The Optical Society
Oral History Project
Interview with Anthony Siegman
Conducted on May 5, 2008, by Lee Sullivan

LS: This is an oral history interview being conducted on Monday May 5th, 2008, between Dr. Anthony Siegman and Lee Sullivan for the Optical Society of America. Dr. Siegman, thank you for taking the time to sit down and talk to us today. I wanted to start out by asking you a little bit about your background in education and how you first became interested in science and physics.

AS: Okay. Well, I was born in Detroit in 1931 and brought up in rural Michigan about thirty miles or so north, between Pontiac and Flint. My parents were both originally from Wisconsin, from essentially farm families. My mother had been a school teacher, my father was the controller for a small ice and fuel company in Detroit. I went to the local schools, county schools, and then I went to an all-boys high school called Catholic Central High School in Detroit, which meant driving in with my father an hour or so each day and back out. And I don't know that I had a large interest in science or any early “a ha” moment about science at that time. In fact, I think in comparison to what I've seen in some of these oral histories from others where people are making deliberate choices as they go along, my feeling looking back on my life is that I've been very, very fortunate, and a lot of lucky things have happened to me, mostly through other people somehow identifying my talents or something and pointing me in the right direction.

And maybe the first of those lucky events, and what eventually at least led into my career in optics on was the fact that sometime in the late 1940s, Harvard College decided that it wanted to institute a new program called the National Scholars Program. And just for a little historical background here, at that time, which I had no knowledge of Harvard or no awareness of as a place to go to school. That was a very different era for people picking colleges. But Harvard at that time drew its students heavily from the old Boston aristocracy, Cabots and Lowells and so on, and the prep school milieu around New England and maybe a few high schools in Boston, and the Jewish and other communities in New York and on the East Coast.

Under the leadership of James Bryant Conant, who was a pretty well-known guy at that time, they decided they wanted to broaden their national and also their international visibility, and so they started a program in which they gave two sizable scholarships in each state. In each of the fifty states, they would pick two national scholars. And somehow, my high school got me to apply for that, and also for a scholarship at Notre Dame, which I think being a Catholic high school they would've preferred that I go to. And somehow, through miracles that I've no conception of actually, I won one of the two from Michigan. So I went off to Harvard as an undergraduate, and as I was preparing for this interview, I realized I actually have absolutely no memory of how I got from Michigan to Harvard in 1949. It certainly wouldn't have been an airplane. So anyway, I arrived there, and I guess I'd always been a fairly bookish and scholarly type, although I had played some neighborhood baseball and done things like this as a child.

At that time at Harvard, you took four courses each semester. And if you took four courses a semester for eight semesters, thirty-two courses, you got a degree. There were distribution and other requirements. And for whatever reason—it was certainly not deliberate planning—I started taking five courses, which was uncommon but not totally unknown. And I didn't have any problem with the academic work. I went back home the first summer and worked in a local gas station, or maybe it was the local grocery store. And sometime in the second year, I realized that going at this rate, at the end of three years, I would be just two courses short of graduating. And so I guess partly as a matter of economy and just because it looked like an easy thing to do, I stayed in Cambridge for the summer between what would've been sophomore and junior years and took two extra courses, and graduated at the end of the third year. And, again much to my surprise at the time as I hadn't even been thinking about this, I learned I had graduated *summa cum laude* in three years flat.

In doing this, I had more or less by accident, majored in a mixture of science and engineering because I was interested in technical things. I had had an outboard motor, an elderly outboard motor that I'd repaired and made run when I was in high school. We lived on a little inland lake in rural Michigan, and things like that. Harvard did not have an engineering major. They had something called the Engineering Science and Applied Physics Program in the physics department. ESAP it was called at the time. And that was what I had majored in. And so I got a job offer at that point from Hughes Research Labs. And once again, just in the year I graduated, 1952, Hughes had started a large cooperative fellowship program. At least, I think it was the first year of that program.

Under this program, they brought in something like two hundred bachelors degree students, who worked, what was it, twenty hours a week, I think, and took classes half time at UCLA, in my case, or USC. And in two years you would end up with a master's degree. And so I went to Hughes. This was in Culver City, Los Angeles, and again, I suppose because of my good academic record and so on, I and one other guy were put in the microwave tube laboratory, which was the most "researchy" area of the Hughes Research Labs at that time. You have to remember, this was just a few years after World War II, there had been all this immense development in radar in microwaves, and so on, in the MIT Radiation Laboratory.

Physicists, as you may know, from all over the country had been brought not only to places like Los Alamos and so on, but also to—in particular, the MIT Rad Lab, the Harvard Radio Research Laboratory, and elsewhere to do new things in microwaves and radar, which were emerging. Some of them had come, in fact, from Stanford University where I ended up, but which again I knew essentially nothing about at that time. And those wartime activities form a lot of the intellectual history or the intellectual background of the eventual development of lasers and optics. This is a theme that I've talked about in giving talks on the history of how the maser and the laser came to be. For example, Charles Townes, who made the first maser in 1954 and became a Nobel laureate, had worked on radar during World War II as a younger scientist, had worked for Bell Laboratories, and actually been flown around in military planes off the coast of Florida testing radar ideas.

If you want, we can get back to that later, but I ended up at Hughes working on something called the traveling wave tube, which was a microwave tube with a very, very large bandwidth. This was beautiful for microwave relay links, which may have existed at that time, but they were certainly in their infancy. And Bell Labs was of course, very interested in running microwave links to replace obsolete telephone lines across the country and across the world. These tubes were also of interest to go into satellites and for military radars and so on. So Hughes, at that time, was one of the centers of research in microwaves, and specifically microwave tubes. Bell Labs, Stanford, Hughes was among one of the top three or four major centers, and maybe GE and a few other places, RCA.

So I was put to work on very early traveling wave tubes with one of the good youngish researchers at Hughes, a man named Dean A. Watkins: Dr. Dean A. Watkins. And to put in some things that lead up to the modern OSA, you have to realize that at that time there were no fast publication of letters and journals. To publish something, there was nine months incubation between when you sent it in, it got reviewed, it got typeset and all this sort of stuff. There was no fast publication. And so in this field of microwave tubes, which was very exciting and very interesting, there was, first of all, a lot of personal communications with the Bell Laboratories, which was then, of course, and it was for a long time after, a premier research place.

The major communication medium for that field, or at least the thing that all the leading people in the field regarded as their first line of communicating with their peers at that time, was something called the "Tube Conference". It was an annual conference. I think it was formally called the Conference on Electron Device Research. It might have been going on for ten years at that time. It was held always, on a university campus with a fairly fixed program, sometime in the late summer or early autumn. It was more or less by invitation only, which the societies didn't like, but the people who were involved in it did this. And people at this meeting would really talk openly even with competitors present about their latest work, but it was understood no photographing of slides, nothing was published. Anything you heard there was not to be cited if you later wrote a paper. It was rally off the record.

What's relevant here is that I think it was probably within a few weeks or within a few months, let's say—after I arrived at Hughes and was assigned to start making measurements on traveling wave tubes and so on, that this meeting was held. It was actually held on the Stanford campus. And the other honors co-op fellow in the tube labs, a man named Orion Itoch, who eventually went on into a career in industry and not in optics. He and I were both brought along to this meeting. Incidentally, I think there were a number of other later notables in optics, Ivan Kaminow for one, I think was another one of those honors co-op fellow that first year. And it may be that Tom Everhart, who was there also―Tom I knew as an undergraduate at Harvard. I'm not sure he was at Hughes. At any rate, we were taken to this meeting. And it was just one session at the time and all very open and so on. A large number of Bell Laboratories notables who later became important people in optics and especially in lasers I would have met, if not at that first meeting, then at later Tube Conferences, since I went to that meeting every year for the next something like ten years. But some names that optics people will know, like Herman Haus and Art Ashkin and Rudy Kimpfner and John R. Pierce, who was the guru of a lot of developments at Bell Laboratories, would have been there. I may think of other names later, but just a lot of other people who then became, ten years later, close colleagues in optics and leading people in the field of optics.

So that series of meetings would have been an immense stimulus if you like, that series of meetings. It no longer exists. It faded away with the development of fast publishing journals and this kind of thing, the need went away. So anyway, I worked for a year and a half on microwave tubes at Hughes. And a great many of the intellectual ideas that were later to be very important in lasers, technical terms like coupled modes and gain and linear amplification and oscillation, a lot of those ideas came straight from the microwave tube world. Lasers, if you like, are essentially microwave tubes or at least oscillators and amplifiers at enormously higher frequency. So that was the intellectual equipment one needed to understand and to make contributions in lasers.

LS: So here you are at Hughes Research Labs—

AS: Yes.

LS: —with your bachelor's degree in engineering.

AS: I had a bachelor's degree, it's actually an A.B. degree, because that was all Harvard gave at the time, but it was more or less a degree in engineering. And I was getting a master's degree, taking courses at UCLA.

LS: In applied physics.

AS: Yes. Again, that program was basically in the Physics Department, but it was a master's in Applied Physics, so okay.

LS: And do you realize at this point that the area you're beginning to focus on is optics?

AS: No. That evolves in the next five years, over during the next three or four years.

LS: Why don't we talk about that period a little bit?

AS: Okay. So the man I was working for at Hughes, Dean Watkins, after a year or so, left to go to a faculty position at Stanford. And I worked for another guy who subsequently became well-known, although not in optics, Dick Johnson, H. Richard Johnson. And then Dean Watkins invited both of us, asks both of the two of us on the fellowship program in that lab, when we finish the master's degrees, to come to Stanford as graduate students, as Ph.D. students in his group; and we both do. I think that I, again, finished the two-year master's degree in a year and a half or something, and so I went up and became a graduate student in Watkins' group in 1954. Watkins subsequently had another graduate student named Kumar Patel, whose name will ring a bell. You probably will interview or somebody will interview Kumar Patel, who invented the CO_2 laser. And he has a different take on Watkins. In any case, I had a perfectly satisfactory experience. I worked on noise in electron beams, and that was my thesis topic. I do early numerical calculations on the first mainframe I think that Stanford ever had—an IBM 650 maybe? I punched cards and programmed in assembly language, and printed things out on immense green bar papers and also attempted an experiment which didn't actually ever turn out well, but the theoretical numerical calculations came out well. And so again, perhaps a year or year and a half after I came to Stanford in 1954, Watkins then moved on from Stanford to start what ultimately becomes a successful company, Watkins Johnson Company, that made microwave tubes.

Through some process that I really can't describe at this time—it certainly didn't result from my applying for this—it must have been through Watkins doing this, I ended up being given an acting assistant professor appointment in 1956, and started teaching a course, and continuing to do research at Stanford. So now I've gone from graduating from high school to at least an acting professor appointment in seven years, which is—well, surely the result of a lot of fortunate circumstances. (I get the Ph.D. a year later in '57 or at least finish the formalities). Now, 1956 is the year that a man at Harvard named Nico Bloembergen—later a Nobel Laureate and an immensely wonderful and distinguished guy, comes up with the idea of the so-called microwave solid state maser. And he writes a classic short paper in *Phys. Rev. Letters* (*Phys Rev Letters* exists by that time) which just absolutely tells how to make a realistic microwave maser amplifier, presents a very clear analysis, and at the same time, very solid physics. And in my view, at least, Bloembergen's invention was the development that broke open ultimately four years later the advent of the laser itself.

Townes' 1951 invention and 1954 successful operation of the first maser, the first ammonia maser, was beyond any question an immense intellectual contribution. But I'm not sure that anyone other than one or two other places, ever built an ammonia maser. I'm not sure that it ever really went into any practical application. It may have been, for a little while, a time or frequency standard. But it is another prime example of the role that microwaves played in the advent of lasers, because Townes understood microwave cavities, which I think a lot of other atomic physicists or molecular physicists wouldn't have. At least he understood that you had to have some kind of resonant structure or cavity along with a population inversion to make a successful stimulated emission device, and he did this in 1954.

But it was Bloembergen who, in essence, with the microwave solid-state maser, showed that microwave pumping, pumping between atomic levels, could create inversion and give you a way to make a device that would be a coherent stimulated emission device. And so I started, at that point, switching over from microwave tubes and began making microwave masers—they turned out to have some applications for very low noise microwave receivers, and they were used for some radio astronomy and some space communications and so on. But they were rapidly made obsolescent. They had much less noise than other microwave tube amplifiers but they were rapidly made obsolescent by subsequent developments, which I'll say a little about.

Another very important idea that came along at this time was something called the parametric amplifier and the parametric oscillator, again in microwaves. And these were very low noise devices that were simpler to make than the masers. They didn't require liquid helium, they didn't require a huge magnet. And incidentally, Ted Maiman, for whom CLEO had the very heavily attended memorial session yesterday, was also working on microwave masers at Hughes, this was his assignment in 1957 to 1960, when he thought more broadly and invented the ruby laser. He and I certainly had two overlaps, or more accurately, near misses. I would've been a graduate student at Stanford in EE when he was doing his physics degree there with Willis Lamb. And he had earlier been a EE master's student at Stanford. The other one I'll come to in a minute. I also worked on these various microwave solid state parametric devices and made some contributions in this.

Jumping back a little bit, there was something that one took in high school at that time called the Kuder Preference test. You were asked a bunch of questions: "If you have an afternoon free, would you rather go to the library and read a book, or go outside and play baseball?" or something like this. The scientific validity of this test is probably very dubious, but they come back and tell you, "Well, you should consider this or that career path, fashion design or whatever." And I certainly remember that when mine came back, what was recommended was not engineering nor science, it was clerical [laughter], which might've been in my genes because since my father was an accountant and a controller. And I wasn't disappointed or anything else. I didn't have any great reaction to this. But I think where it may have been a valid assessment, is that I have always gotten satisfaction from collecting ideas and analyses and scientific principles in whatever it is I'm working on, and reading a lot of papers and collecting references, and trying to bring those all together into a unified, clear presentation—put everything in a common notation and so on. We'll get back to this, I think, in a short while.

So as a new faculty member, I started writing class notes and a book on microwave masers. And I guess it would have been the early 1960s, a year or two after the laser was invented that that book came out with McGraw-Hill. In retrospect, it was an excellent book. It was very well done. The only problem was that the microwave maser, the solid-state maser, was being totally replaced by other things. Nonetheless, it got translated into Russian and Polish and got good reviews. But it just totally lost its market about the time it came out. So now it's 1957, and then 1958. 1958 is a landmark year—and Townes and Schawlow write this famous Phys. Rev. paper on of how to make a laser. And as Jeff Hecht said in the tribute yesterday, they give an excellent physical analysis of what's needed to make a laser, and they try to suggest one or two ways that you might do it, none of which actually really ever turn out to be successful, or at least important. But their stature in the field, and their writing this Phys. Rev. paper gets a lot more people interested in how can we go from masers to lasers. And I really was not one of those, to be honest. I didn't have the imagination— the drive toward looking for broader things that, for instance, Ted Maiman obviously had.

Then the next year, the thing that happened in 1959, was that Charles Townes organized the famous Shawanga Lodge meeting on quantum electronics in 1959. This was sort of a Tube Conference-like thing, in that it was mostly by invitation, a few hundred people. It was held at a resort in the Catskills whose clientele was probably to a large extent from New York City and from the Jewish community in New York City. And they, I don't think had ever had a technical meeting before. One of the little wrinkles that I remember is apparently normally, if you go there, you go there as a family, you go for a week, and you book or are assigned a table in the dining room. That's where you sit every day for a week, and your partially used wine bottle may get taken back and brought out at the next meal and so on. And that's what the waiters were used to. And, of course, the scientists would come out of the meeting room and whoever you were talking with, you wanted to sit down with, and I remember some incident where they just—the waiters were not able to cope with this idea. They wanted everybody to sit at the same seat for the rest of the week, like being assigned a seat on a cruise. And that was kind of entertaining.

But this was a great meeting in which something like eleven subsequent Nobel prizes. I've forgotten the number, it's eight, nine, eleven, something like that, at least eight Nobel prizes came out of the attendees of that meeting. And, in fact, at least one of the graduate students who was kind of an errand boy at the meeting later became a Nobel laureate. Townes had brought some of his graduate students along to the meeting to assist with things at the meeting. And so Maiman would have been at that meeting, and I was at that meeting. In fact, Townes invited me to be on the program committee for that meeting, which was really, looking back, a substantial recognition. I was, by far, the youngest guy and the least distinguished at the Program Committee meeting. I have a classic picture of the Program Committee meeting, which I cherish, in which you can go around the room and point out Bloembergen and Townes and Charles Kittel and a lot of other people who—Rudy Kompfner—who were at the time very distinguished.

And so Townes must have somehow found some merit in something I'd done on microwave masers. And so I, again, got to know a lot of the people at Bell Labs. And so it's now 1960. Maiman makes the ruby laser. As several people said last night, or yesterday afternoon, it was so simple, you could buy the parts off the shelf, the flash lamp, you could order the ruby—I guess I'm not dead sure about the following, and nobody said it last night, but this was still the era of mechanical wrist watches with jewels for the bearings. And a 21-jewel Bulova was something that would be like a Rolex today in terms of the advertising. These jewels are tiny little things that sat into the watch, and a pointed pin goes into them as the bearing. These watches are assembled by presumably—I don't know, again, perhaps Asian women or something in sweatshops are making these. And they're putting the jewels in with tweezers. The jewels are sapphire. But sapphire is clear and transparent, it's very hard to see them. So you want a little color in them, and it's known that you can put in some chromium and chromium-doped sapphire becomes red or pink and is ruby. And now you can see them.

And so that meant that ruby was grown, as I understand it, by the ton. This was not a precious crystal or jewel, despite what Cathleen Maiman said yesterday, and was available. So you could buy—they weren't necessarily good optical quality—but you could buy ruby rods. So we started instantly to acquire one of these at Stanford. And part of the milieu at the time was also that as a result of all the success of radar during World War II, the government was generously funding research. You may recall, there was an ad for whiskey: "Now that you're up, get me a Grants." And there was a physics cartoon about "Now that you're up, get me a grant."

So we were really able to do this, and it wasn't expensive. And two or three of us young faculty members got together and—in addition to making ruby lasers—were thinking about ways to improve them. The spiral flash lamp that's shown in all the pictures of Maiman was connected to a capacitor bank, and then you had to make a spark to trigger it. Our first spark source was a Ford Falcon spark coil from the auto parts store. I began to develop at that time, an interest particularly in how to shape the mirrors that were necessary on lasers of all sorts. There had been work done by—this was mentioned again yesterday—by Fox and Li at Bell Labs and Jim Gordon and others, and the idea of so-called stable optical resonators, stable periodic focusing systems was becoming understood. The stability of periodic optical focusing systems had antecedents in the focusing of electron beams, which is something I knew something about from having worked on the beams going through these long skinny traveling wave tubes, the electron beams.

And then through a series of circumstances, I realized that it would be possible to make a so-called unstable resonator that could be used for high-power lasers, and it would have some attractive attributes of large mode volume and so on. That idea has since become a useful technology for various kinds of high power lasers. If the Star Wars lasers had ever been feasible in many other ways, which they weren't—never were—they probably would've used unstable resonators. And Bob Byer, whom you talked to earlier, at one point made a very good high-energy solid-state laser using an unstable resonator, among other things. So at that point then, I began to switch over more and more from microwaves into optics and lasers and applications of lasers and techniques for lasers.

LS: And now we're right in the era at which you joined the Optical Society, which I have as 1961.

AS: That's interesting. I didn't have that number. That's very likely. Okay.

LS: This is according to the Optical Society of America's records.

AS: Okay, I believe it, I believe it!

LS: If they're accurate, then we can go by that.

LS: So tell me what you knew about OSA when you joined in '61.

AS: Well, let's see. Let's first of all say that the primary society for the microwave part of my career would have been the IEEE, which had previously been the IRE, the Institute of

Radio Engineers. And the journals that were published—there were the Transactions, the Transactions of the various technical groups. There was the IEEE Transactions on Electron Devices, and the IEEE Transactions on Microwave Theory and Technique, ED and MTT, and primarily the electron devices one. A lot of the early laser papers went into that. And I probably joined because I was interested in getting the journals, in particular the *Journal of the Optical Society of America*.

At some point along there, and again, I have a hard time stating the exact date, IEEE began its *Journal of Quantum Electronics*, the IEEE, *JQE*. And at some point in that era, the IEEE *LEOS* would have been formed as kind of a spin-off away from electron devices. The Transition on Electron Devices basically converted to solid-state semi-conductor sorts of things, at least for a while. So I almost surely would've joined OSA, partly to go to the meetings, partly to get the journals. That would have been the natural thing to do. Then there began to be a set of quantum electronics conferences. The Townes Shawanga Lodge meeting was a sort of a predecessor to that. The first real quantum electronics conference in the sort of continuing series, I think would've been in Berkeley. I was going to go back and dig into records and chronologies and I'll turn over to Chad a lot of the early program booklets and so on from those meetings. But there was one in Berkeley, I think, in 1960, maybe '61. And then there was one a few years later in Paris. These were planned and put together by something called the Joint Council on Quantum Electronics. A cooperative cross society group between IEEE and OSA. So there would've been representatives from both of those societies.

You may also find out from others, there were some people in the OSA who almost had to be dragged kicking and screaming into the laser field or into recognizing that lasers were going to revolutionize the society. They wanted to stay with traditional optics. I don't recall ever knowing very much about that aspect myself, probably because I came into this more from the IEEE side. And I haven't had a chance to go back and see just when I did what in this. But I certainly became a member of one of those, the JCQE, part of that administrative or organizing process for society affairs. I really don't remember details, except that at some point, I was asked first to be the program chairman of one of the Quantum Electronics meetings. I'm embarrassed how fuzzy the details are, but I was asked to be the program chairman for one of those meetings which was held in Phoenix, I think, in Arizona, and it might've been '65. And there was a kind of semi-automatic progression at the time that whoever was the program chairman for one meeting became the conference chairman for the next meeting.

And so I was program chairman and then conference chairman for two of those meetings in the late 60s, in either Phoenix or Tucson, and the other in Miami. They were enormously oversubscribed meetings because the field was just exploding. And it turns out if you look into it, essentially every major laser that we have today had actually been demonstrated or invented in at least some kind of primitive form by 1966. There just was an immense explosion of the field, of lasers and of non-linear optics, within five to six years after Maiman's first ruby laser. In 1960 alone, after he made the ruby laser on May 16th, 1960, within three or four months, a man named Peter Sorokin at IBM Labs had made samarium and uranium solid-state lasers, at least one of which was a true four-level laser. Those were the second and third lasers. And then Bell Labs really pushed on their gas lasers, and I believe the first helium neon laser, not the red one that is now common, but the 1.15 micron helium neon in the infrared was operated December 1960, and published early the following year. And then in 1961 through '66, a real explosion of the field. And as part of my personal chronology in 1964, Bloembergen went on sabbatical from his position at Harvard, and I was invited to come back and occupy his office or at least teach a course or two for the semester that he was away. So I was at Harvard for, essentially, a semester with my wife and two small children—three small children. It was 1964. And since we were going to be back there for a year, I then spent the Spring semester of 1965 and into the summer working on an experiment that Charles Townes and Ali Javan had in progress at MIT. It was actually a kind of a Michelson-Morley ether drift experiment that they were doing using a laser on a rotating table. They were doing this in a seismograph vault that was out in a suburban town called Harvard, Massachusetts.

That was not something I directly had much real technical background in, general laser knowledge. And I'm having a mental block here—there was someone who was primarily doing that for them, whom I worked with. I'll probably think of this in a minute. But that was a very nice year-long sabbatical on the east coast. When I came back to Stanford, at some point along the way, Schawlow had come to Stanford as a faculty member. And I could say quite a bit about kind of the earlier history of how Stanford got into microwaves and so on back in the 1930s, but I think I'll skip over that. But Felix Bloch and a man named W.W. Hanson, who died tragically early but was a really important figure, invented the microwave cavity and patented it with a patent in which the figures look like what everybody knows in microwave textbooks, in standard electromagnetic theory textbooks. And, of course, the Varian brothers and the Klystron, and Hewlett and Packard. And Frederick Emmons Terman, who was a major seminal figure in all this.

And so I was in that milieu, or I was a descendant, if you like, of that Stanford ancestry, or maybe an adopted child of it. At that point, Bell Labs had invented something called mode locking, which is a way of generating ultra short pulses by having the signal that runs around inside the laser cavity, instead of being a continuous signal going back and forth, it becomes a very short pulse. This pulse goes around and around and this generates a train of very short pulses, and those are very useful for communications and measurement purposes, and have led to the whole field of ultra fast physics today. My contribution to that topic came from my having taught a Fourier transform course, so that I understood the time-frequency-bandwidth relationships of all this very clearly from electrical engineering concepts. Those were fundamental EE concepts. And I was able, together with a grad student named Dirk Kuizenga, to come up with a very simple analysis and model that really, in a very simple way, predicted the properties of active mode locking , the pulse widths that could be obtained, and how to design for them and how they depended on this and that. This analysis, and experiments we did to confirm it, achieved some notoriety. Its the Kuizenga-Siegman model of active mode locking.

And I became involved, along with a lot of other people, with a lot of chemists and others who wanted to use those pulses to measure fast chemical reactions and physical phenomena. OSA started a topical meeting, which was initially known as the ultra-short pulse meeting, which has gone on for many years, and continues today. So I attended a number of those meetings and was on program committees, and ultimately co-chaired one of those meetings, and I'd have to think when that actually was, mid 70s, early 70s maybe.

At the same time, I was regularly, year after year, teaching a two-quarter Stanford graduate EE course which started out as masers and lasers, and rapidly evolved into lasers only. The analysis and the fundamental physics of masers and lasers are very similar. It's just the same phenomena at different frequencies, or very different wavelengths. So I wrote a successor to the microwave solid-state masers book, which was called Introduction to Lasers and Masers. It had a dramatic picture of a laser beam seen from twenty miles away on the cover, which came from an early Spectra Physics advertising brochure. That book came out in 1972, and, looking back, was also a good job. And after it came out, I just routinely continued generating class notes and amending and improving them. And incidentally, I came to recognize very early on that the best way to do this was to computerize it, was to do it on the computer. Before I realized that I must have burned up many, many reams of a special erasable paper called corrasable bond. Okay. I would type everything myself. I was a good typist.

Although, as an aside here, a weird or not-so-weird anecdote. When I was in high school, I wanted to take a typing course, which was taught at the school. And the principal of the school, Father Sheehy, would not let me do this because he just thought I should do serious things. I was smart enough, I didn't have to take a typing course, and he wouldn't let me take it for credit. In any event, that 1972 book ultimately evolved into a 1986 book called *Lasers*, which we can come back to.

LS: I'd actually like to focus on your involvement in the OSA.

AS: Oh, okay.

LS: Okay, here we go. And resuming. We were going to talk about your involvement in OSA during this period from your joining in '61 to, you know, mid-70s.

AS: Well again, there were two periods of my active involvement wit OSA affairs and OSA governance. One was quite early, and one was much more recent. And I have only scattered memories of the early period. I was on the OSA Board for a while at a time when Peter Franken was president, and Jarus Quinn was the executive director. I knew Jarus very well. He was, of course, a wonderful person. And I have memories of attending the board meetings, which at that time were held in, I don't know, a Holiday Inn or something, looking onto the harbor in Annapolis. There were certainly several meetings that I went to there.

LS: Do you remember what the board was particularly concerned with at that time?

AS: You know, the answer is no. I'm sure that looking at the minutes would bring back many memories. There were certainly concerns over keeping the Society self-sufficient, and keeping up with the field, the evolving fields. I remember, it may have been Peter Franken, it may have been someone else, who pushed to have a paid commercial survey done of the membership, by hiring a professional surveying firm. And I remember some opposition to that. "We know our members. We don't need to waste money on something like this." My memory is this survey turned out to be surprisingly productive, actually. And I do not remember controversial issues. There may have been some disputes, not disputes, but discussions over how the journals should evolve, whether we should start a letters journal, that kind of thing. And I don't know how much opportunity I'll have, but I'd like to go back and look at some of my files from that time and see if I can bring out some other memories. But I'm surprised at how little I remember from being on the board in those early days. I certainly had a lot of involvement in, of course, the planning for those two early quantum electronics conferences. But I think I may have more exciting things to talk about if we move ahead. Because there was a later period when I was certainly a member of the society and active in its affairs, and may even have been, from time to time, on the board.

I know that Jarus Quinn, at one point, asked me several times to be one of the three candidates for president, which the OSA puts up every year. And I think I was heavily involved with career, family and research, and was unwilling to do that on those times. And so I finally, as I neared retirement in the middle 1990's, was asked to run again and did say yes. That would've been in 1996 when I pretty well knew I was going to retire at the end of 1998 and stop doing contract work or at least stop doing active contract-sponsored research and writing proposals.

And so I agreed and was elected to the presidential chain. As a result, I was vice president in '97, president elect '98, president in 1999 and then past president in 2000. I'd like to add: I'm most impressed and gratified that every year, three people, three distinguished people are willing to run for these jobs, and two of them get turned down. One really shouldn't say turned down—it turns out the elections are always pretty close.

At any rate, in 1996 I went back on the board and started getting very involved in OSA affairs. There's one thing I did at that time that I look back on with particular satisfaction. I was familiar at this point with how aggressively Stanford University did fundraising, or "development" is the buzz word. I had not been directly involved in that myself for Stanford, other than knowing about it, and I knew there were lots of named buildings and so forth and so on. (As an aside, the Economics Department building at Stanford has four different names on the four sides of the building almost literally, identifying four different donors.) So, there was a still quite young guy named Dwayne Fullerton who had been the School of Engineering's associate dean for development, whom I knew, and I knew he had retired. And I thought that the OSA really ought to get into the development business, and ought to have a development officer akin to university administrations, and have a development policy.

So I arranged for OSA to hire Dwayne as a consultant, or at least pay for his airfare to come down and talk to the Board, or at least the executive committee of the OSA at one of our meetings. I'm pretty sure it was in Long Beach. It would've been in '97, '98, something like this. And I remember Paul Forman in particular, who just died recently but who was a very wise person in the OSA leadership, being very impressed and very interested in this. I believe these efforts were essentially what laid the seeds for the founding of the OSA foundation; in any event I helped in pushing that idea. And I think that's turned out very well.

Now the more controversial event during my presidential years was there had been one attempt, a decade earlier, to try to somehow merge SPIE and OSA as overlapping sister societies, although societies of quite different character. So that idea somehow was brought up again. I guess the guilty parties would've been Paul Forman from the OSA side, and a very great guy named M. J. Soileau from SPIE, who is a real builder, and is essentially the founder of CREOL, the Orlando University of Central Florida’s whole optics program. He's now a vice president down there. So they persuaded the two societies to put together what was called a Joint Task Force to look into the possibility. This proposal immediately became widely referred to as a “merger”, but it was, in fact, a proposal in which OSA, SPIE and possibly other groups would become largely independent and different components within a kind of an umbrella organization, which became known as the UNO. It was to be the “UN of Optics”, and so it was called the UNO, which stood for “unnamed organization”, and the final name would be decided on later.

So, this Joint Task Force, a group of about twenty people from the two societies, including the executive directors, and the treasurers, and I believe Liz Rogan was a member, had a whole long series of meetings trying to work out, was this feasible, would it work? We had a lot of meetings, like once a month, under a formula where we'd all fly in Friday night, meet all day Saturday, and leave early Sunday to get home to our families, by mid-day Sunday. This massive effort was very wisely led. I've been on a lot of university committees, including Stanford's Faculty Senate. I've enjoyed a lot of them. I was on the steering committee at the senate at the time when Condoleezza Rice was our provost, so I was in the room with her many times. And I've always enjoyed the contacts that these university meetings brought me. But this Joint Task Force of the OSA and SPIE was certainly the most impressive group that I've ever been part of, in the way it worked and collaborated and cooperated and brought ideas together—the most impressive such process that I've ever participated in.

And there came to be near unanimity that this "merger" would be a good idea and could be worked out, with a couple of exceptions. One of these was Bob Byer, whom I think you also interviewed, who I think just recognized that this could be a great idea, but it just wasn't going to happen. Another was Erich Ippen, who was from MIT, wonderful guy and very distinguished guy, who just had to say that he didn't believe in it. He didn't think it was a good idea. And he was very wise in that, and very prescient, I think, in that. And so he actually stepped off this joint task force.

This was an embarrassing—not embarrassing, but it was a difficult thing for him in a way because this was going to become something that the OSA had to sell to its members, and he was the next in line for president. The election would be held the year I was president. Eriich was in this difficult position of sincerely not believing in it, but not at the same time trying not to stand in the way. So this became, as you may know, a very controversial issue. There were "old timers" in the OSA and some new-timers, younger timers who looked down on SPIE, I think it's fair to say. SPIE is not a society in which the primary focus is on refereed or peer reviewed work. Rather they attempt to organize meetings quickly, they're productive, get books out. They put out a meetings proceeding a day all year long. They put out three hundred to three hundred and fifty volumes of meetings proceedings a year.

They have a mailing list of—at the time, 250,000 names, even though they only have about twelve thousand members. And I learned from university librarians, at the time at least, that their proceedings, even though they were totally un-refereed and had a lot of what I think it would be fair to characterize as somewhat mediocre stuff, were extremely popular with industry in the Stanford area, for example, and students as well. The industrial people would say, "These proceedings are very timely, up to date, they're very useful to us. Just give them to us, don't worry about the peer review, and we'll sort out what we like." And that was a very different ethos from OSA in which I think around 65 percent of the membership is Ph.D.s, and, I would really emphasize, peer-reviewed journals and peer-reviewed meetings are the core of what OSA does.

So it was my job, and I was sincerely behind it at the time, to try to sell this concept of "UNO" to the OSA membership. I think it required a two-thirds favorable vote of the membership, certainly of those voting to pass. And the opposition from those in OSA who didn't agree with this got to be fairly heated. Looking back, I think I handled it reasonably well. I think I did a decent, good job of giving everyone the chance to voice their feelings on this, to voice their objections and to make sure that both sides of the argument got full publicity and so on. At least, that was what I tried to do. And in fact, in the end, the vote came out just practically 50/50. I think it was, you know, I said at one point 49.37 percent of the voters of the membership endorsed me, and the other 50 point whatever,—67, 63—percent were vehemently opposed to this. And knowing that it was going to take a long while for this to work if the proposal passed, I mean I must say, as the vote neared, I began to realize that if it passed, we would really have to do this, and that was going to be very tough. That was going to be very difficult. And so I have to say, I was actually immensely relieved when it didn't pass. I was truly not disappointed a bit.

I remember I had to announce the results of the vote at a huge OSA meeting which, again, I think was in Long Beach. The vote was handled by an outside organization, and I truly didn't know what the results were going to be until minutes before I went to the podium. And I remember vividly standing in front of this huge session of the meeting and saying something to the effect that I supposed it was up to me at this point to say something statesmanlike, only I didn't know what that would be. And in fact, it turned out that there were people in SPIE who were also very proud of their society and of its approach to things. They regarded themselves justifiably as very nimble in meeting professional needs. "Nimble" was a word that was used a lot to describe SPIE and that was absolutely true and correct. And they—I don't want to say exactly what the adjectives were but I believe they thought that we in OSA were very elitist, I guess would be a safe adjective to use. And they, it turned out, were not interested in joining with OSA either. And the vote, actually, was substantially negative on the SPIE side, which I must admit I was somewhat surprised at, and it showed that I didn't understand that sentiment within SPIE.

Now, one of the points that the opponents of the UNO proposal argued in OSA was we could do all the cooperative things that we said could be done better in a unified organization: shared meetings, shared publications, this kind of thing, could be done without joining into a single organization. And I think that has, in fact, not turned out to be true but not because of any ill feelings or lack of desire to collaborate, just practical considerations. If you try to put, say, two of our OSA and SPIE meetings together so that people in the field can get to both of them on one air ticket, or if we tried to share computer facilities or that kind of thing, it just doesn't work because we're two separate organizations, and it's just a practical sort of thing. So our relationships with SPIE since that time have been friendly and cooperative, but not deeply intertwined. This is recent history I know, but this is the history I know about, and that will be more significant history twenty years down the line. I would say OSA is proud of this and happy that our relationships with IEEE LEOS are also exceedingly friendly and cooperative.

Some of the other things that came out of this were more active efforts to get the two boards, or at least the officers of OSA and IEEE LEOS to meet pretty regularly, and also the executive directors. And the same thing, to a lesser extent with SPIE, which is really a different breed of cat as a society. And that's not to say anything pejorative. So we do things where we can cooperate with them, like the Joe Goodman Book Award awarded by each society alternately and that kind of thing. But the promise that we could cooperate with them even more closely without merging has not been fulfilled. The opponents were wrong on that—that has not been fulfilled, and they were optimistic, let's say.

LS: I want to ask you a quick question about something you said. You mentioned trying to make sure that, you know, all the points were publicized—

AS: Yes.

LS: —on either side of the controversy. What kind of mechanisms did you have for making those viewpoints public?

AS: Well, the president writes a column every month. OPN comes out every month and goes to all the membership. We printed many letters on the proposal. I think there were letters on all sides of the proposal. The people who didn't think the merger was a good idea at one point wanted to put out more, they wanted either a special mailing, or I've forgotten the exact details, but they wanted to get their objections out, and they felt that the society was only putting out the company line, in a sense. I think we made arrangements for there to be a special mailing, as I recall, to all the members, which was a moderately expensive kind of thing, in which they could put in their arguments and their objections and their concerns and so on in considerable detail as one part of this. And the Society—not the Society, but the proponents could also put theirs. And that was, I guess, the major thing. At first there were some concerns about doing this. There were some—I'm not going to say they didn't want to do this, but they were concerned that it would cost too much, or something like this. And we certainly—the Society ultimately agreed to do that, and I think it considerably soothed the waters.

I'd like to also add one other point,—this might be kind of my parting advice or something, looking back. I think it's very important that the OSA strategic planning committee really look hard at emerging fields of optics and attempt to identify them. It turns out, for example, that way back twenty or more years ago, there was a small OSA meeting one of the topical meetings, on optical fibers. And that meeting ultimately grew into the OFC, which turned into just the massive money maker for the Society. And OSA basically owns a third of that meeting, and I think probably owns the trademarks, and so on, on it are.

One of the things that SPIE does very well and very effectively, is to move very rapidly if a new field or topic comes along, and very quickly set up a meeting on this, whereas OSA takes much longer to say "Well, who is really the leading person in this field, and who should chair this meeting, who should organize it? How do we make sure this is going to be really a good meeting?" And so, for example in certain areas of optics—photolithography would be one premier example—SPIE, by getting the first meeting organized, gets itself identified as the place to go for that field and can sort of grab off a field, if you like, by acting fast. And I think OSA has to, along with coping with all the changes in publications and so on, it needs to do that strategic planning very, very well.

LS: Okay. I'm going to switch subjects while remaining with the OSA here for a moment.

AS: Okay.

LS: And talk to you a little bit about prizes and awards. You've received a couple yourself. The Wood Prize in 1980 and the Ives Medal in 1987, and you've also served on committees for the awards.

AS: Yes.

LS: Tell me a little bit about what you feel the place of the awards is within the Society.

AS: Societies, and society awards are a tradition that, of course, goes way back. Probably to Isaac Newton or thereabouts. Certainly societies have always done this, so it must be a deep-seated something in the human psyche to recognize people. I think that these awards are very gratifying to the people who receive them. They certainly also serve a practical function outside the society's remit, as does refereed publication, for promotion and professional advancement, particularly for faculty members who are trying to get promoted or get tenure. I suppose awards may draw people into the society. You don't exactly join the society hoping to get an award. I doubt there are very many, if anybody, who does that. But having gotten an award is a way of bringing very good people into Society affairs, and keeping them members and participants in the planning. For instance in the strategic planning I was talking about, and the serving on the board, serving on committees and so on.

I believe that, by and large, societies do a pretty good job of picking the awardees for these awards. Certainly they're not political—it's not a political game where I vote for you and you vote for me—it really isn't. At least I have never encountered one, anything that I would remotely consider—the kind of thing where you scratch my back, I scratch yours, and you vote for me this year, and I'll vote for you next year. I think the committee decisions are very serious and seriously undertaken—people at least try to and generally are able to recognize merit and accomplishment and try to recognize an honor by these awards.

As a side rant here, in also a merit or theory, the patent system is a peer review process—if not a peer review, at least a review kind of thing comparable to publication. A patent, like a publication, should have an important idea, it should be significant, it should be well-written. And from the interesting experiences I've had in the patent system as an expert witness since retirement, and to some extent before, I may, one of these days, write a diatribe that points out that none of these things are really true about the patent system. The patent system just isn't able to do a competent peer review. Ninety-seven percent of patents that are applied for get awarded, and I fear that 90-something percent of them are junk. That's the end of that rant, but whereas peer review works sincerely and, by and large, well in the publications and awards worlds, it does a lousy job with patents. It works about as well as it can.

LS: Both for publications and for awards?

AS: Even better for awards because it's a smaller set. It's a smaller set and—well, okay. And when you say awards, also election to fellowship is a significant award.

LS: You mentioned the OSA foundation board a little bit earlier. You served on that from 2003 to 2008, so you're currently—

AS: Yeah, I'm currently—I'll be going off at the end of this year.

LS: Will you talk a little bit more, I get the sense that we've sort of covered in some ways its birth from—you know, from the idea of meeting to—

AS: I think an executive director of our Society, John Thorner, who suggested the idea of a separate foundation. I believe that Steve Fantone who was the treasurer, and a great guy and great treasurer, was initially opposed, and now thinks it was a pretty good idea. And he's been on the board.

LS: We're about to run a little bit over our 90 minutes.

AS: Okay.

LS: So I just want to warn you.

AS: Okay.

LS: And ask you about what you feel, I guess looking back, you've got a nice 47-year perspective on the OSA. Can you talk a little bit about what you feel like is the—what you feel is the importance of the Society today, and looking forward to the future?

AS: For just about two hundred years, the royal institution in Britain has been having a set of Christmas lectures by very distinguished scientists for families and children. Some very distinguished scientist tries to explain some important scientific field. These are televised on the BBC and so on today, but they date back to Michael Faraday. And Sir William Bragg gave one of these lectures in 1931 on optics, on aspects of optics. It's available as a Dover book, actually, his lectures. And he opened with the phrase "Light brings us news of the universe," Somehow that caught me, and I have always thought that was a wonderful phrase, and I've used it.

Bragg was talking about, first of all, astronomy telling us about what's out in the stars, and to some extent spectroscopy and telling us about what's inside atoms.
(I think he actually did not talk so much about spectroscopy.) But light, in various ways, 'brings us news' about everything from the whole universe, light coming from distant stars, and black holes and all that sort of thing, tells us everything we know about the cosmos. And light coming from atoms, from inside atoms through spectral lines and so on, tells us everything we know, maybe not everything, but a vast amount of what we know about atoms. This is the source of the periodic table and everything, all the fundamental things we know about atoms and molecules. And then, the situation today is that light coming over optical fibers is what weaves the World Wide Web and brings the whole intellectual universe of the world into your laptop. On a typical evening, I and my wife and my daughter, if she's visiting us, are likely to be all sitting with our laptops in the living room probing libraries, and news sources, and blogs and archives totally around the world.

And its optics that really makes all of that possible. And the advent of the laser, besides enabling the fiber optics web, enables us to just do so many kinds of tasks, and scientific measurements, incomparable scientific measurements and incomparable engineering. The supermarket scanner is a mundane example, and the police laser radar. It is very interesting that lasers as an economic market are a very minor one—the worldwide market for lasers is a few billion dollars total. And that amount might buy you one semi-conductor fabrication plant, a few billion dollars, you see. But the economic impact of lasers on daily life, on engineering, as they weld auto bodies and so forth and so on, is just immense. It's just beyond compare. Part of the problem is the laser is so incredibly productive as a tool. If you have a laser cutter, it never wears out. It never needs blades. It may need a few supplies along the way, but it's—well, anyway. The Optical Society is a small society by many standards, for example compared to IEEE, which has 150,000 members or something. But it has a bright future, not to coin a phrase.

LS: Let me throw some words of yours back at you—

AS: Okay.

LS: —and ask you a question about them. Back in 1990 in an article in *OPN*, you were one of several people asked about what you would go into if you were starting your career. If you were twenty-five years old today, what research directions would you take. And you said, "If I were twenty-five and seeking a chance to make a really fundamental contribution to human welfare, I would very possibly look toward the chemistry / biology area hoping to find a chemical biological solar solution to our energy and transportation—"

AS: Ah, okay. That's—

LS: "I might look toward the field of computers, artificial intelligence, neural nets or communications looking for ways to manage and communicate among wildly disbursed social systems to the benefit of all."

AS: I was more prescient than I knew I was [laughter].

LS: So I would ask you whether you would amend, amplify or in any other way respond to what you had to say.

AS: Well, look. Certainly medicine as we were—last night with some problems—medicine and biology and biophotonics are areas that are making immense advances. Stanford is building a biophotonics building as we speak, and that is certainly going to be an immense area. And the Web and the internet have blossomed beyond anything any of us could have foreseen, I think, even at that time. And I don't really know what will be the next “big thing”.

I think at this point, I would rather answer the question, “What concerns me?” And I think what concerns me these days are more social issues such as keeping the Web free, keeping true network neutrality, keeping the forces of commercial advertising from being even worse than they are today and taking over the Internet.

Here’s an interesting quote. In 1890, Lord Acton said “power tends to corrupt; absolute power corrupts absolutely.” And that's what often gets cited to explain dictators and so forth. I’d suggest that a modern version of this is that “advertising tends to corrupt” publications, journals, newspapers and so on. Infomercials and that sort of thing. And “total dependence on advertising corrupts totally”, probably. And somehow—you know, Google, for example, is a wonderful, wonderful institution, and it does all these incredible things. But it's totally dependent on, almost all of its total income stream, its total revenue stream is advertising. And in some ways, I'd rather pay—I'm old enough to have bought an encyclopedia for my family, you know, and paid like a thousand bucks for it in 1964.

And you could put some reliance on the intellectual honesty of Britannica. And I'm not saying that – there's neither honesty nor dishonesty in Google. There's no editorial control over what you get. But who pays the piper calls the tune. And the fact that more and more things depend on advertising, is something I worry about.

LS: Well, that's an interesting sidelight.

AS: Yeah. So I think that there are lots of interesting scientific challenges and so on, but it's the social and political challenges, keeping our elections honest, which means listening to the people who talk about verified voting. Keeping the internet free and open, which means listening to the people who talk about network neutrality. Those kinds of issues I think much more about these days.

LS: I want to wrap up with a question that I always try and wrap up with, which is I've covered everything I wanted to talk to you about. Is there anything that you want to talk about that I haven't asked you?

AS: You know, I got to voice my pet peeves and rants in this interview. But overall, my involvement with optics has been a wonderful ride, and still is, fortunately. If I had to look back, I would try to pay more personal attention to the colleagues and the students, particularly, that I dealt with. I don't think I have ever been a distant person to students, and I've always really sincerely felt committed to the welfare of my Ph.D. students, and the students I taught in class. But I look back and wish I'd paid more attention to some of their personal lives and that sort of thing.

LS: Thank you very much.

AS: Okay, well thank you.

LS: I really appreciate it, and it's been a pleasure.

AS: It's gone better than I had hoped.

[End of Interview]