might be formidable. For example, no solid, and especially not dark ruby, is free from variation of refractive index caused by strain. Thus if a plane wave started out at one mirror and was amplified as it passed down the rod, it would be distorted beyond recognition before it could reach the other end and would not return nicely to the first mirror. Nevertheless, I did manage to find from Bill Mims, who was working on microwave masers, a rod of dark ruby. And as early as December 1958 I had the ends polished flat and parallel. I still have the order to

have this done at Laboratory Optical Company. However, I did not acquire flashlamps and merely tried this half heartedly with a General Radio Strobotac which I had bought for measuring fluorescence lifetimes. This was not enough power and nothing happened. One other reason I did not push more agressively on it was that I was not sure just how cold the crystal would have to be to empty the lower state of the optical transition. And besides, there were too.many interesting things to do in studying the spectrum.

But others were active. At Bell Labs, Ali Javan had conceived the idea of a helium-neon maser making use of transfer of energy from metastable helium atoms produced in a gas discharge to particular excited levels of neon atoms. Actually, I had heard the idea of using a gas discharge even before we wrote our first paper. Willard S. Boyle at Bell Labs had mentioned this possibility but he did not explore it seriously. He in fact described it in the context that processes in a semiconductor were similar to those in a gas discharge, and that it would be interesting to try and find a semiconductor analog of a gas discharge and that might produce population inversion which would permit optical maser action. Indeed Boyle has a patent on semiconductor lasers. We did not mention this idea in our paper although both the gas discharge and semiconductor possibilities seemed real, because these were Boyle's ideas and not ours and it was up to him to publish them, but he did not do it.

Ali Javan, however, did work out in some detail the properties of a particular system and he published a theoretical analysis in 1959. John Sanders from Oxford who visited Bell Labs in 1959 for eight months or so had another proposal using pure helium. People objected to Sanders' scheme in that the lower level would not have a short lifetime because of trapping of the radiation which was supposed to empty it, but there were objections in nearly everything anyone proposed and Sanders did spend some time trying it out. However, he had to return to England before any real conclusions were reached.

Javan, whom I had known as a student with Charles Townes at Columbia and later as a postdoctoral worker, was and is an extremely ingenious and able scientist. His enthusiasm attracted others, most particularly Donald R. Herriott and he was able to arrange for the Laboratory to hire William R. Bennett, Jr. They made

detailed studies of the processes in the gas discharges and Herriott developed optical components of great precision and quality for a helium-neon gas discharge optical maser. This all took considerable time, and indeed the research management at Bell Labs became concerned whether this was all a waste of a rather considerable amount of money or whether there was indeed some hope in it. I remember people went around and asked various opinions, but since these opinions were quite uniformly optimistic, the research was continued. But Javan and everyone else believed that the conditions for maser action in gases might have to be quite special as there are many processes tending to restore thermal equilibrium. I don't think anyone realized, as we now know to be true, that nearly any gas will lase if excitation is violent enough. Indeed no one even considered looking at pulsed gas discharges until considerably later. Perhaps that was because of the preoccupation with communications around Bell Labs, for which a continuous laser seemed the only really interesting one. Indeed that was another obstacle which detracted from my pursuing pulsed laser operation in ruby.

Around the same time, C.G.B. Garrett and Wolfgang Kaiser, both of whom had worked previously on semiconductors, became interested in trying to develop a solid state optical maser. They were, very reasonably, attracted to the rare earth compounds and transparent crystals which, as we had noted in our paper, give strong, sharp fluorescent lines. With these materials, four level systems with any empty lower state would not be hard to find, but they do not have the broad pumping bands which make ruby so attractive. Any incoherent lamp used to pump them is likely to be largely wasted because only small portions of its output spectrum can be absorbed by the rare earth ions.

At TRG, Inc. Gordon Gould and other associates including Richard T. Daly had an Air Force contract to work on optical masers which was, at least in part, classified. They invited me to visit and give a talk in the spring of 1960, and we exchanged ideas about work on spectroscopy of rare earth ions of the sort that might be useful for optical masers. However, everything we discussed presented formidable problems and an operating laser did not seem close.

Sometime early in 1960, we heard from one of Bell Labs management that Hughes Aircraft Research Laboratory

in Malibu was also working on optical masers. This point did not register very sharply, because I did not know the people there at all well and did not believe that they had anyone with any optical experience. Nor did I know about the article which was published by Harold Lyons in the Hughes news magazine in which the use of a ruby as a three level maser material was proposed. T.H. Maiman's name was familiar from work he had done on ruby microwave masers, including a paper at the Quantum Electronics Conference.

I spent the spring semester of 1960 at Columbia University as a visiting associate professor. I had been asked to do this because Townes was away at the Institute for Defense Analysis and it seemed good to have someone knowledgeable around in case his students had difficulties between his weekly visits, and also to teach courses. It was not possible for me to move to New York and so I commuted there daily, spending half a day or so at Bell Labs every week. This was an exhausting ordeal and I ended up the semester quite ill with a succession of colds and an infection which took weeks to clear away. But during that spring, I received a paper from Physical Review Letters to referee. In it, T.H. Maiman described some experiments on excitation of ruby by a bright light or flash and made quantitative measurements on the fraction of atoms excited. Maiman's paper indicated that some percent of the atoms could be excited, although it was not possible to tell from the text whether he felt optimistic about being able to produce about ten times greater excitation needed to get more in the excited than in the ground state. I think I suspected that he was interested in optically pumped microwave masers. This manuscript was published in the May 15, 1960 issue of Physical Review Letters.

Late in June, there was a conference on Coherence in Optics at the University of Rochester and some of the Bell Labs people attended. They heard there, from Malcolm Stitch, that Maiman had succeeded in operating a ruby laser. This caused both excitement and puzzlement. Then, in early July there was a press conference at which Hughes announced some information about Maiman's attainment of stimulated emission in ruby. Accompanying the press announcement was a photograph showing Maiman with a rod, very much as I had described, inside a flashlamp. By that time, several people at Bell Labs were working with flashlamps, and as there were not many such lamps on the market, it was easily recognizable as

a General Electric FT-524. Preprints of Maiman's manuscript for publication were sent out to trade magazines, and we obtained a copy from a magazine writer who came to Bell Labs to get our reactions and find out what Bell Labs was doing. There was some skepticism, but it seemed quite convincing to me. So, many of the people who were trying to produce various kinds of solid state optical masers, began to try to obtain laser action in ruby.

Among them were Garrett and Kaiser, with some help from Walter Bond who was developing techniques for polishing and coating the crystals, and also D.F. Nelson and R.J. Collins. These were in different departments of Bell Labs and physically separated by a considerable distance in the two different buildings of the Laboratory. A few weeks later, I heard from TRG that they had gotten their ruby laser to operate the night before. And so I went to ask Collins and Nelson about their progress. They felt that they needed better diagnostic equipment than they had, in particular a better spectrograph to tell what was happening. I had acquired a good spectrograph for my research and so I joined their effort and they moved their equipment down to my spectrograph. Within a day or two they also had achieved laser action. I remember they were using a General Electric FT-524 lamp which was rated at a maximum input of 4000 volts from a 400 microfared capacitor. At this rated output, the optical maser threshold was not achieved so they raised in input voltage to 4200 volts, 200 volts above the manufacturer's rating, and their success was attained. It does not pay to be gentle when you have a threshold effect! Lasers are nonequilibrium devices and you sometimes have to be fairly violent to get sufficiently far away from equilibrium.

We did not start out by seeing the beam, but by looking at an oscillograph showing the output of a photomultiplier which received the light filtered through the spectrograph. When laser action was achieved, there was a brief burst of much more intense light than the fluorescence which was always emitted whenever you excited the ruby crystal at all. The experiment was of course quite improvised in many respects and the bright light from the flashlamp lit up the whole room and made it very difficult to see what was going on other than what the instruments told us. In fact, we were not at all sure that the beam could be seen if there were a beam. The light from the ruby is at a

wavelength so long that the eye is two hundred times less sensitive than in the green and the pulse lasts only about one two-thousandth of a second.

But the first thing we could do was to find out whether the ruby optical maser had any of the predicted properties. One of these was that the light output should be more monochromatic, that is, should be confined at a narrower band of wavelengths than that of spontaneous emission. It should be directional and it should be coherent, according to the theory of Townes and myself. We set out to try and check these points. The frequency spread could only be measured by flashing the laser repeatedly, photographing the output on an oscilloscope and changing the wavelength setting of the spectrometer between successive flashes. This was something of a problem, because the metal coatings on the ends of the ruby rod, which were usually gold, did not last long at the high intensities of the experiment.

Directionality, which we now can see very easily by just looking at the beam, seemed hard to investigate because we did not know whether we could see the beam. The obvious way to do this was to put a camera focussed for infinity where it could receive the laser output and see whether the laser produced a small spot. A red filter would have to be placed over the camera to screen out the white light from the flashlamp. This was one of the things we were going to get around to but it finally began to be such an obsession with me that after one sleepless night I came in prepared to tell everyone that I was ready to fight to do this experiment next. At this point everyone agreed without a fight, and we found that the output was indeed focussed to a small spot indicating a beam divergence of about a hundredth of a radian, or a half a degree.

Now Maiman had indicated in his paper that he would not expect to get directionality because of reflection from the side surfaces of the crystal. So we deliberately left the sides of our crystal rough in order to minimize reflection from the side walls. Thus I really did expect to get a beam and was gratified when it was obtained. It was a bit crushing a few days later, however, when Garrett, Kaiser and Bond also observed a beam just as good as ours while the sides of their crystal were polished. It was some months later before we realized that the pump light is actually focussed within the crystal so that the degree of excitation is

higher on the axis than it is near the walls. This means that while amplification is being obtained near the center of the crystal the outer parts are still absorbing and so reflections from the side walls are prevented.

In our photographs, we noticed that the spot had a grainy substructure and so we found that the output came in filaments. This helped to explain why the beam was not even more perfectly directional than it was, as some diffraction spreading is inevitable from these small individual filaments. It also showed how laser action could occur in solid substances that were known to be highly imperfect. Indeed, study of the line-widths showed the ruby crystal, which we used for our earliest laser experiments, to be the most badly strained one I have ever encountered. But somehow there happened to be some small paths over which light could find its way from one end mirror to the other and if the gain was high enough to overcome the diffraction losses in such a thin column, lasing could occur.

Not very long afterward, Garrett and Kaiser took the trouble to box in the laser so that the stray light from the flashlamp could not reach the eye and light could only come out through a hole at the end of the ruby rod. They found, and everyone was excited to realize, that the beam indeed could be very easily seen as a bright red spot where it struck the wall. Maiman also apparently observed the directionality about the same time, for he submitted an abstract to the meeting of the Optical Society of America and his paper was presented at their meeting in October 1960.

One day, George Devlin asked "Is there any sign of hysteresis?" This would not be surprising because many oscillators do tend to show an overshoot when oscillation starts or stops, and so we looked more carefully at the oscilloscope trace. Fortunately, we had acquired a Tektronics 555 dual beam oscilloscope in which one beam gives a magnified picture of a small portion of the trace displayed by the other beam. That is, we could delay the second beam sweep so that it covered an interval of one hundred microseconds stretching between five hundred and six hundred microseconds after the initiation of the flash. When we did, we could see that the output was indeed quite spiky and instead of being a burst of about five hundred microseconds it was in fact a series randomly spaced, very intense, one-microsecond pulses.

To prove the coherence was somewhat more difficult experimentally. We knew that if the light coming out of the laser rod was spatially coherent, we could tell it by doing a Young's two-slit diffraction experiment with the two slits being right in the plane of the laser end mirror. Alternatively, we could have one wide single aperture and observe diffraction maxima and minima in the light coming out through this small opening. The troubles were purely the experimental ones of laying down the suitable patterns on the mirror and then having the mirrors hold together long enough to do the experiment. We had not yet learned how to make dielectric mirrors suitable for ruby laser operations. The single aperture experiment was done first and the results showed definite coherence across the width of the slit which was about 50 micrometers by 150 micrometers.

Between the two groups at Bell Telephone Laboratories we had amassed good experimental proof of the predicted properties of an optical maser. We therefore decided to submit a Letter for publication to Physical Review Letters and we set a cut-off date beyond which we would stop experimenting and finish the manuscript. The single slit diffraction pattern was obtained before the cut-off date but the double-slit was not until a day or two later. The double slit results were therefore submitted separately as a paper for oral presentation at a meeting of the American Physical Society.

In writing this Letter we were concerned, of course, to present our findings clearly and concisely. However, we were afraid to use the title "optical maser," because we had heard some reports that Maiman's letter reporting his very important results had been rejected by Physical Review Letters and we thought that the reason might be because the editors of that distinguished journal had previously expressed a disinterest in further papers on masers as being too concerned with devices for a journal at the frontier of physics. We therefore called the paper "Coherence, Narrowing, Directionality, and Relaxation Oscillations in the Light Emission from Ruby," and it was coauthored by R.J. Collins, D.F. Nelson, W.L. Bond, C.B.G. Garrett, W. Kaiser and myself. I learned much later that the reason for rejection of Maiman's paper was quite different. The editor thought, mistakenly, that it was just a small extension of the paper which he had published very recently, so that it violated their rule against serial publication.

We also were concerned in that paper to be more specific than Maiman had been in describing just exactly what we had used and had done. We therefore pointed out that our laser rods had been five millimeters in diameter and 4.0 cm long even though this was about the dimensions of the one shown in the newspaper photograph of Maiman. We also specified that the lamp used was a General Electric FT-524. It was a year later that I learned from Donald Buddenhagen that Maiman had in fact not used the FT-524 in his experiments. It had been shown in the press photograph because the lamps which he had actually used were all broken by that time. In a way that was fortunate, because the diameter of the FT-524 was larger than the lamp which Maiman used originally and had enough space in it to permit us to put in a small Dewar flask and do experiments at lower temperatures. We also learned much later that Maiman's original crystal had also not been as long and narrow as the one shown in the photograph, but that was not published until some time later.

While this work was in the course of publication, the Laboratories felt it important to demonstrate that this work had something to do with communications which is the prime task of the Bell System. They therefore arranged to transmit pulses of laser light from the Holmdel Branch of Bell Telephone Laboratories to Murray Hill, an airline distance of about twenty-five miles. There was known to be a direct line of sight between the two laboratories, and there was a tower at Murray Hill designed for microwave propagation experiments to and from Holmdel. The experiment was carried out by Boyle, Collins and Nelson and they were quickly successful in not only seeing the beam by eye but photographing an oscilloscope trace recording in detail the individual pulsations or spikes which simulated a message that might later be encoded on a laser beam. All this was reported at a press conference soon after the publication of our Physical Review Letter and received widespread attention.

Meanwhile, I had been continuing my studies of the dark ruby spectrum hoping to unravel the pair lines and find out which ones belonged to which kinds of pairs of chromium ions, nearest neighbors, second nearest and so on. While that had not gotten very far, I came to realize that the intensity of these pair lines was really strikingly high. This indicated to me that they were being pumped by the much more numerous isolated

atoms. For example, if the concentration is a tenth of a percent, then only one in a thousand of the chromium ions will happen to have a neighbor at the adjacent particular crystal lattice site. Yet, even though the pairs could not be as numerous, the lines could be as strong or even stronger than the lines of the isolated ions. Indeed the absorption was relatively weak, as it should be since paired ions are few, but the emission was strong. This indicated that there was an even more efficient method of exciting the pair lines than I expected and, while I did not want to get into competition with all those who were doing laser work, I finally decided to try out the pair-line ruby laser. By that time, in the fall of 1960, I had acquired a power supply and large flashlamps. The experiments were successful and George Devlin and I found that laser action could be obtained on either of the strongest pair lines separately or simultaneously, with or without laser emission at the R-line. This gave an interesting proof that the 7009Å line and the 7041Å line came from distinct, separate systems and that they are both separate from the R-lines.

We had avoided the word maser in the paper which Bell Laboratory people had sent to Physical Review Letters. But it was apparent by that time, that not everyone had understood that what we were talking about was really coherent stimulated emission from atoms in a resonator. I therefore decided that I would use the term optical maser in this paper, even if that did mean automatic disqualification from Physical Review Letters, and sent it instead to the slightly slower but equally reputable Physical Review. Physical Review Letters, which provides quick publication of important new results, has to be somewhat selective and must reject a substantial fraction of the papers submitted if it is to maintain a rapid publication schedule. Thus they can sometimes apply somewhat arbitrary criteria like the ban on masers which I supposed to be in effect. Indeed they must do so, because the initial discovery in any field is often followed by a growing flood of follow-up papers of gradually diminishing urgency.

However, somewhat to my surprise the paper ended up being published in Physical Review Letters. It happened because on the very same day that the paper by myself and Devlin arrived at the editorial office of the two journals, they also received a paper by I. Wieder and L.R. Sarles reporting stimulated emission from the

pair lines in ruby. The first I knew of it was when the Wieder-Sarles paper was sent to me to referee. I of course said that it looked like good work to me. Then the editors were faced with a necessity of treating both comparable papers on a similar basis, either both in Physical Review or both in Physical Review Letters and they chose the latter. Thus both papers describing laser action in dark ruby appeared in the February 1, 1961 issue of Physical Review Letters. Though the dark line ruby laser has been confirmed, it has not been very much explored because it has so far been operated at temperatures considerably below room temperature.

Around the end of November, 1960 I received a telephone call from Mirek Stevenson, who had been a graduate student with Townes at Columbia but was by that time at the IBM-Watson Research Laboratory. He told me that he and Peter Sorokin had obtained laser action in two more substances, calcium fluoride containing divalent samarium ions and calcium fluoride containing trivalent uranium ions. Both of these materials operated at cryogenic temperatures. Sorokin and Stevenson's results were already in course of publication in the IBM Journal and appeared a day or so later. I remember this event not only because it was one of the earliest laser materials, probably the second and third laser materials to operate, but also because a day or so later when the IBM group announced their results I had a telephone call from a reporter on the New York Times asking for comments on it. Fortunately, by that time I did know about it and I was able to help him get his story straight. For instance, I suggested that it was important for him to mention that the samarium and the uranium ions were respectively divalent and trivalent, and so it appeared in the New York Times story. At that time, there were not as many good science writers as there are now, and I have often been impressed by the care in which the New York Times took to verify their stories.

The other memorable aspect was that Stevenson told me sometime later that he had accepted the challenge of finding an optical maser material as a management problem. Even as a graduate student he had been very interested in investing and had been extremely successful in the stock market. Subsequently he founded several companies, mostly in the field of investment and investment research. But when presented with the problem of quickly constructing an optical maser he went at it by seeking

out the sources of supply for each of the components, that is the crystals, the flashlamp and the power supply and assembling them from these sources so that the experiment could be done in a hurry. This contrasted somewhat with a do-it-yourself approach which most of the others in the field followed. In this case it certainly did pay off, because Garrett and Kaiser at Bell Labs had been working on similar substances, but had not been able to reach the point of getting suitable samples installed in suitable equipment for tests. Undoubtedly, Sorokin also played an important part in this work for he has remained active and has indeed produced several of the most important advances in laser physics since that time.

The first public demonstration of an operating laser was given at the Nerem Electronics Meeting in Boston on November 17, 1960, accompanying my talk. Lewis Winner, who organized the Nerem Meeting had invited me early in the summer, before our experiments on lasers had advanced very far. Fortunately, by the time of the talk our results on the properties of ruby lasers had been published and my colleagues agreed that I could talk on them as well as about my own theoretical work. A large ruby laser power supply was transported to the hall and set up and we demonstrated that a bright red flash could be produced and shown as a momentary bright spot on the screen. We knew of nothing more spectacular to do with it at that time.

A few months later, in January of 1961, George Devlin and I were able to load an optical maser into the back of a station wagon and drive it to Toronto for a similar demonstration at a meeting of the Royal Canadian Institute. But by June of 1961, Bob Ammons had built a really portable ruby laser for Robert J. Collins to take and display at the International Commission for Optics Meeting in London. It used a commercial 200 watt-second photoflash power supply and a small ruby rod with a flashlamp and a little elliptical cylinder reflector.

During the same period, we were finding out some things that these early lasers could do. Willard Boyle showed that if the beam from a ruby laser was focused to a small spot on the surface of an absorber, it would vaporize a bit of that material, producing a white-hot jet. The temperature was so high that even the most refractory materials, such as carbon, could be instantly vaporized. Boyle realized that lasers could then be

used for drilling holes and all sorts of materials processing. Indeed within the next few years the Bell System, in their Western Electric manufacturing division, put lasers to use drilling holes in diamonds which were to be used for wire-drawing dies. Again, Boyle did not publish his results but I was able to use photographs of one of these laser produced jets to illustrate in articles which I wrote for Scientific American and the Solid State Journal. Very soon people began to drill holes in razor blades by focusing ruby lasers onto them. As lasers were made larger, it became possible to drill holes simultaneously through several razor blades stacked one behind the other. Soon, someone suggested that laser output power should be measured in Gillettes!

From the beginning, writers of popular accounts in the newspapers and some people in the military expected, or at least hoped, that lasers would fulfill the old dream of a death-ray. We had a good bit of fun with this notion, which was so very far beyond the capabilities of even the largest lasers. I have often shown a slide of our "death-ray countermeasures," which I made at that time. It shows some suits of shining armor, of the kind that knights used to wear. It was easy to calculate that an unprotected two hundred pound man could be completely evaporated by about two hundred million one-joule shots from a typical ruby laser. If we could deliver them at the rate of one per second, which was rather better than we could do, he would only have to stand there for six years. Still, it was obvious even then that, once the principle was established, you could expect ultimately to have very large sustained as well as pulsed powers even though we did not yet know how to achieve them.

But the problem remained of what one could do with the small, expensive, heavy pulsed lasers then existing. I moved to Stanford University in September 1961 and a few months later, in January of 1962 appeared on a local television program called "Science in Action." I wanted to illustrate my remarks with experiments but had only one very small ruby laser not even big enough to drill a hole in a razor blade. G. Frank Imbusch, who was then one of my graduate students, suggested that we could break a balloon with it and this was tested and found to be possible. So Imbusch, Linn Mollenauer another student, and I went to the studio. While I was rehearsing, the students fixed the balloon with a cardboard base and fins to look like a rocket ship, and positioned it in

front of the laser. I can assure you that I was much concerned as to whether the thing would actually work or not as the program was being broadcast live. Fortunately, it did work and I began showing balloon-breaking demonstrations. A year or so later, when I was in Washington I spoke at a meeting of a group of the International Scientific Radio Union (U.R.S.I.) which happened to meet at the same time as the Optical Society. We were therefore able to make a demonstration with a large ruby laser kindly provided by Trion Instruments (later Laser Systems, Inc.). With this laser we could not only break a blue balloon but we could let the light pass first through a red balloon which did not absorb the red light and was unaffected. This made it a more spectacular demonstration and showed that the color of the light had something to do with it.

Still, the mind works slowly and it was not until near the end of 1963 when it occurred to me that the balloon breaking experiment could be done with a dark blue balloon inside a clear outer balloon. I realized this as I was sitting with my son at the San Francisco Zoo and watching other children carrying such balloons. Of course such a demonstration is a very good illustration of something that could be done with a laser and not easily in any other way, for the outer balloon remained unharmed as it did not absorb the light. This is also a good illustration of the use of lasers to accomplish things that are at otherwise inaccessible places, for example as they are used for surgery on the retina of the eye. To make the stunt more effective Ken Sherwin, the technician with our research group, built a small, portable ruby laser into the housing from a toy ray-gun replacing the flashlight with which the toy had been sold. I showed this first at the meeting of the American Association for the Advancement of Science at Cleveland in December, 1963. It was at that time, while getting ready for the A.A.A.S. Meeting, that I realized that laser erasing was possible although my little ray-gun did not have enough power to erase more than a small portion of a typewritten character.

The idea of a laser eraser appealed to me, not only because it was elegant and useful, but also because it pointed in quite the opposite direction to the thinking of most of the efforts seeking laser applications. Here was something that certainly could be done, and for which there was a very large potential market. The only problems were engineering and economic. In this it

contrasted sharply with attempts to make laser weapons for which there were very large amounts of money, but which no one knew how to do. Naively, I thought that I could just announce the idea of a laser eraser and people would start to make them. When this failed, I was urged to apply for a patent and did. It was apparent that nobody would make the substantial investment to produce laser erasers without at least the protection of a patent. The patent was finally granted in 1970, but it may be that even that will not be sufficient to overcome the economic obstacles to laser eraser use until suitable lasers are put into quantity production for some other purpose.

Perhaps a little should be said about the atmosphere surrounding the laser research and the way information was communicated. The initial paper by Townes and myself setting forth the requirements for an optical maser and indicating the properties needed for suitable materials did inspire both considerable interest and considerable skepticism. A number of very good reasons were advanced why lasers might not ever work, but some people did take the possibilities seriously enough to work on them. When lasers were actually demonstrated in 1960, they did cause considerable excitement both in the popular press and in the scientific and engineering community. Peter Franken had described later, at a Symposium of the Optical Society of America in 1971, the atmosphere at the Optical Society sessions on lasers in the spring of 1961. "..... I recall now, just ten years ago on the nose, the first meeting of any professional society on the laser. That was in the spring meeting of 1961 in Pittsburgh of this Society and many of you may not have had the opportunity to be there. Let me spend a minute and tell you what it was like: sheer panic. At most meetings, just to give you one parameter, at most meetings people carry some cameras and a man will show a slide, that is have a slide shown at his request and you'll hear a few clicks. At that meeting every time a slide was projected it was like the sullen rumble of semi-quieted, semi-automatic fire. In fact the high point in that meeting occurred during my good friend Arthur Schawlow's lecture, which was a chalk talk and he was talking about some of the puzzles and mechanisms of the ruby laser. I recall a real key point, I forget the rest of your talk, but the one point I remember was your saying "We think that there are two mechanisms operative in the spiking of the ruby laser," which was a big puzzle then. He went to the blackboard,

picked up a piece of chalk and wrote down the number 1, turned away from the blackboard and a dozen cameras went off. This is the kind of panic that was going on ....." I think Franken must have picked up a bit of the excitement himself, for he soon borrowed a laser and then with his associates Hill, Peters and Weinrich achieved for the first time optical harmonic generation, thereby ushering in the new and ever growing field of nonlinear optics.

Not only were there scientists, but the early laser meeting attracted a very large number of outsiders such as engineers in aerospace companies, who were eager to garner any scrap of knowledge about these new devices which they might incorporate in systems and sell to the government. To me, the high point of this general excitement over the prospects of lasers came at the meetings sponsored at the Meeting of the Polytechnic Institute of Brooklyn in New York in March of 1963 where it seemed that every few feet along the corridor there was someone else asking me some question or other. But then the excitement subsided as we expected and people got down to the serious business of learning more about the operation of lasers, finding new ones and finding out what they could do. Moreover, the Tower of Babel effect become more noticeable as individual specialties within the laser field grew large enough so that specialists from areas could hardly communicate with others and indeed did not feel that it was necessary to do so very often.

What did I learn from all this? Many things, although no golden rule on how to do good research. To every suggested maxim for guiding research, it is easy to find a counter example. I have made many mistakes and have seen my colleagues make mistakes either overlooking something which later seemed obvious or even denying it. Yet a good forthright error is often a stimulating thing as it challenges others to prove you wrong. Many times new workers in the field seemed quite foolish and they foundered initially but then suddenly they were productive and producing their own original contributions.

It also appears that there are times when one should attack a problem head-on, seriously analyzing the steps needed to attain a desired goal. This was what Townes and I did in our analysis of the conditions for an optical maser. At other times it is better to

admit that we do not know enough and just sit back and add fundamental knowledge, depending on both instinct and logic to pick out potentially fruitful areas.

I also believe it is a good thing for some of us should mix pure and applied research committing ourselves fully to neither. Thus Townes could, as he did in the late 40's, study deeply the processes in the ammonia molecules as an example of the interaction between molecules and electromagnetic fields. From this study, he was able to obtain enough knowledge to invent the maser. But if he had not had the practical interests in finding out about applications, then that particular piece of knowledge might not have ever found its way to the mind of a person interested in generating electromagnetic waves.

As one looks at the parallel histories of other new fields of technology such as electronics or aviation, one cannot help realizing that lasers are still in a very early stage of their development and that many of the most exciting experiences of discovery are still to come.

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Masers and Lasers

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CHARLES H. Townes likes to tell the story of how he invented the maser. In the spring of 1951, while he was a Professor of physics at Columbia University and I was a postdoctoral research associate in his laboratory, we both attended the meeting of the American Physical Society in Washington. As Townes remembers it, we shared a room at the Franklin Park Hotel. He had several small children and so was used to waking up early, while I, being then a bachelor, was used to sleeping later in the morning. When this happened one day, he dressed quietly in order not to disturb me and went outside to enjoy the pleasant spring morning in Franklin Park. There he thought about the Office of Naval Research's Millimeter Wave Committee which was to meet later that day. Two years of meetings had so far failed to produce any major breakthrough in ways of generating radio waves shorter than the centimeter lengths of the microwave region. At that point he realized how to use molecules rather than free electrons to generate these waves.

Townes had for several years realized that the sharp resonances in atoms or molecules could act as radio circuit elements. He had even obtained a patent on some of these uses while at Bell Telephone Laboratories. He was also aware that whereas ordinary molecules absorb waves, excited molecules could amplify by the process of stimulated emission. Lamb and Retherford [1] had remarked on that possibility. But there seemed to be nearly insuperable problems. Stimulated emission is the true negative of absorption, and the same atoms or molecules can do either. An atom in a lower energy state absorbs radiation, thereby being excited to a higher state with more stored energy. Amplification, on the other hand, occurs when an electromagnetic wave interacts with atoms already excited to upper energy levels and stimulates them to emit, thereby enhancing the wave at the expense of the atom's stored energy. Thus if there are more atoms in the higher energy state, amplification will occur. But far more commonly, atoms in the lower state are more numerous and so absorption predominates. Indeed, in thermal equilibrium at any temperature, there are always more atoms in lower energy states than in upper ones, so that absorption is commonplace and amplification by stimulated emission is never observed. Thus even though the concept of stimulated emission had been introduced by Einstein more than thirty years earlier [2], and its existence had been confirmed experimentally by Ladenburg [3], the possibility that it could be dominant seemed, to most scientists, so remote as to be not worth considering.

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Yet, radiofrequency and microwave spectroscopy had blossomed in the 1940's for studying atoms, molecules, and nuclei. With even a modest amount of radio power it was easy to saturate an absorption and drastically alter the numbers of atoms in the various quantum states. Indeed one had to be careful to avoid distorting the spectra by saturation. In the limit of high radio wave intensity, the populations of the upper and lower states were equalized. This process would not invert the population distribution and produce the excess of upper state atoms needed for amplification. Nevertheless radiofrequency resonance studies showed that it was really possible to get far away from equilibrium and to drastically alter the absorption.

There was one other clue. Purcell and Pound at Harvard University [4], when studying nuclear magnetic resonance found that the relaxation time of lithium nuclei in lithium fluoride is extraordinarily long, fifteen seconds at room temperature. When a magnetic field is applied to the crystal, the nuclei of the lithium atoms, acting as the tiny magnets that they are, precess around the magnetic field. After a time, they exchange energy with the crystal's thermal vibrations, and at very low temperature would all settle down with their magnetic moments pointing in the direction of the magnetic field. But at room temperature a smaller fraction of the spins are thermally excited to the higher energy state where they are opposite to the field direction. Transitions between the two states can be induced by the absorption or emission of radio waves, and ordinarily absorption predominates. Purcell and Pound discovered an ingenious method for rapidly reversing the spin direction, and this resulted in a momentary change of the nuclear resonance signal from absorption to stimulated emission. They did not discuss the possibility of amplification, and their effect was probably just a small reduction in the losses of the circuit coupled to the crystal. Indeed when Joseph Weber three years later in 1953 considered seriously whether useful amplification by stimulated emission could be obtained, he discussed the momentary gain from inverted spin systems and found it to be very small.

But when Townes, on that spring morning in 1951, thought about how to make an oscillator or amplifier using stimulated emission, he thought about gaseous molecules and, in particular, ammonia. He and his students had investigated many aspects of this molecule which so strongly absorbs microwaves. Then he realized that there was a way to separate excited molecules which could emit microwaves from unexcited absorbing molecules. If a beam of ammonia molecules passed through a suitable electric field gradient, the molecules would be deflected. Most important, molecules in these two lowest states would deflect in opposite directions, and so they would be spatially separated. An

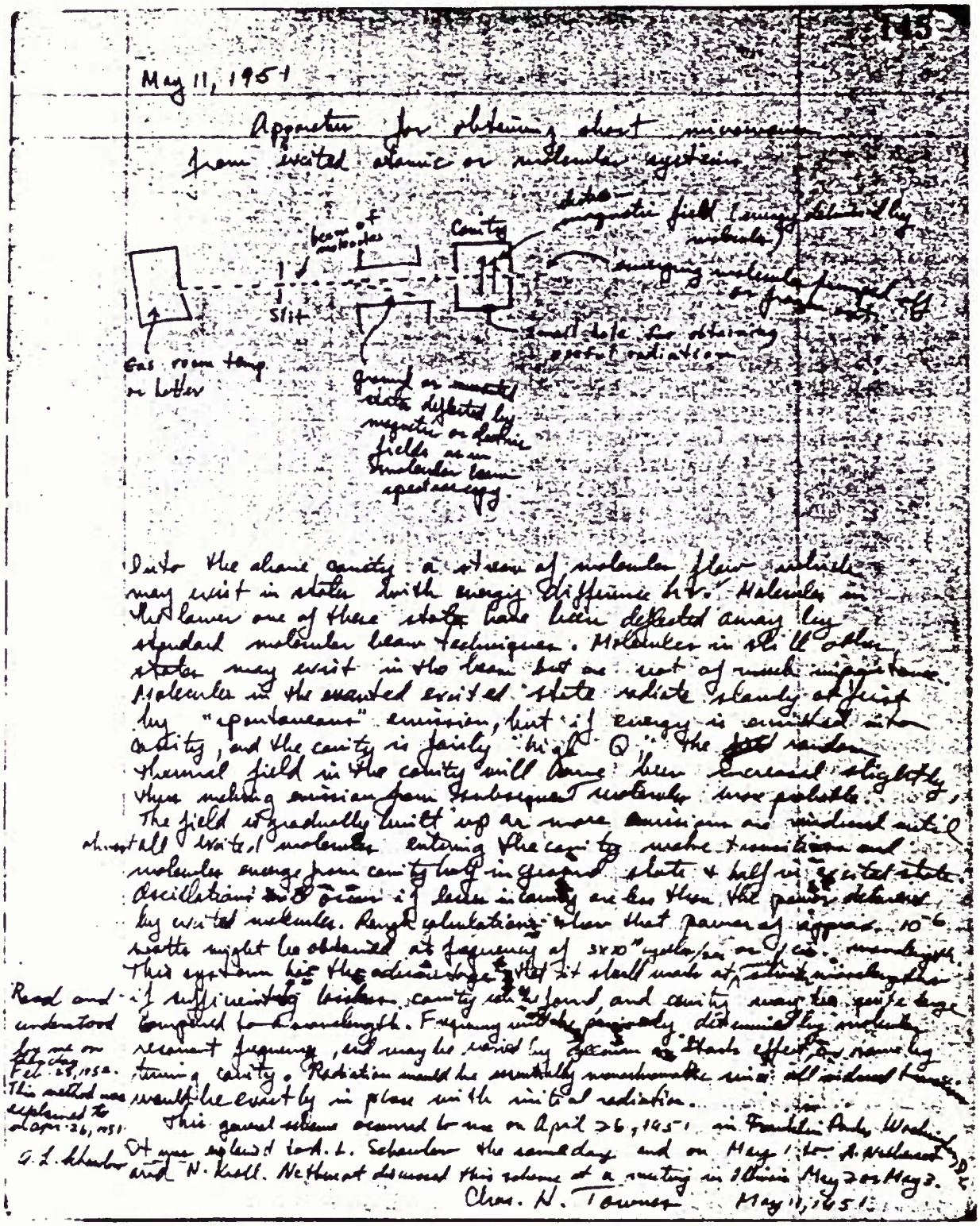

Fig. 1. Page from C. H. Townes' notebook recording the idea of the original maser, 1951.

aperture at the end of the beam could then be positioned to accept only molecules in the excited state, and to permit them to pass into a cavity resonator.

The resonator would be tuned to the microwave frequency that the excited ammonia molecules could amplify, and would greatly increase the coupling between the molecules and an electromagnetic wave. Thus even a moderate, attainable number of excited ammonia molecules could give observable amplification. Moreover, if excited molecules could be supplied fast enough, the rate of stimulated emission would exceed the resonator losses. Then a sustained time-coherent oscillator would be produced from the stored energy of incoherently excited molecules. Fig. 1 shows Townes' notebook entry recording the invention.

Townes was optimistic that such a molecular oscillator-amplifier would work and so he began to build one with James Gordon, a graduate student, and Herbert Zeiger, a

Fig. 2. C. H. Townes and T. C. Gordon with the second ammonia maser. T. C. Wang on the right stands by the first maser, 1954.

research associate. He did not feel it appropriate to publish the ideas until they had experimental confirmation. However, he talked about it quite widely and described it in the Columbia Radiation Laboratory's Progress Report. The project was difficult and complex, but oscillations were obtained from an ammonia beam device early in 1954 [6]. It was dubbed a "MASER" an acronym for Microwave Amplification by Stimulated Emission of Radiation. This first molecular oscillator produced a pure frequency microwave output of about 10^{-8} W at 23.87 GHz. The output frequency was primarily determined by the molecular resonance, so that its oscillation frequency could be used as a frequency standard or atomic clock. As an amplifier, it gave excellent low-noise performance but the narrowness of the spectral lines, which made them so suitable for a wavelength standard, implied a very narrow bandwidth and little tunability. Fig. 2 shows Townes with students J. P. Gordon and T. C. Wang with the first ammonia-beam maser.

In the three years between Townes' conception of the maser and publication of the first results, others had been seeking the way to a molecular amplifier. In 1953, J. Weber discussed the possibility of obtaining traveling wave amplification and mentioned the possibility of using nuclear or electron spins [5]. However, he did not propose a particular workable material or the use of a resonator. N. G. Basov and A. M. Prokhorov early in 1954 proposed a method of selecting out excited molecules in a molecular beam, quite similar to Townes' [7]. Later Basov and Prokhorov suggested another possible method of obtaining active molecules for a maser oscillator [8]. They pointed out that if the atomic or molecular system had three or more energy levels, a high-frequency electromagnetic field could excite enough atoms for amplification. The high-frequency field would be tuned to excite atoms from the lowest to the highest of the three quantum states. If strong enough, it would equalize the number of atoms in these two states and there would then be no more absorption but also no amplification at that frequency. However, there could be amplification at the frequency corresponding to a transition between the upper and intermediate levels or to that between the intermediate and lower levels. In these three-level schemes, the output frequency would be lower than that of the pumping field. Thus the three-level maser did not offer a way to produce a shorter wavelength shorter than that of the driving oscillator. Basov and Prokhorov did not discuss a specific molecular or atomic system, and the importance of this idea was not widely appreciated at the time. Indeed, Townes had also recognized that masers could be pumped by radio waves or even by light and recorded that in his notebook in 1954, but had not bothered to publish because it was not yet specific. Attention then turned to electron spins in paramagnetic solids, which gave strong microwave resonances which could be tuned by a wide range by an applied magnetic field. Combrisson, Honig, and Townes [9] were able to produce momentary microwave amplification effects from donor atoms in silicon after suddenly inverting their electron spins by the method of adiabatic fast passage. In the following year N. Bloembergen [10], not knowing of Basov and Prokhorov's 1955 paper, independently discovered the three-level pumping method and made a detailed proposal for three-level, solid-state masers using nickel–zinc fluosilicate and gadolinium–lanthanum ethyl sulfate. A few months later, H. E. D. Scovil, G. Feher, and H. Seidel [11] obtained continuous-wave maser amplification in gadolinium–lanthanum ethyl sulfate with relaxation rates altered by the addition of 0.2 percent cerium ions.

During the next several years, work on solid-state three-level masers grew rapidly, and they were quickly applied as sensitive low-noise preamplifiers for microwave astronomy, satellite communications, and radar. New materials were found, such as potassium cobalt–chromium cyanide by A. L. McWhorter and James W. Meyer [12], and ruby by G. Makhov, C. Kikuchi, J. Lambe, and R. W. Terhune. Both Bloembergen's and Townes' groups built masers, the former using the potassium cobalt–chromium cyanide and the latter using ruby. Consequently, when Townes and Bloembergen shared the Liebmann Award of the Institute of Electrical and Electronic Engineers in 1959, Townes had the ruby from his radioastronomy maser made into a pin and presented it to his wife. On the way home from the awards ceremony, Mrs. Bloembergen asked her husband why he didn't do something like that from his maser. All Bloembergen could do was reply "But my maser was made of cyanide, dear!"

The rapid growth of maser research for low-noise amplifiers and wavelength standards is strikingly shown by the large number of papers, and especially the many laboratories represented, at the first Quantum Electronics Conference in September of 1959. But already there was competition from parametric amplifiers. Indeed one very able scientist at Bell Telephone Laboratories decided that masers had no future in communications, and so he changed to working on parametric amplifiers.

While Townes and his students had invented the acronym "MASER" from Microwave Amplification by Stimulated Emission of Radiation, very soon there was talk of similar acronyms for other, as yet unknown, devices. These

included RASER (Radio Frequency), IRASER (Infra Red), LASER (light), UVASER (Ultraviolet), XRASER (X Rays) and GRASER (Gamma Rays). Of all these, the term LASER has subsequently become an accepted, commonly used term.

From Maser to Laser

When Townes first thought about the ammonia molecular-beam oscillator in 1951, he had hoped that it could be made to work on a rotational transition and generate a wavelength in the half-millimeter region. But it had turned out to be more convenient to work in the centimeter region, where techniques were well known. So the problem of using atomic systems to generate coherent radiation of wavelengths shorter than the visible region remained unsolved when Townes and I turned our attention to it in 1957.

Although I had been a research associate at Columbia University with Townes and had been excited by the maser concept when I heard it in 1951, I did not work on it. By then I had accepted a position in the physics research group at Bell Telephone Laboratories. There I was hired to work on superconductivity, and so that was what I did. On weekends I worked with Townes on a book, *Microwave Spectroscopy*, which was published in 1955. But by 1957, I felt that the time was right to take a serious look into the possibilities of extending the maser principle to shorter wavelengths. I wanted to identify the difficulties and see if solutions could be found for them. To start, I had only some vague notions about using ions in crystals, which do sometimes have fairly sharp resonances in the far-infrared region. As Townes was then consulting with Bell Telephone Laboratories, I mentioned this to him. He also had begun to think about the problem, and so we decided to work on it together.

By that time (around October 1957), Townes felt that it would be better to jump over the difficult far-infrared region and try to build a maser in the near-infrared or even visible, where so much more was known. Moreover, he had some ideas about one particular possible system using thallium atoms. He had made some notes and arranged to send me a copy.

We both realized that incoherent light from a lamp could be used to pump three-level or four-level optical masers, analogous to these already in use at microwaves, if the pumping light is bright enough. The fact that the pumping light would be incoherent would not matter, any more than the independent arrival times of the molecules of the ammonia beam to its resonator. In either case, the output wave's coherence would be determined by the wave stored in the resonator. The excited atoms would be stimulated to emit radiation in phase with the stored wave.

The first problem was to see how much exciting light would be needed and see if that would be obtainable from any conceivable lamps. To begin with, it seemed best to study atoms, and preferably fairly simple atoms such as the alkali metals. The spectra of these atoms were already quite well measured and understood, so that we could look up the energy levels in the monumental *Atomic Energy Levels* [14], a National Bureau of Standards Publication edited by Charlotte Moore-Sitterly. Information on the relative and absolute strengths of the various spectral lines was much scarcer, but there was some help from the tabulation of L. Biermann in the *Landolt-Börnstein Tables* [15]. Moreover, in the book *Resonance Radiation and Excited Atoms* [16], by A. C. G. Mitchell and M. W. Zemansky, there was a wealth of stimulating information about the absorption and emission of light from alkali atoms. Finally bright alkali-metal lamps could be made, and were already commercially available, which emitted just those wavelengths needed to excite other alkali atoms.

Townes had the maser equation, which would permit us to calculate how many excited atoms would be needed for optical maser oscillation. This equation expressed the requirement that there must be enough excited atoms to ensure that stimulated emission will supply energy to the optical electromagnetic field faster than the field will lose energy at the resonator walls. If the atom's oscillator strength (a measure of the effective number of electrons in a classical oscillator that would absorb or radiate as well as the atoms at the particular wavelength) is large, a small optical field will stimulate strong emission and only a few excited atoms will be needed. If the oscillator strength is small, as it is for "forbidden" transitions, correspondingly more excited atoms will be needed for maser oscillation. For a microwave gas maser, similar statements could be made, except that the custom is to talk in terms of the transition dipole moment rather than oscillator strength.

However, there is one important difference between the optical and microwave cases. In the optical region, an excited atom rapidly loses its stored energy by spontaneous emission. Like stimulated emission, the rate of spontaneous emission is also proportional to the oscillator strength. Thus when the oscillator strength is high the excited atoms decay quickly and must be replaced quickly. As a result, the rate at which excited atoms must be supplied is independent of the oscillator strength. A forbidden spectral line could be as usable for an optical maser as an allowed line

Our study of the maser equation revealed another interesting fact. A light wave in the resonator would lose energy only at the walls, and would gain it by stimulating emission as it passed from one wall to another. Thus it would be helpful to make the device fairly large, very much larger than one optical wavelength. Then the stimulated emission could take place over a relatively long path between wall reflections. The greater this distance, the less the required density of excited atoms would be. Thus we could reduce the power density requirement for the pumping radiation by making the device larger.

With these considerations in mind, we made an estimate of the pumping power requirements for an optical maser using potassium vapor. The excitation would be supplied at a wavelength of 404.7 nm and the output would be in the infrared at either 3.14 or 2.71 μm. Assuming, for the calculation, convenient-appearing dimensions of 10-cm length and 1-cm crosssection, we estimated that a pumping power of 1.2 mW would be needed. This seemed quite attainable, particularly since measurements by our colleague

Robert J. Collins (now at the University of Minnesota) showed that outputs of up to 0.6 mW at the required wavelength could be obtained from a single commercial lamp. Brighter and bigger lamps could very probably be made, and the dimensions of the structure could be changed if needed.

Throughout these discussions, we had always thought of a resonator which, unlike that of a microwave maser, would be very large compared with a wavelength of the radiation. This seemed to be rather obviously necessary, for optical wavelengths are of the order of 1/20 000 cm. Even if a tiny single-wavelength resonator could be fabricated, it would be too small to hold enough amplifying atoms to overcome its losses. On the other hand, a large resonator would not provide mode selection in the way that a microwave maser's cavity does. A resonator of centimeter dimensions would be capable of sustaining very many different modes of oscillation, even within the small bandwidth that the excited atoms could amplify. Martin Peter, another Bell Laboratories colleague who is now at the University of Geneva, particularly urged us to worry about the problem of mode selection. He feared that the output of an optical maser, even if it did oscillate, would be a jumble of rapidly fluctuating modes of oscillation, difficult to distinguish from spontaneous radiation. Townes felt rather that the problem might not be crucial, since some perhaps moderate number of modes would have lower losses than the others and would be most likely to attain oscillation. While the oscillations in the favored modes would fluctuate, they might well be quite distinctive. In all, he felt that lack of a better method should not prevent going ahead with work on systems in which modes were not well controlled.

At this point, I recalled that the resonant modes of a large box can be thought of as waves traveling in various directions between the walls of the box. Their wavelengths would be restricted by the fairly narrow bandwidth of the amplifying spectral lines. If we could restrict their directions, we would drastically reduce the number of available modes of oscillation. I thought of such things as having one or more walls made up of diffraction gratings. Then I realized that the problem did have a simple solution. We could remove almost all of the walls of the box, and leave only two small mirror-like sections facing each other at the ends of a long pencil-like column of amplifying material. As long as the end surfaces were much larger than a wavelength they would act as good mirrors and reflect waves straight back and forth between them. A wave traveling in a direction inclined at even a small angle to the axis would miss the end mirror and be lost. When I told Townes about this idea, he pointed out that the directional selection would be even better than I thought, because the amplification would permit a wave in the selected direction to make many passes back and forth along the axis. An off-axis wave might make one or two traversals but would then be lost. Even though a detailed theory of the laser resonator came considerably later, [17], [18] it seemed clear that it would indeed select modes and produce a narrow beam. These detailed calculations confirmed our conclusion that the optical maser modes would be very nearly plane waves.

Thus we knew we had a suitable structure and several likely materials, and were convinced that an optical maser could be made. Still, it had never been done and there might be unforeseen problems. So we decided to publish our analysis, and it appeared in the December 15, 1958 issue of *Physical Review* [19]. Townes then started a graduate student, Herman Cummins on a project to try and build an optical maser using potassium vapor. He was later joined by another student, Isaac Abella, and by Dr. Oliver S. Heavens, an English scientist who was already well known for his work on optical properties and uses of thin films. Considerable progress was made, but eventually the successes of other researchers led the Columbia University group to abandon work on potassium for easier approaches.

Our paper on optical masers attracted considerable interest. Some people had serious doubts that it would even be possible to build an optical maser, and some very plausible arguments were advanced to prove that it would not work. But a number of others seriously sought suitable materials and ways to activate them. At Bell Telephone Laboratories, W. S. Boyle mentioned the possibility of using a gas discharge, but did not explore it seriously. He was interested in semiconductors and carried his analysis far enough to get a patent on a laser using recombination radiation in a semiconductor. Independently, Ali Javan considered using a gas discharge as the amplifying medium for an optical maser, and published a specific proposed method for using a mixture of helium and neon [20]. With William R. Bennett, Jr. and Donald R. Herriott he set out to build a gas-discharge optical maser. They made measurements of the excited state lifetimes and optical amplification under various conditions. Then they designed a laser structure with a discharge 1 m long, with extremely flat, very highly reflecting end mirrors inside the ends of the tube. John Sanders, visiting Bell Telephone Laboratories for eight months from Oxford University, proposed that an electrical discharge in pure helium might be used [21] and made some efforts to test the idea.

For myself, I felt that it would be better to try to use a solid material that could be optically excited to provide optical maser action. This was very much in keeping with the strong emphasis on solid-state devices in Bell Laboratories at that time. Although I knew nothing about the optical and luminescence properties of solids, we had mentioned the possibility of solid maser materials in our 1958 paper. So this study provided me with a good reason to stop research on superconductivity and begin learning about luminescent crystals.

One substance that was easy to start with was ruby, for that material was being extensively studied for solid-state microwave masers. Colleagues like Joseph Geusic had large stocks of ruby crystals with various concentrations of chromium ions in the aluminum-oxide crystal. I was intrigued by the fact that strong sharp-line fluorescence in the deep red could be excited by broadband light in the green and blue region of the spectrum. Thus one could use

a broad-band pumping light to get relatively high gain per excited atom of a sharp-line emitter.

However, in ruby all of the chromium ions are initially in the ground electronic state. In emitting fluorescence they return to this same state, and so the fluorescent light can be absorbed by the unexcited ions. As a result, it would be necessary to excite more than half of all the chromium ions before optical amplification would exceed the absorption losses. As no laser had ever been built I assumed that it meant it must be very difficult to make one. Without doing a serious quantitative analysis, I assumed that one would require a fourth energy level far enough above the ground state so that it would be nearly empty at the operating temperature. Then as soon as any atoms were excited to the upper state, amplification could be obtained by stimulating them to emit to this empty final level. Because the chromium ions in ruby lacked any such fourth level, this did not seem a promising material for laser action.

Nevertheless the spectrum of ruby had some intriguing features that seemed worth studying. The theory of the spectra of ions like chromium in crystals was fairly well developed by that time, especially from the work of S. Sugano and Y. Tanabe in Japan [22]. The theory explained only the two strong red "R" lines, but there are many weaker lines within a few hundred angstroms further to the red of these. These satellite or neighbor lines had been known for about fifty years but had never been explained. I thought at first that they might arise from the crystal lattice vibrations and thus might give information about those modes of vibration. However my technician, George Devlin, noticed that the intensity of these lines varied from sample to sample. We soon found, with the collaboration of Darwin L. Wood, that the intensity of the neighbor lines increased rapidly as the chromium concentration was increased. Thus we were convinced that the neighbor lines arose from pairs of adjacent chromium ions, whose energy levels were split by exchange interactions and we published this finding in collaboration with Albert M. Clogston who analyzed the theoretical aspects [21]. The lines were spread out enough to give us hope that for near-neighbor pairs the exchange-splitting might produce the fourth level we wanted for the lower state of a laser. I pointed out the possibility of getting laser action in dark ruby at one of the satellite wavelengths, at the first Quantum Electronics Conference in September 1959 [23]. We were encouraged by the fact that at low temperatures a crystal of dark ruby showed strong emission but little absorption at some of these wavelengths. I even made a brief try at getting laser action from a rod of dark ruby using a small flashlamp, but that was not sufficient and I returned to trying to analyze the neighbor-line spectrum of ruby.

Others made more serious attempts to achieve operation of an optical maser. At Bell Telephone Laboratories, Geoffrey Garrett and Wolfgang Kaiser investigated rare-earth ions in crystals, looking for a four-level system that could be energized by a bright lamp. At TRG, Incorporated, a group including Gordon Gould and Richard Daly studied both gases and solids.

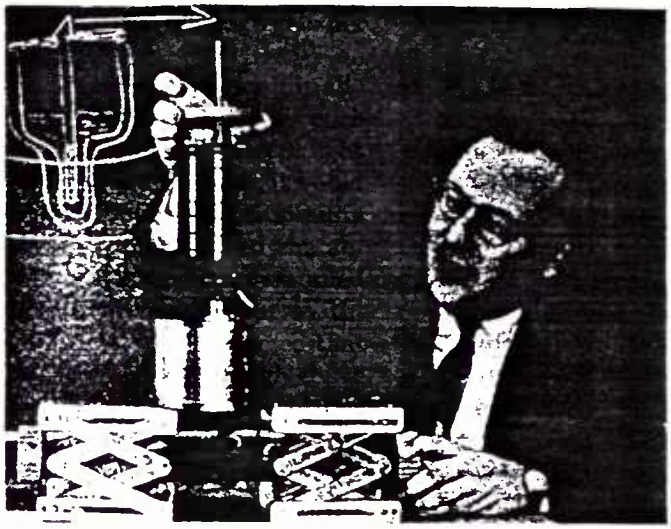

Fig. 3. A. L. Schawlow with an early laser using a dark ruby rod, cooled by liquid nitrogen.

But the first success was achieved by Theodore H. Maiman at the Hughes Aircraft Company's research laboratory in Malibu, California. Maiman had worked on microwave solid-state masers using ruby, and his laser used a rod of pink ruby. A careful quantitative study convinced him that a flash lamp could excite enough ions to give laser action in pink ruby. He succeeded in demonstrating stimulated emission in ruby and announced the results in July, 1960 [24].

A number of scientists at Bell Telephone Laboratories then pooled their resources and used optically-pumped ruby crystals to confirm the predicted properties of laser light [25]: directionality, monochromaticity, and coherence, as well as the high intensity that Maiman had reported. They also found that the laser flash was composed of many short spikes, each lasting about 1 μs.

Before the end of 1960, no less than four other lasers were operated. Unknown to me, Peter Sorokin at IBM Research Laboratory had also been studying four-level ionic systems in crystals and, with Mirek Stevenson, he obtained laser action both from trivalent uranium ions and from divalent samarium ions in calcium fluoride [26], [27]. These were the first operating four-level lasers, but they were very soon followed by another.

In November 1960, George Devlin and I tried again for laser action in the satellite lines of dark ruby, using the same rod as before but with a larger flashlamp (see Fig. 3). We obtained oscillation at 701.0 and 704.1 nm, in addition to the R line at 694.3 nm. At low temperatures, the satellite lines had the lower pumping requirements as expected since they were in a four-level system with an empty lower level. At just about the same time, Irwin Wieder and Lynn R. Sarles of Varian Associates also observed stimulated optical emission in the satellite lines. By coincidence, both our papers and theirs arrived at the *Physical Review* office on the same day, December 19, 1960 [28]–[29].

But even before then Javan, Bennett, and Herriott had achieved laser oscillation in their helium–neon electrical discharge. Theirs was not only the first gas discharge laser, but also the first continuous-wave laser [30]. It operated

at several wavelengths around 1.15 μm in the near infrared. In 1962, A. D. White and J. D. Rigden [31] found that the same gas mixture would oscillate at 632.8 nm in the visible if mirrors designed for high reflectivity in the red rather than infrared region were used.

By the end of 1960, five different kinds of lasers had been operated. Pulses with peak power in the kilowatt range had been demonstrated, as well as low-power continuous operation. Already the range of wavelengths spanned a range of 3.6:1, from ruby's 0.69 μm to calcium fluoride: uranium's 2.5 μm. From then on the progress has been increasingly rapid, with many new materials and great extensions of the wavelength and power outputs both continuous and pulsed. Quite marvelous and unexpected things have been discovered. Yet I cannot help thinking that even greater surprises may lie in the future. There are so many things that we can imagine but cannot do economically or even cannot do at all. Quantum electronics has attracted some of the keenest and most original minds of our generation and they are continually making surprising discoveries. As a result, the subsequent history of lasers is already much longer than these early beginnings.

At the time when we wrote our 1958 paper, Townes and I were convinced that an optical maser could be made. But we were surprised to find how simple the first lasers turned out to be. Indeed many people since then have wondered why lasers were not discovered twenty or thirty years earlier, long before microwave masers. The techniques were available then, but the ideas were not. Every scientist was trained to view the world as close to being in equilibrium. Really radical departures from equilibrium would be needed for stimulated emission to dominate, and that seemed unthinkable. However in microwave and radio-frequency spectroscopy, the quanta of radiation are very small, and so molecules or nuclear spins can only absorb small amounts of power before being strongly saturated. Thus large departures from equilibrium were observed without being sought, and researchers had to become very conscious of stimulated emission. Thus minds were prepared to generate the clever ideas that led to masers, and eventually to lasers. It was understanding and ideas that were needed, more than techniques. Fortunately, scientists were able to undertake wide ranging studies of matter under unfamiliar conditions. They were thus led to new insights and from that to the radically novel devices we now know as masers and lasers.

REFERENCES

[1] W. E. Lamb, Jr., and R. C. Retherford, "Fine structure of the hydrogen atoms, Part I," *Phys. Rev.*, vol. 79, p. 549, 1950 (remark on p. 570).

[2] A. Einstein, "On the quantum theory of radiation," *Phys. Zeit.*, vol. 18, p. 121, 1917.

[3] R. Ladenberg, "Investigations on the anomalous dispersion of excited gases; Part I, Test of the quantum-theoretical dispersion formula," *Z. Physik*, vol. 48, p. 15, 1928.

[4] E. M. Purcell and R. V. Pound, "A nuclear spin system at negative temperature," *Phys. Rev.*, vol. 81, p. 279, 1951.

[5] J. Weber, "Amplification of microwave radiation by substances not in thermal equilibrium," *Trans. I.R.E. Professional Group on Electron Devices*, vol. PGED-3, June 1953.

[6] J. P. Gordon, H. J. Zieger, and C. H. Townes, "Molecular microwave oscillator and new hyperfine structure in the microwave spectrum of NH_3," *Phys. Rev.*, vol. 95, p. 282, 1954.

[7] N. G. Basov and A. M. Prokhorov, "Application of molecular beams to the radio spectroscopic study of the rotation spectra of molecules," *Zh. Eksp. Theo. Fiz.*, vol. 27, p. 431, 1954.

[8] N. G. Basov and A. M. Prokhorov, "On possible methods of producing active molecules for molecular generator," *Zh. Eksp. Theo. Fiz.*, vol. 28, p. 249, 1955.

[9] J. Combrisson, A. Honig, and C. H. Townes, "Use of electron spin resonance for a microwave oscillator or amplifier," *Comptes Rendues Acad. Sci. Paris*, vol. 242, p. 2451, 1956.

[10] N. Bloembergen, "Proposal for a new type solid state maser," *Phys. Rev.*, vol. 104, p. 324, 1956.

[11] H. E. D. Scovil, G. Feher, and H. Seidel, "Operation of a solid state maser," *Phys. Rev.*, vol. 105, p. 762, 1957.

[12] A. L. McWhorter and J. W. Meyer, "Solid state maser amplifier," *Phys. Rev.*, vol. 109, p. 312, 1958.

[13] G. Makhov *et al.*, "Maser action in ruby," *Phys. Rev.*, vol. 109, p. 1399, 1958.

[14] Charlotte E. Moore, *Atomic Energy Levels*, U.S. National Bureau of Standards Circular 467 (1949, 1952, and 1958).

[15] A. Eucken and K H. Hellwege, Eds., *Landolt-Börnstein Tables Vol. I, Atoms and Ions*. Berlin: Springer-Verlag, 1950.

[16] A. C. G. Mitchell and M. W. Zemansky, *Resonance Radiation and Excited Atoms*, Cambridge University Press, 1934.

[17] A. G. Fox and T. Li, "Resonant modes in an optical maser," *Proc. I.R.E.*, vol. 48, p. 1904, 1960.

[18] G. Boyd and J. P. Gordon, "Confocal multimode resonator for millimeter through optical wavelength masers," *Bell Syst. Tech. J.*, vol. 40, p. 489, 1961.

[19] A. L. Schawlow and C. H. Townes, "Infrared and optical masers," *Phys. Rev.*, vol. 112, p. 1940, 1958.

[20] A. Javan, "Possibility of production of negative temperature in gas discharges," *Phys. Rev. Lett.*, vol. 3, p. 87, 1959.

[21] J. H. Sanders, "Optical maser design," *Phys. Rev. Lett.*, vol. 3, p. 86, 1959.

[22] S. Sugano and Y. Tanabe, "Absorption spectra of Cr^{3+} in Al_2O_3: Part A Theoretical studies of the absorption bands and lines," *J. Phys. Soc. Japan*, vol. 13, p. 880, 1958.

[23] A. L. Schawlow, D. L. Wood, and A. M. Clogston, "Electronic spectra of exchange-coupled ion pairs in crystals," *Phys. Rev. Lett.*, vol. 3, p. 271, 1959.

[24] T. H. Maiman, "Optical maser action in ruby," *British Communications and Electronics*, vol. 7, p. 674, 1960; *Nature*, vol. 187, p. 493, 1960.

[25] R. J. Collins *et al.*, "Ph Coherence, narrowing, directionality, and relaxation oscillations in the light emission from ruby," *Phys. Rev. Lett.*, vol. 5, p. 303, 1960.

[26] P. P. Sorokin and M. J. Stevenson, "Stimulated infrared emission from trivalent uranium," *Phys. Rev. Lett.*, vol. 5, p. 557, 1960.

[27] P. P. Sorokin and M. J. Stevenson, "Solid-state optical maser using divalent samarium in calcium fluoride," *IBM J. Res. Develop.*, vol. 5, p. 56, 1961.

[28] A. L. Schawlow and G. E. Devlin, "Simultaneous optical maser action in two ruby satellit lines," *Phys. Rev. Lett.*, vol. 6, p. 96, 1961.

[29] I. Wieder and L. R. Sarles, "Stimulated optical emission from exchange-coupled Ions of Cr^{+++} in Al_2O_3," *Phys. Rev. Lett.*, vol. 6, p. 95, 1961.

[30] A. Javan, W. R. Bennett, Jr., and D. R. Herriott, "Population inversion and continuous optical maser oscillation in a gas discharge containing a He-Ne mixture," *Phys. Rev. Lett.*, vol. 6, p. 106, 1961.

[31] A. D. White and J. D. Rigden, "Continuous gas maser operation in the visible," *Proc. I.R.E.*, vol. 50, p. 1697, 1962.

NEVER TOO LATE - Communication With Autistic Adults

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ABSTRACT

Two years ago, at the age of 27, our son was essentially without speech and his only means of communication was with gestures. As will be described, he is now able to communicate anything he wants to say, by using a keyboard device.

INTRODUCTION

When our son Artie was small, he displayed the symptoms we now know as autism. But even the name "autism" was only about a decade old then, and few people knew it. Even if they did, there was no effective treatment and no school classes for him. We sought help everywhere, and often we thought that some time in the future something might be discovered or we would find someone with ideas that would have helped him if we had known it in time. But we made a conscious decision that we would at all times do the best we knew, and not regret later what we did not know. We kept on searching, and trying, and at last he has made progress at an age when many parents are no longer struggling. After all, most of us keep learning throughout our lives. Art was 55 when he bought his first microcomputer (an Apple I!). So we are happy to report that it isn't too late, and our son in his mid twenties has learned how to communicate.

CHILDHOOD AND ADOLESCENCE

He was very withdrawn and unreachable. From age eight he lived at Clear Water Ranch, and from about 10 to 14, with Mrs. Grace Turner in a 6-boy group home in Cloverdale. Those were relatively good years for him and he learned some living skills. Mrs. Turner brought him to Stanford for some trials on Dr. Colby's talking typewriter, but he did not take to it.

At adolescence, people became afraid of him, because he was big and strong and would have occasional tantrums, even though he never hit anyone. Eventually we could find no place that would take him but the state hospital, which promised various training and vocational programs.

But the promised programs somehow never materialized. Instead, they drugged him with antipsychotics despite our vigorous protests. We kept in close touch, with weekly visits, but he was too doped to do anything. Eventually, when the adverse side effects of these drugs became apparent, we obtained a court order of conservatorship, which specified our right to approve his medical treatment, and the drugs were stopped. Years had been wasted, and his behavior had deteriorated somewhat in the chaotic and violent conditions of the hospital. But he was now much warmer and responsive, and wanted to be with us.

SIGN LANGUAGE

A behavioral specialist suggested that we try teaching him sign language, and we hired a sign language teacher to go in and work with him at the hospital. He would sit at a table with her, and make signs for such things as candies and cookies, and would receive a little of the named food when he made the right sign. He also learned a few other signs, such as bed and eat. In a way, this was a failure because few of the staff knew sign language, so that he had really no opportunity to use it for any practical purpose. In another sense it was very important, because for the first time he would sit and work with someone who was trying to teach him.

HIS FIRST TEACHER

We also found a very good teacher, experienced with autistic children, who was able to get him to recognize some letters on cards. She had Artie make signs for words printed on cards. It was suprising to us that he could do it at all, but sometimes could do it well. At other times he would just go through his repertoire of different signs. The teacher also found that he could even do beginning arithmetic by picking a number card for the sum of two small numbers, e.g. 2+3=5. Unfortunately, she soon got a better job and was not longer available. We tried other teachers, but he made no further progress with them at all. He was glad to see them, and willing to sit with them, but he was content to repeat the same simple things time after time.

FINDING THE COMMUNICATOR

In December of 1981 we visited Stockholm for the Nobel presentation. We met there with Karin Stensland Junker. She is the mother of an autistic girl, whom she has told about in the well known book **Child in a Glass Ball.** She is also a clinical psychologist. Dr. Junker told us about a young man, age 24, who had been brought to her office. He looked typically autistic, and just sat there rocking and apparently ignoring everyone. Yet he had learned to communicate with a little device that looked like a calculator. It had an alphabetical keyboard, and printed the

letters on a paper tape as they were typed. She asked him, for instance, "May I have some of your tapes?" He replied, in Swedish of course, "No." "Why not," she asked. "You can't read it when the sun shines" was his reply. This device uses thermal printing, and the tapes do fade quickly in sunlight.

This was exciting. How could Artie tell us something like that, even if he understood it? He had no way. We must get one of these devices and try it with him! But we didn't even know its name or who made it. Eventually we found that it was a CANON COMMUNICATOR, and that there was a distributor in Palo Alto, a mile or so from our house (2). We bought one, but it was a total failure. He would hit just a few keys over and over, like X X X X Z Z Z Z, and would not type words. So we put it aside, and tried other things.

ALPHABET CARDS

Each had a cutout into which the letter would fit. We would say the name of the letter and the word illustrating it, as he put each one into the card.

TOUCH AND TELL

This device (3), made by Texas Instruments, uses a voice synthesizer to ask things like "Show me the red letter G," or "Show me the small letter r." We found that he knew the names of all the letters, lower case as well as capitals. He could even answer when the voice asked for "the letter for octopus," etc.

LOTTO CARDS

We had him match pictures to pictures, and then pictures to words. We would print words on the back of cards, and have him match the word to a picture, or even give us a word on request. Another way was to have him put the words on the right picture on the board.

MAKING UP WORDS FROM LETTERS

We tried scrambling letters to arrange into a word, and he was able to spell his name and some other familiar words that way. Then we had him pick letters out of a box to spell a word.

READING BOOK

We obtained a child's book, the story of The Three Bears. Aurelia told him that she had learned to read by underlining a word, wherever it appeared. "Let's under line the word <u>bear</u>, wherever we see it." He was able to do that right away. Soon, we found that he could pick out any word on a page of that book. Even more surprisingly, he could pick out words from a magazine page, even one as difficult as the **New Yorker.**

LETTER FROM THE RONNLUNDS.

We had obtained the address of the parents of the Swedish boy who used the communicator, and had written to them asking how they had taught him to read. Their letter described what they had done, and we were much encouraged to learn that the way we were working with Artie was rather similar to the way they had taught their son. Their letter is reprinted in our article (1).

EPSON COMPUTER

At that time, Artie was living in a state hospital, and we could only work with him on visits. Conditions at the hospital were such that it was not practical to set up a computer and leave it there. However, the EPSON HX-20 computer was introduced, and it was battery operated and hardly bigger than a notebook. We found a way to program it in such a way as to induce him to hit just one key on a keyboard. A short program in the BASIC language would display on the screen a word, chosen randomly from a list in the program. A dash was shown under each letter of the word. Then nothing would happen unless he hit the right key to match the next letter of the word displayed on the screen. When the word was complete, it was printed out on the strip printer built into the computer. The first time we tried it, he did it eagerly for more than an hour until the tape ran out, and he stuffed the printer tapes into his pocket.

We tried to get him to use the computer to select an alternative, e.g. steak or pizza, but without much success.

COMMUNICATION BOARDS

By this time it was apparent that he knew the alphabet, and could recognize and spell some words. But he was not yet using words for communication. An article about our work up to that point (1) was submitted in August 1983, and concluded with "We are excited that our son is not too old to learn and is making progress The spark is there. Can we learn to fan it into flame, will it grow by itself, or will it fade away again?" But a few weeks later, progress was so dramatic that we had to write a postscript to the article.

USING WORDS TO COMMUNICATE

During the summer of 1983, a speech pathologist introduced us to communication boards. These were cards or even sheets of paper, on which words were printed. He could answer a question or choose something by pointing to the appropriate word. With a communication board, he could make a choice of foods, activities, or tools and materials for those activities.

The speech therapist thought that he might require pictures on the board, but this was not at all needed.

Around the same time, we made word cards for foods, then colors, etc, and had him put the cards on the object named. This, and the communication board, gave him practice in recognizing words, and gave him an opportunity to make choices by using words.

VOCAID

The VOCAID (3) is another Texas Instruments device, similar in construction to the TOUCH AND TELL. However, it is programmed for people who have lost their voice and need to transmit essential messages, like "I AM COLD." Artie was able to use this immediately, and surprised us by stringing together two phrases to make a sentence. For instance, once he told us "I AM...UPSET." However, the range of choices is limited and he did not use it much. When he moved to a group home, at first the VOCAID was useful to let the staff know when he was upset or needed something.

RETURN TO THE COMMUNICATOR

Most of these activities took place on outings from the hospital to a park. Afterwards we would finish with pizza and then ice cream. Art would get cheese pizza, because that was what he preferred. But since Artie was now making choices, we could let him choose what kind of pizza he wanted, by pointing to one of the words printed on a sheet of paper. Indeed, he unhesitatingly preferred sausage pizza. Then we had him confirm his choice by reproducing the chosen word on the COMMUNICATOR. He was willing to do this.

A few weeks later, one of these outings was finishing with ice cream at a small shopping center. Then Artie waved his hand in a way that indicated vaguely he wanted something in a particular direction. So, instead of guessing, we took a chance and said "Come on out to the car and tell us what you want, on the COMMUNICATOR." We didn't really expect that to work, but he typed "SHOES," and indeed there was a shoe store there. So we took him in and bought shoes for him.

AURELIA'S VISIT TO ARTIE on the following Monday.

Two days later, Aurelia visited Artie, and went through his usual activities on the grounds of the hospital. But this time, she had him make his choices of activities by typing on the COMMUNICATOR, rather than by the communication board. Of course, these were all words he had seen often. But at the end, she asked him where he would like to go for a snack. He replied "GO TO MACDONALDS." "What would you like to eat there," she asked. "HAMBURGER..COKE..ICE CREAM" were soon forthcoming. After that, he wanted "PIZZA WHEEL" (a nearby pizza parlor), and "SAUSAGE PIZZA."

This was really working, and so he asked for and got a visit to a steak house for steak. It was getting late then, and she wanted to end the visit, but he typed "STAY YOUR TIME WITH ME." Then he added, not just once but three times, "I WANT TO GO HOME." So, although we were not ready for a visit, he came home, and remained for three weeks while arrangements were completed for a placement in a new group home.

THE STAY AT OUR HOUSE

We tried to see whether Artie would use the COMMUNICATOR independently, and sometimes he would if he was especially eager. Most of the time, he wanted a hand on his for reassurance and help. We decided that getting his communications was more important at that stage than forcing him to be independent, and indeed he still wants a reassuring hand on his. We asked him when did he learn to read. He replied "WHEN I WAS TEN YEARS OLD." "Who taught you?" "GRACE (Turner)." "Why didn't you show it." "TOO HARD." "How is it that you can do it now," Aurelia asked. He smiled sweetly and replied "I LOVE YOU." He told us that he liked cowboy movies. "Why cowboy movies," we asked. "WIN THE WEST" was his answer. We learned that he liked the color red. One evening, he really surprised us by using the communicator to ask for "CHOCOLATE PUDDING."

We found that Artie could print some without the keyboard. We learned this one day when Aurelia wrote down the list of tasks for the day. He likes to do chores around the house, but one day he grabbed the pen and printed VOTE. "Vote?" she asked. "VOTE WHAT WE MAY DO," was his reply. Then he crossed one of the jobs off the list. He does some printing now, but always wants another person's hand steadying the pen.

AT PARADISE SCHOOL

At first, many of the staff did not believe in the Communicator, and did not use it with him. If someone tried half-heartedly, he would resist and conceal by just typing Z Z Z Y X X X X, etc. We persuaded some of them to have him choose morsels of different foods with it, and they were able to do that, but only a few of them advanced any further. Now, however he does communicate fairly freely with several staff members, and with the teacher who works with him twice a week. Sometimes he has used the COMMUNICATOR to initiate things like "MAKE COOKIES."

We visit him nearly every week, and he tells Aurelia a lot of things. We had noticed that the mother of the Swedish boy said that he never asks questions. So, one night at dinner, Aurelia asked Artie if he would like to ask any questions. His reply was a question: "WHY SHOULD I TYPE QUESTIONS?" "Because there might be something you would like to know," she said. His next question was "DO YOU LOVE ME?" On being asssured that she did, he asked "WHY CAN'T I LIVE AT HOME?" "Because there are things

you need to learn here," she said. Then he asked "WHY AM I NOT LIKE HELEN (his sister)?" Again thinking quickly, Aurelia replied "Helen has a job." "I CAN GET A JOB," he said. "You can when you have learned some more," he was told. Since that time, he has often told Aurelia of his feelings, worries, and hopes. He communicates that he very much wants to be normal, and indeed anything that makes him feel more like a normal person is a strong motivator.

ARITHMETIC

Art also programmed the EPSON computer to ask Artie questions about addition, then multiplication and division. We found that he knew the multiplication tables and could add, subtract, and divide simple numbers.

READING COMPREHENSION

We asked him questions about text on a page after reading it silently. We found he could read even faster than we could, and we are fast readers. He must have been catching things at a glance all these years, when he never seemed to read anything.

THE TEACHER

Artie has been gradually revealing so many things he knew, that we sometimes wonder whether he is really learning anything new. However, we have found a teacher, not experienced but persistent, observant and resourceful. At first, she ~tarted on addition and he typed answers on the COMMUNICATOR. After a few lessons, he gave some answers to questions, in words and sentences.

She started on multiplication with flash cards for 2X2, etc, and he seemed happy to work on that level. We told her that he already knows the multiplication tables. The next week she said, "You're right, he does." He told her that he did not know about carrying in addition, and she taught him that. Subsequently he learned, very quickly, about multiplying large numbers, long division, decimals and fractions, going through a grade level in about 3 months of 2 lessons or less per week. He does some printing with the teacher, mainly for answering questions. However, he has written some letters to us, with a little prompting from the teacher.

OTHER PEOPLE'S RESULTS

Two young men in Ottawa, at the McHugh School (Stanley Tovell, Program Director) have learned to communicate with a SHARP MEMOWRITER, quite independently of our work. They are also using some computer programs and one of them is doing reading and arithmetic at a tenth grade level. Keyboards do not work for everyone in their classes.

A 35-year old man, living at Camarillo state hospital who had learned to read earlier but was not communicating. His mother heard about our article, and since then has gotten communication from him with a typewriter. We learned at the NSAC Meeting that he is now using a COMMUNICATOR.

THINGS THAT DID NOT WORK WELL

Texas Instruments Computer TI 99-44

We bought it about two months before Artie came home from the hospital, because it had a very striking program for learning the alphabet. But it was not practical to use at the hospital. Ly the time that he came home, it was clear that he was much past learning the alphabet. We set the microcomputer up at his new group home, and Artie did do some of the arithmetic programs with one of the staff members there. He was also able to handle the reading programs at the third grade level or so. The difficulty was that he found them too slow, and did not want to wait for the computer's response.

Voice synthesizers.

We tried both the VOTRAX TYPE 'N TALK and a borrowed voice synthesizer in the $3000 price range, but he resisted them. He was with his mother when Art was trying out the more expensive synthesizer. He told her "That sounds silly. I could learn to talk with one of those, but I want to talk by myself." As he always has, he occasionally says some words, but he is not able to use them at will for communication.

THINGS WE HAVE LEARNED

You have to be interested in what he wants to say. It is important to make each step easy and rewarding. He has a good long attention span, but a short frustration span. His biggest problem seems to be an overwhelming shyness, like stage fright, that makes him afraid to try things and to reveal what he can do. Devices are very helpful, but the close, supportive, personal interaction is essential.

CONCLUSION

Autistic adults can learn, just as normal adults can. It really is never too late. As for how we feel, we can only quote the Bible story of the prodigal son, and say that "our son who was dead is alive again."

NOTES

(1) ***The Endless Search for Help***, by Aurelia T. Schawlow and Arthur L. Schawlow, in **Integrating Hoderately and Severely Handicapped**

Learners: Strategies That Work, edited by Michael F. Brady and Philip Gunter, Charles Thomas Publishing Company, 1985.

(2) CANON COMMUNICATOR is distributed by CANON U.S.A., and can be obtained from medical supply companies.

(3) TOUCH 'N TELL and VOCAID are products of Texas Instruments. TOUCH 'N TELL is found in toy stores, and VOCAID in medical supply stores.

INTEGRATING MODERATELY AND SEVERELY HANDICAPPED LEARNERS

Strategies that Work

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CHAPTER 1*

OUR SON: THE ENDLESS SEARCH FOR HELP

AURELIA T. SCHAWLOW AND ARTHUR L. SCHAWLOW

Our son was born on March 21, 1956. That was about two weeks later than expected, and more than two feet of snow had fallen in New Jersey over the preceding weekend. There were some anxious hours, but eventually the snow did end and roads were cleared in time for the trip to the hospital. The birth was long and difficult, and was complicated by a deep transverse arrest that the doctor, busy with another patient, overlooked for several hours. But our son was so beautiful and lively; our first child! We named him Arthur (after his father and paternal grandfather) Keith. We have always called him Artie.

At the time, Art was a research physicist at Bell Telephone Laboratories in Murray Hill, New Jersey, and Aurelia was choir director of the First Baptist Church in Morristown. We had a new house in Madison, and Artie was the center of our life. Before long, he was joined by two sisters, Helen, born in July, 1957, and Edith, born in November, 1959. Artie seemed to us a perfect baby, ahead of the books' predictions in physical development like turning over, sitting up, standing and walking. Yet, if we had been more experienced parents, we might have known that he was neither as demanding nor as responsive as most children. He was quick to learn things by himself, but not at all easy to teach.

Around the age of one, he began to say a few words, but then he stopped. He gradually became more withdrawn and often seemed content to amuse himself by listening to music or playing with toys.

Becoming concerned, we began what was to become an endless search for help. An old friend, a distinguished European-born neurologist, told us that our son had "a mathematician's personality" and would eventually start to talk. At that time, New Jersey had no medical school. Even though we lived in an affluent suburban area, there were few specialists familiar with the more unusual childhood conditions. We did find a pediatric neurologist

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who, although extremely busy, eventually did examine our son. She decided that he had petit mal epilepsy and prescribed a drug for that. Almost immediately, it was apparent to us that the drug was not helping, and that he was becoming even more withdrawn. Moreover, he became incontinent, so that he was no longer "acceptable" in a nursery school. In addition, there were occasional episodes when his face would suddenly turn purple, and he would have a far away look. We tried to get the doctor to see what was happening, but we could not even get our telephone calls returned much of the time. This was the first of several times that doctors blithely prescribed drugs and then refused to recognize harmful effects that were immediately evident to us. Another such incident occurred when Artie was about seven years old. A neurologist prescribed heavy doses of an amphetamine. We begged him to monitor the effects closely, but it was the same story. Perhaps he felt we must give the drug enough time to act. We think that he was looking for some kind of anomalous reaction of the drug, but it was quickly and painfully evident that the drug was doing just what amphetamines usually do. Artie lost his appetite, and would only eat a very few things. His stereotypic behavior became even more persistent. Then, too, he was awake until 1:00 a.m. night after night.

The Search for Services

By 1961, when he was five years old, it was apparent that there was no help to be had for him in our area of New Jersey, not even an appropriate school or day care program. Thus, when Art received an offer of a professorship from Stanford University, and we learned that parents there had set up a school for children like Artie, we decided to move. One of Art's colleagues in the physics department had a daughter who was similarly withdrawn and nonverbal, and his wife had been a leader in establishing the school.

The first year at Stanford was a good year for Artie, and the school seemed to be something he enjoyed. But then the director of the school left for a better position, and things did not go well for our son under her replacement. Artie was not willing to participate in the group activities and would wander off by himself more often. We sought help from neurologists, which led us to the amphetamine incident recounted earlier, and from psychiatrists. The psychiatrists, although they would probably not be classed strictly as psychoanalysts, were influenced by that school of thought. Their approach was just to try to get us, the parents, to search our souls to find what terrible things we were doing wrong. Not only is this approach useless, but it is also destructive, because it makes the parents less, rather than more, able to cope with the difficult behavior of the autistic child. Also, that approach immediately puts a gap between the parent and psychiatrist. If the

psychiatrist insists on treating the parents as his patients, he cannot work with them to help the child who is, after all, the person who needs help.

By the time Artie was eight years old, it was apparent that we could not provide the teaching and companionship of other children that he needed. The public schools had nothing at all for him in those days. But we did find a residential setting, Clearwater Ranch Children's Home, which seemed as if it could provide a good environment for him in a rural area. We placed Artie in the home and visited him there weekly. He came home for occasional visits. After a year or so, he was moved to the Clearwater Ranch Town House, in the small town of Cloverdale. There were six boys in the house, and it was run by a wonderful lady, Mrs. Grace Turner. She had spotted Artie at the ranch and asked for him because he had reminded her of a boy who had started to talk while in her house. Artie learned many things there and those were, on the whole, good years for him in which he gradually became less withdrawn. He did not really talk satisfactorily then, but he sometimes said phrases or sentences. Artie became able to do a number of household chores and took pleasure in performing them.

However, at adolescence, several things took a turn for the worse. Mrs. Turner was not able to continue with the Town House. Artie grew big enough so that he frightened some of the teenage girls who worked on the staff. Even though at that time he never harmed staff or other residents, the staff became particularly nervous when he began to have occasional tantrums. Eventually, they decided that Artie was too big to manage, and we had to find another place for him.

We found another ranch and he moved there. He was immersed in a larger group and apparently did not get enough individual attention because he became more isolated and withdrawn. When he began tearing up clothes and sheets, this home decided that they could not manage him. However, he still had never attacked nor hit anybody.

Hospitals and Teachers

In desparation, we put Artie into a state hospital. It was in a convenient location, 17 miles from our home, and had spacious grounds. We were told about their various programs and workshops, and it looked like a reasonable choice. Besides, we had no alternatives, because there were then few group homes for autistic adolescents and adults.

The hospital proved to be far worse than we feared initially. Somehow, the classes never materialized, or would get cancelled after one or two sessions because the person in charge was needed elsewhere, or for some other reason, or for no reason. Worse than that, the hospital relied on massive doses of antipsychotic drugs of the phenothiazine group (e.g.,

Mellaril or Thorazine) for behavior control. They insisted on drugging Artie until he looked like a zombie. I don't think they had any idea of Artie's abilities; under those drugs he looked and acted really stupid and half asleep. The wards were quite chaotic with a lot of boys who would frequently hit others. We brought him home for visits almost every weekend in order to take him swimming, but under the drugs, it was difficult to teach him.

We continually argued with the doctors and with the hospital authorities to reduce or eliminate the drugs. We were never told that the drugs were good for Artie, but rather that the staff wanted them, or that he needed a tranquilizer for such a violent environment. Not only did the drugs affect his behavior, but physical side effects became evident. Thorazine made his skin very sensitive to sunburn, and it was constantly irritated. Mellaril made it nearly impossible for him to swallow. He still has a habit of holding saliva in his mouth that originated when Mellaril prevented him from swallowing.

Eventually, we learned of the very great danger of tardive dyskinesia, an irreversible condition that often follows prolonged treatment with these drugs. Sometimes the authorities would yield a bit to our entreaties and reduce the drugs; but if they had any trouble with his behavior, a higher dose would again be administered. His behavior did become rougher, a matter of sheer survival in that environment. At last, we found a newspaper report of a court decision that drugs could not be given to patients without their consent. When we showed that to the director of the hospital, her staff studied it and told us that we could not speak for Artie on that because we were not his guardians. We then went to the expense of getting a court order of limited conservatorship which specifically set forth our right to control his medical treatment. The hospital administration was furious, and tried several times to get rid of us and Artie. But somehow, once the drugs were gone, we found in Artie a warm and loving person, who clearly wanted attention and was open to learning. What a contrast from the remote and unreachable child we had known.

We, of course, wanted Artie out of the hospital. We tried several times keeping him at home for extended periods. However, we could not find any appropriate program for him in our community, and it was too much for us to supervise him all day, every day. Since we couldn't manage to keep him occupied at home, we tried seeing what a teacher might be able to do at the hospital. A teacher who had some experience as a sign language instructor, but was working on a college degree to acquire credentials, began visiting Artie at the hospital to try teaching him sign language. It was part of the plan from the beginning that signing might also help him to relax enough to produce some speech. She started with food words like candy, cookie, and apple. The correct sign, or an approximation, was rewarded by a bit of that delicacy. Somewhat to our surprise, he was happy to sit and work for an

hour or so at a time on various signs. Although it did not seem easy for him, he did learn a number of signs including those for some articles of clothing and some familiar objects, and he made sounds with some of them. Most striking was the word "bed" which he would say clearly while making that sign. Sometimes surprising things came out, like "peanut butter." Apparently, the effort of making the sign helped to overcome his inhibition against speech. But neither then nor subsequently has he been in a situation where he could really use those signs for communication, and so he rarely does.

What really seemed important was that he had acquired a taste for working at a table with a teacher for extended periods. While he was still working on the signs, we were able to hire an inspired teacher, Mrs. Joanne Glass, who began to work on recognizing letters, words, and simple arithmetic. Progress was exciting, but after a few months, she moved to a better job which did not leave time for working with our son. We hired students who kept up the human contact but did not have the skills to make much progress.

Learning and the Surprise of Technology

Meanwhile, we continued to visit our son every week, and we kept up these visits even later when he moved to a residential home 65 miles away. In many ways, this house and the accompanying day program were great improvements over the hospital; however, the emphasis was on controlling behavior and, as far as we could tell, they did not take seriously the possibility of academic learning. Many of these things we did on our visits were not academic either, but we did use letter cards every week. He learned to recognize letter shapes by inserting them into the cards from which they had been stamped. Each time he inserted one, we would repeat the name of the letter and usually the word which accompanied it on the card (e.g., "L for lion"). However, we only began to realize what he had learned when we bought a Texas Instruments Touch and Tell™. This device produces a synthesized voice that asks things like "Where is the red letter Q?" or "Can you find the small letter X?" Correct answers are given by pressing the appropriate letter on the device and are rewarded by a few notes of a tune and by the voice saying cheerily, "You found the small letter X." After a slightly timid start, it was soon evident that Artie knew all of the letters of the alphabet by name, upper, and lower case. He also had no difficulty when the synthesized voice asked him to "Show me the letter for king," or some other word. Artie had learned the alphabet.

So he knew the alphabet, but how much did he really know about words? There are various packages of Word Lotto available with cards which bear pictures of familiar objects and their names, such as "umbrella" or "shoe."

He could easily pick out any card asked for by name and match it with the right picture on the large card. Aurelia then printed the words on the backs of the small cards and asked him for the cards by name. The first week he succeeded in identifying the six cards presented to him and the next week. 12 more. By then it was apparent that he could recognize any card she presented and was not just memorizing them at that time.

Next, Aurelia tried reading a beginning level story book with him. It was a simple version of the old story about the three bears. Then she told him. "I taught myself to read by underlining words whenever I could find them. Let's see if you can find the word 'bear' and underline it." He was able to do that right away, and it was soon apparent that he could recognize any word. not just a list of words that he had been taught. He really had developed the idea of how to read.

But how could he communicate what he was thinking? We were given a most exciting clue during our trip to Stockholm in December 1981. There we met Dr. Karin Stensland Junker, who is a renowned authority on autism and author of the book *Child in the Glass Ball.* This book is about her own autistic daughter. Dr. Junker told us that a young man, 24 years old. had been brought to her office a few months earlier. He appeared very withdrawn and typically autistic, and sat seeming not to notice what was going on around him. Yet he had learned to communicate by using a calculator-like device that printed out words on a paper tape. For instance, she asked him, "May I have some of your tapes?" He replied (in Swedish). "No." "Why not?" she asked. "Because it's no good when the sun shines" he said. The device uses a thermal printer, and the tapes fade when exposed to sunlight. Mats, as his family called the young man, understood that the tapes faded; now, at last, he could express it. How marvelous! Just imagine how frustrated he must have felt when he could not express his thoughts at all.

We found out some months later that this device is a Canon Communicator™, which is sold in this country for about $600 by Telesensory Systems near our home in Palo Alto. We bought one, but our son would only type words if we guided his hand and mostly seemed to want to bang the keys randomly. Meanwhile, we were trying some of the other things described earlier. We got the address of the Swedish boy's parents and eventually received a letter telling how they had taught their son to read and type. We were excited because we found many similarities between what they had done and what we were trying.

The Epson HX-20™ portable microcomputer became available late in 1982, with a full-size keyboard, a liquid crystal display, and a small printer. We wrote a program for this computer which would present either single letters or words on the screen with a series of dashes underneath, like:

Viola Rönnlund
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17-02 1983

Mr. A. Schawlow
Stanford University
Stanford, California 94305
U S A

Dear Mr. Schawlow,
Thank you for your letter of October of last year and I apologize for the delay in replying. It has taken a while to compile everything and then to get it translated to English. I hope it will be of help to you.

Here are some notes on Mats and how he learned to read and write with the aid of a typewriter and later to use his communicator. Mats's sister, who is a teacher, and I, his mother, started to teach Mats a kind of unit reading. That was as early as 1967. We were only able to teach him during his summer vacations and every second or third week when he came home to see us.

We tried to make him point at pictures representing words which belonged to phrases he was to read later. We asked him about each picture and he seemed to understand us and pointed at the right pictures.

The next step was to give him words. First he combined two-letter, and then three-letter words with the pictures. For the most part, he managed to match the right words with the pictures.

Mats lived at home between 1970 and 1975 and we continued the way we had started - combining words with pictures. Gradually, he managed to put longer words, five-letter words, six-letter words, to the right pictures. We proceeded with short phrases, e.g., "The child is playing," "The rabbit is eating," and these he managed to place with the correct pictures.

After this, Mats started making words from letters. I showed pictures of, for example, a girl, a boy, an animal, a house, a car, or a rose. We had mixed the letters and he chose the right ones to spell out the word suitable to the picture. We kept this up for about a year. Then he started spelling out two-word phrases. These phrases were gradually increased to five words.

Some time later, we started reading books. I read aloud and Mats followed each line with his finger. After a couple of pages, we stopped and I asked him to show me where a certain sentence

appeared in the text. At about this time, I encouraged Mats to practice writing the letters of the alphabet by hand.

Later on we started practicing with an electric typewriter. Mats tried to answer questions from a book for children in second and third grade (8-9 year olds). The questions were, for example, "A lemon is not sweet but _____." Mats had to type out the missing word on the typewriter. Another simple example was to answer a question such as, "this animal you get milk from.' In most cases, he managed to type the correct answer.

We continued the reading - now going one step further - both of us reading quietly to ourselves. After having read three or four pages, I asked who the passage was about and what sort of actions were being carried out. He progressed very well and his spelling was remarkably good considering he had not been practicing difficult words. We continued with this type of reading and are doing so at present. By now we have read a great number of books. Nowadays he uses his communicator to answer our questions. The advantage is that he can always carry the communicator he got three years ago.

Mats has typed several letters to friends and relatives on his electric typewriter. However, I have always to dictate these letters or nothing would be done - he finds it difficult to take the initiative himself. He can also write by hand and he has written some letters in this way, though I must hold my hand lightly on his. Right now I am trying to make him write by hand without any support. Every now and then it works.

When it comes to reading and writing, we are hoping that he will start asking us questions. We feel that this would be a great step in our mutual contact and communication. We so much want him to take an interest in the world around him instead of turning inward into himself, but we do not know how to help him in this matter.

These are some of the methods we have used with Mats and for the most part, we have had very encouraging results. I do hope our efforts can be of help to you and that you will keep in contact with us and let us know if you have any success. We wish you all the good fortune and patience when helping your son.

Kind regards

(signed) Viola and Magnus Rönnlund

B O O K

The program was written to ensure that nothing was entered if he hit several keys at the same time. He had to hit the right key, and no others. Artie could bang at the keys all he wanted, but nothing would happen unless he hit the right letter, first B and then O, O, and K. Each time a correct letter was entered, the computer would emit a brief tone. After the word was finished, the computer would play four notes of music and print the word on the little printer. This worked and Artie enjoyed it, although he still wanted a parent's hand on his to guide him. Sometimes, he would do nearly all of the guiding by himself, but he would rarely do it alone. Later, we wrote another program that displayed and printed larger characters, up to seven on a line rather than the twenty of the computer's regular character set. This was an improvement, but he still wanted a hand on his.

Right now, Artie's primary problem seems to be motivation. He has not seemed to realize that he can communicate with words. We are working on that now. Instead of emphasizing the typing, we are getting him to point at YES or NO or to circle the desired word. He is beginning also to use a communication board to pick out which of his activities, or which food, he wants by pointing to a word on a board. We learned in Orlando, Florida, last year about one teenage autistic boy whose behavior improved dramatically when he was given a communication board to describe his moods. When he came to school, he could point out whether he felt sick, angry, or tired. A communication board is quick and direct, but the range of choices is necessarily limited. However, it does seem to be working for Artie and gives him a verbal way to tell us some things. We hope that his success with it will soon carry over into answering questions by typing a word. Then we will return to the Canon Communicator™ which he can carry with him and which might eventually be used to express any thought.

To teach him to type, we have used the Epson microcomputer, (although it is far from ideal) because we had to use a portable computer. Since he is in the state hospital, which is thoroughly chaotic, we can not set up and use a regular computer. It would, we think, be better to have a large, brightly colored display. We expect that he will move to a group home shortly, and we will be able to experiment a little there with a home computer (e.g., the Texas Instruments 99/4A™) for which programs for preschool and beginning reading are available. The Texas Instrument computer has a voice synthesizer which should be useful. It can talk as well as display words on the screen.

Artie's Future

Clearly, we are very much in the middle of our attempts to help our son learn to communicate. Already he has learned much, as we have learned much

about what he knows and can do. He learns quickly at times and does not require endless repetition. His attention span is not short, and he will work at a task for a long time if he wants to and feels it is worthwhile. On the other hand, he becomes easily frustrated and we need to plan things so that he can quickly succeed at each new step. Anything that helps communicate his thoughts and feelings will help to reduce the frustration that he must feel so often.

Adults normally expect to keep learning new things throughout their lives. For instance, Art began to learn about microcomputers when he was 56 years old. Unfortunately, too often we *assume* severe limitations on the learning potential of developmentally disabled adults. As a result of our experiences with Artie, we believe that in many cases, the only reason they do not learn is that nobody tries to teach them. We are excited that our son is not at all too old to learn and is making progress. He wants to learn and enjoys it. How far he will get we do not know. We are learning something new every week about what he knows and how to get him to reveal it. We are still learning new approaches from other parents and from teachers and hope for more in the future.

The spark is there. Can we learn to fan it into flame, will it grow by itself, or will it fade away again?

Postscript

This chapter, to this point, was finished and submitted in August, 1983. We ended with a question; that question now has an answer. Since September, Artie has been communicating fluently with the Canon Communicator™. He seems able to type out anything he is motivated to communicate, with good spelling and sentence structure. It seems hard to believe, but it has happened.

In mid-August, we began consulting with a speech pathologist, Ms. Brendan Webster. She devised some simple board systems which guaranteed Artie immediate feedback if he communicated by pointing to the word for an activity, material, or snack. He learned that communication by printed word got results. He also learned to use a Texas Instruments VOCAID™ as a talking communication board. Although his vocabulary is limited, he has learned to pair phrases to develop sentences (e.g., "I would like . . . to be left alone.").

Then we reintroduced the keyboard and asked him to type out the word to which he had just pointed. It became clear that he could switch back and forth from the typewriter keyboard to the alphabetically-arranged Canon Communicator™ with no problems at all. Again, using only the choices from his communication boards, we ensured that he got whatever he typed. We still did not know whether he could type the words without copying from the communication boards.

In September we took him out for his usual outing and ended up at an ice

cream store. He has often waved his arm upward in a gesture indicating that he wanted something in a particular direction. This time, he waved in the direction of a shoe store. We told him, "Come out to the car, get the machine, and tell us what you want." Then he took the communicator, and typed "SHOES." We were delighted, and took him in the store and bought him new shoes.

Two days later, Aurelia visited him on the hospital grounds. Instead of using the communication board to choose activities and materials, he typed his choices without copying. At the end of the visit, she said, "Tell me where you would like to go now." He typed, "GO TO MCDONALDS." She then asked, "What would you like to eat there?" and he answered, "HAMBURGER." She asked, "Would you like ice cream?" and he replied by typing, "ICE CREAM." "What would you like to drink?" she asked, and he replied "COKE." He got each item and then he asked for "PIZZA WHEEL" (a nearby pizza parlor), and "SAUSAGE PIZZA." After that he wanted, and received, steak and salad from another nearby place. It was getting late, and time for his mother to go home, but Artie typed, "STAY YOUR TIME WITH ME."

Then he typed, not once but three times, "I WANT TO GO HOME." Although that was not a convenient time for a home visit, she could not deny him and so he came home. It became apparent that he really hated the hospital and was deathly afraid of returning. Fortunately, a few weeks later, we were at last able to complete arrangements for him to move to an excellent new group home. During his three weeks at home, he communicated by typing every day. He now has a surprisingly large vocabulary, quite good spelling, and good grammar.

So far, Artie communicates mainly with his mother, and primarily about matters that interest him but are not too deeply emotional. However, he has told us that he has been able to read "SINCE I WAS TEN," and that he had not shown it before because it was "TOO HARD." His mother asked, "How is it that you can do it now?" He gave her a sweet smile and typed "I LOVE YOU."

After a few hours in the new group home, he typed "I LIKE IT HERE." He is beginning to communicate with the staff members there, and we hope that before long, he will do it without anyone's hand on his. So far, he wants the reassurance and occasional guidance of a familiar hand.

From these experiences, we are even more convinced that our son does not need endless repetitions to acquire a new skill. He needs, rather, to understand what is wanted and to gain confidence that he can do it. Usually this requires finding easy steps and motivation. Given those, progress is rapid.

Most of all, we have learned that he knows much more than he has been able to communicate and that he can learn more. We are more than ever convinced that the main reason many autistic adults appear unable to learn is because so few people try to reach them and to teach them.

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