Optical Society of America
Oral History Project
Interview with James J. Coleman
Conducted on March 18, 2013 by Adrian Kinnane

AK: It's March 18th, 2013. We're in Anaheim, California, and speaking with James J. Coleman, who is currently Professor of Electrical and Computer Engineering at the University of Illinois. Dr. Coleman is this year's recipient of the John Tyndall Award from the OSA and from the Photonic Society. Congratulations for that, by the way. That's really quite an honor. What you received the award for, we will get into because we want very much to discuss some of your work. But I would like to start in the beginning and ask where were you born and when?

JC: I was born in Chicago in 1950 and grew up in Chicago until I went away to school at the University of Illinois.

AK: Were you in the city itself?

JC: I grew up on the South Side, not far from Midway Airport. Back in those days you could ride your bike over to Midway Airport and talk your way onto airplanes and pilots would show us around. We would claim we had a science project or something like that and used that as an excuse to get a tour of an airplane.

AK: Those were the days.

JC: Yes. Not like that anymore.

AK: Tell me a little bit about family. Do you have brothers or sisters?

JC: I have five brothers and sisters. There were three girls and three boys in the family, the girls first and the boys after, so the personality and the culture of the family changed as the girls grew up and left and it just became a male household.

AK: The differences in age were what?

JC: The total range was about fifteen years between my older sister and my younger brother, so the younger boys didn't really even know their sisters very well. Most of them had gone off to college or on to the rest of their lives. My mother was always mumbling about how it had changed when she had all girls in the house and the boys were little, and then the girls were gone and she was outnumbered by all the males.

AK: Where were you in the hierarchy?

JC: I was the oldest boy. It was a gap of four years after the girls, and then I was born and then my brothers.

AK: Tell me a little bit about your early interest science or engineering. Was it evident in your earlier years?

JC: Radio, it absolutely had to do with shortwave radio. I got a shortwave radio from an uncle who had a 1950s era Zenith shortwave radio that wasn't working. My uncle was not particularly technically-inclined, so I took all the tubes out and went up to the tube tester at the drugstore and found all the tubes were fine, so I went back and put them in what was apparently the right sockets.

The radio came on and I heard the BBC and Deutsche Welle and Radio Australia, and I was hooked on radios and wanted to become an electrical engineer. I wanted to know how they worked. In a sense it's the communications part of my career that goes all the way back. It wasn't so much science, not physics or chemistry but radios and televisions. I wanted to know how they worked, wanted to be able to fix them.

AK: Tubes.

JC: Tubes, yes. The Zenith Trans-Oceanic was a portable radio that weighed about seventy pounds and had a big antenna that had suction cups. On a train, you could stick it onto the window of the train. That was a feature of this, that you could put the antenna on the window in the train. I never took it on a train or anything. I don't know how long it would have run on a set of batteries. But it was just fascinating to that you could hear things from all over the world like that. We had television, I knew about that, but it wasn't the same as long distance, far away exotic places.

AK: It does stretch the imagination, doesn't it?

JC: It certainly did mine, but more than that I wanted to know how it worked. I couldn't believe this. What was all this stuff in there? I knew about testing the tubes but that was really it, and if anything else had been wrong that would have been the end of it probably. There was nobody I knew that was technically involved in anything like radio or television.

AK: About how old were you at that time?

JC: I can't really say for sure. I think I was twelve, more or less. The radio wandered down through the family and ended up in my hands, and I do have some shortwave listening cards that I got from some of those broadcast stations. In those days large foreign broadcasting systems would intentionally aim signals in English to the United States as both political and news propaganda, if you will.

So you could get Radio Moscow in English certain times of the day when the propagation was right to the United States; the BBC of course. It was an avocation to listen, to log the information. You'd send them a report and they would always send you a nice card that confirmed your report. Then magazines, and I'd get these things in the mail from time to time from these exotic places. I really enjoyed that.

AK: Were either of your parents employed in any way having to do with technology?

JC: My father was a pattern maker, and that's a skill, a craft trade that's gone now for the most part because of C & C [CAD/CAM] machines and things like that. In those days someone would design a part to be cast out of steel or aluminum. They would give it to a pattern maker who would make one out of wood, and it was slightly larger to account for the metal shrinking when it cooled. Then the pattern would be used to make the mold, and then you'd take the pattern out and pour in the molten aluminum or molten steel.

Pattern makers were a shop or a trade that was often associated with a foundry, and of course back again in those days there were many more cast steel, cast iron, cast aluminum parts, many of which now are machined or forged or stamped and made in other ways, or even made out of plastics now. Many of the things that my father might have made in the forties and fifties now can be made out of high tension plastics and things like that.

AK: How about your mom?

JC: My mom was a mom. She had six children and her hands full. She didn't even drive until she was in her fifties. She did start working after the girls were gone and the boys were old enough to fend for themselves, so she did work in a few places just to make some money. She was a mom.

AK: Tell me a little bit about your science education.

JC: I was fortunate enough that my mother really insisted that all of the kids be sent to Catholic schools. In Chicago, the Catholic schools were frankly better academically than the city schools. That was a decision she made for faith-based reasons, but it led to very important advantages for all of us kids for educational reasons. I went to the Catholic schools and went to a boys' Catholic high school in one of the suburbs near Chicago, and it was college prep. There were no shop programs or trade programs or business skills programs.

Everyone by definition was being prepared for a college education, and even students who weren't the best students were getting exposed to such a good education that the scores on standardized tests and the ability for what in our case was mostly a first generation chance to go to college. In that neighborhood, none of the parents had college degrees; it was very much a blue collar neighborhood, so it was only our generation that started to go to college. That was enabled by my mother. Even though money was pretty tight she found the money to do that. That was very important to her.

AK: What was the teaching order in high school?

JC: The teaching order in the high school I attended, St. Laurence, was the Irish Christian brothers, and they taught us a lot. It was fun. That order is based mainly in New York City and we took the New York State Regents Exams, and that's part of the reason the academics were so high, but it was a pretty rough and tough place – all boys, so the gloves were off most of the time. And I have to say I loved it, that whole environment. I took my share of lumps but I never minded it.

AK: You thrived.

JC: Yes, I liked it.

AK: Was there any particular teacher, sometimes there is, sometimes there isn't, any particular teacher who stands out in your memory as having been particularly supportive of your interests?

JC: I have several teachers that I remember very fondly who probably don't remember me very well. Surprisingly, the one who had the most influence on me was probably Tom Gorman, who was an English teacher. He just had a way of dealing with high school boys and he just knew how to get a lesson to you, life lessons. I don't know how much

English I actually learned from him, but life lessons. I'll never forget that I did something that I shouldn't have done and he didn't say a word, he just waited a few days. Then he made me take the part of Eliza Doolittle in *Pygmalion,* and he knew I didn't really want to be Eliza Doolittle. The cause and effect wasn't lost on me and it was a good life lesson.

AK: Let's talk a little bit about your senior year in high school. Actually it starts in junior year, when you're thinking about what you might do next, and it was a college prep environment, so the idea of college is certainly in the atmosphere. Tell me a little bit about your own decision about how you approached where to go, what to major in, how to finance it, all that.

JC: How I chose to go to college and the decisions I made I'd like to say had more planning and insight than they really did. One of my uncles had been to the University of Illinois in Champagne on the G.I. Bill and got a degree in mechanical engineering. Illinois was on my radar screen as a good place for engineering. To be honest, part of the appeal was just that it was 100 miles from home. My sister had gone to college. My eldest sister had gone to college and lived at home, and I don't know how she managed to make it through college living with all of us.

I wanted to go away if possible, but I also was not the greatest high school student so not all the opportunities were available to me. Fortunately I did very well on the standardized tests, because otherwise I would have never got into Illinois, but I was unsure about what I was going to do so I did two things. I applied to the University of Illinois-Engineering. And I took the airmen's qualifying exam for the U.S. Air Force.

I let fate decide and said if Illinois takes me then I'll got to Illinois. If they don't, I'll go to the Air Force. Illinois took me and admitted me to the electrical department, again based almost entirely on the ACT scores, the standardized tests, because my grades were not that good. They got better in high school, but I wasn't a serious student for a significant part of that time.

AK: But you did have a very serious interest.

JC: I did, and that never wavered. My interest in electrical engineering came from my interest in radios and I figured those are the guys that are going to teach me how this stuff works. I went there, even as a freshman, while I'm still taking calculus and basic chemistry and physics. I already knew which electives I was going to take in my senior year because I wanted those radio and television and communications and antennas and propagation, those kinds of things.

I had that all planned, so I never wavered. I guess the average in the United States is three or four college majors, but I never thought about doing anything else until, ironically, I started working in the field that brings us here today, when I switched out of electronics and into semiconductors and lasers and things like that. That was later in graduate school.

AK: So you would have gone away then as a resident student, University of Illinois, you would have started that in ’68?

JC: Yes. I started college in ’68, and my parents didn’t have the resources to send me there but I was lucky enough to get a really great job in a factory in the Chicago area. That gave me most of what I needed to get through school. I made the rest up washing dishes in the dormitories and doing that kind of part time work. I didn’t have a lot, but going to college is more about fun and you don’t need a lot of money. You know, a couple pairs of blue jeans, and you can get by on peanut butter for a long period of time.

AK: As daunting as it was in those days, it was still financially easier than it is now, wasn’t it?

JC: Absolutely. The tuition and fees for a state resident, even when you factor in inflation, seems like an amazing bargain. Even the public universities, your University of Illinois, that differential has gone away for in-state residents and out-of-state residents and the total costs are substantially higher. Having a few children who have gone through college and I’ve got one more starting next year, I know about the size of those bills.

AK: So you graduated in the prescribed four years.

JC: I did, I graduated in four years. I worked on some summer things to get that. It was the average. The average was a bit more than four years in those days in all the engineering disciplines, but I got done in 1972.

AK: Engineering is a crowded curriculum, isn't it?

JC: Yes. The curriculum at that time was a bit more than it is now because there were some things, like physical education and so on that are now options but were required in those days. Students worked hard in engineering compared to their colleagues and other colleges even on our campus. I was able to get some summer classes and some independent study things to get a little bit of credit in the bank so I could take a more manageable load.

When I went to college, you have to remember what 1968 was like. That was the Tet Offensive and Kent State was during those years. I can't imagine, and everybody feels this way about their college years I'm sure, but it's hard for me to imagine there were any four years that were more tumultuous for young people than '68 to '72.

The probability of being drafted was a real issue, and the university hadn't projected me to be a very successful student because of my high school record. I was frightened. I wanted to be there. I learned how to work and as soon as I learned how to work, more out of fear than brains, I found it wasn't that hard. If you just do a little bit of the work, a lot of it was pretty straightforward. So my grades continually got better while I was an undergraduate. I just found the right thing for me to do, never had any problems after that.

AK: So 1972, tell me a little bit now about the decision about what to do next. You're going to graduate.

JC: That's a great question, how did I decide to go on to graduate school, and it actually comes back to the draft and the army. I had a low draft number, but I had a student deferment so I didn't try to get a job when I was senior. I didn't apply to graduate school. I didn't do anything. I just took my classes and enjoyed my senior year because I was certain that I was going to be drafted because of the lottery number that I had. For political reasons going on elsewhere, by the spring of 1972 the Vietnam War started to wind down and they announced they were going to 125 and my draft number was 130.

In the spring of 1972, after the opportunity to look for jobs and all of that were past, I didn't know what I was going to do. I went and talked to somebody in the department and said can I stay around here? What can I do to stay around here? I figured at least then the following year I could get into the normal recruiting cycle and find a job. They said, no problem. We'll just enroll you in the graduate program. You can do a master's degree so you're not wasting your time, and besides, we'll give you an assistantship.

They admitted me to the graduate program on the spot, and they were able to do that because I was part of their program; they knew exactly what my capabilities were. I started graduate school. I had no idea about graduate school. I didn't know about PhDs. No one in the neighborhood I grew up in even went to college for the most part, let alone graduate programs.

I stayed, and I walked in the graduate program and started working on research and I couldn't believe they were going to pay me to go to school and get advance degrees and let me work with all this cool equipment. Again, I'd like to say I had a great plan and I was focused on this from the beginning, but I wasn't. I started graduate school figuring to leave in a year with a master's degree, and instead stayed three years and did my PhD.

AK: Tell me a little bit about how that happened. I think it varies with programs in schools, sort of the shading between a master's and the PhD. At some point a decision is made to stop or go on.

JC: The link between the master's degree and a PhD at Illinois is maybe less distinct than it is at some other universities. Most of the master's degree candidates at Illinois are PhD students by intent. They just need to get past the master's degree and move on. In my case I originally enrolled, at least in my mind, as a terminal master's degree student. In fact, I did my master's thesis for somebody else other than the person I did my PhD thesis for. I got done in under a year, and part of that was because the support they gave me was a fellowship that required a full load of classes. I gained ground on a typical graduate student, about a half a year's worth, and so I was ready to start doing PhD work the following summer.

By then I had met my advisor, Nick Holonyak, and you may recognize that name. The OSA of course has a Nick Holonyak Jr. Award for someone who does work in photonics. I asked Nick about a position and he told me a couple days later that, yes, he had a position that I could have. I got into his lab and I worked very hard. Nick expected people to work very hard, and he had a culture in his laboratory of people whose main business there was to get something done.

We would finish getting the data for papers one day and submit them for publication the next day. If he had data he didn't sit on it. He wanted it with a postmark date on it. Many others in the optical fiber communications community came from that same laboratory; it's not a surprise that we all were inculcated with this culture of don't let the dust settle on you.

AK: He was your thesis advisor?

JC: Yes, Nick was my thesis advisor. And I finished my PhD in 1975 in the summer, probably a bit earlier than I would have under ordinary circumstances. But Nick required some back surgery and so he let me finish my thesis a few months early. While he was out for a significant part of the time in the fall of ’75, I could manage his laboratory for him and have a little more of a bully pulpit as a post-doc rather than as a graduate student. I probably wouldn’t have gotten my PhD as early as 1975, but probably in the early part of ’76.

AK: Then what, after you got your PhD?

JC: The work we were doing in 1975 and 1976 was of interest to lots of laboratories. The possibility for solid state light emitters, you know, semiconductor, LEDs, lasers for telecommunications or other reasons; it was not hard to find a job. There were lots of good opportunities in major places. GT had research labs, Bell Labs, RCA Labs, General Electric, Kodak, Texas Instruments, Hewlett-Packard, all of these major large companies had research labs that were investing in this work so finding a job was not a problem at that time.

Finding the one that matched your interest took a little more thought, but it was a good time. It was a good economic time. That kind of work, which is what led really to a significant part of OSA and a significant part of optical fiber communications, was just emerging, and getting significant growth. In the end I had to choose from a number of places. I ended up choosing Bell Laboratories because I thought that was the best chance for me to work with and learn from the brightest people in the industry. I don’t mean to diminish the bright people elsewhere, but Bell Laboratories had an unusually high density of them. There were 4,000 PhDs in the company and most of them were really, really brilliant people.

This time I was doing a good plan. This was not serendipitous. Some of the other things, I realize how lucky I am that they worked out the way they did, but this was what I wanted to do. I realized that all the other companies I interviewed with, I could go to Bell Labs and work there for four or five years and I could go to any of those other ones, but not necessarily in the reverse. I had the chance to go Bell Labs; I absolutely took it, and it really was a very wonderful decision.

I was only there for about three years, but during those three years I learned some amazing things from really bright people, like how to separate personal issues from science issues. They could be your closest friends, but when it came to talking about the technology or the science they were very aggressive and you had to understand that it wasn't aggressive about you. It was about wanting to know. The hunger those people had to want to know everything made it a remarkable place to be as a young person and shaped, the opportunities I've had ever since.

AK: Who were you working with, what kind of things were you working on, and what sort of support or mentoring system existed there for you?

JC: The Bell Labs system and the position I was hired in was with Mort Panish's group, and Mort Panish is one of the co-authors on the first CW room temperature laser in the United States. There was also a laser like that in the Soviet Union, but we didn't know about it at the time. The Bell system had already determined that fiber optic telecommunications was going to matter for them in the future, and so they were hiring people to work in any of those areas.

One of my close friends and colleagues, Joe Campbell, was hired to work on the detector side, and there were a number of us working on the laser side to try to make lasers that could work at room temperature under modulation to transmit data. I was hired specifically because I'd been working on materials that were suitable for the wavelength range, the 1.5 micron wavelength range, for low loss optical fibers.

That was the project. I was given complete freedom on how to pursue that, what materials to use, what structures to make. My job was not to specifically fill a technical need, but just work as hard as I could in that area and try to add to the body of knowledge and publish novel structures, novel materials, novel devices to push the whole technology forward.

Mort was a wonderful supervisor. I think back and I realize I never knew what my budget was. I had no idea what my budget was, because I never asked for anything that I didn't get. I never asked for anything I didn't need either, so maybe if I had pushed it I'd have found out. The system and the people around there, when you ran into a technical problem it was a totally simple matter to find somebody who was one of the world's experts on that. They, to everybody, to each other, but also especially to young people, they were wonderful. I learned about collaboration, I learned about doing really good quality science at Bell Labs.

AK: This was in New Jersey.

JC: Yes, Murray Hill in New Jersey.

AK: So you relocated. You were living out in Illinois.

JC: I moved from Illinois to New Jersey, and I eventually got used to the narrower streets and the crazy drivers, but I think I was sufficiently aggressive that that part didn't bother me too much.

AK: And you were at Bell Labs three years?

JC: Seventy-six, '77 and '78 I was at Bell Labs.

AK: Tell me a bit then about the next transition for you.

JC: The next transition was pretty different. At Rockwell International in Anaheim, where we are right now, there was not even the major lab. Rockwell had its major lab up in Thousand Oaks which is about ninety miles from here, and they had a satellite laboratory, a microelectronics division that was on the grounds in Anaheim where the Minuteman missiles were made. It used to be a North American aviation plant and that had been bought by Rockwell and became part of the Rockwell International family.

There was a physical chemist there by the name of Harold Manasevit who had invented a new process for growing semiconductor layers using a gas phase process. Up till now everything I did at Illinois or at Bell Labs involved using a liquid phase epitaxial process, and it worked. It was simple, it wasn't very expensive, but it was not exactly amenable to high volume commercial, large area samples. It was pretty uneven. It was a great scientific tool, but had serious limitations as far as commercial potential, especially if things really could take off, as they obviously have for fiber optics and things like that.

Rockwell was not considered anything on a par with Bell Labs in terms of science, technology and all of that. I had some friends at Rockwell, one of them was Dan Dapkus who I knew from Illinois. Dan actually had the same Christian Brothers and grew up on the South Side of Chicago at a different high school a few miles from where I went to high school. Then he went to Illinois and then he went to Bell Labs.

I didn't know him well, he was a few years older than me, but he had decided to leave Bell Labs and go to Rockwell because it was an opportunity to take this new process, this new growth process and get in early on exploiting it. Dan is a brilliant and visionary guy. He stayed at Rockwell for a few more years than I did and he's been a professor at University of Southern California since then.

This process, called metalorganic chemical vapor deposition, MOCVD, was in its infancy and Rockwell was the place where it was invented. He went there specifically to have a chance to take this new technology and do something with it. I went there because he was there and I believed and trusted his vision of that material. Frankly, the people at Bell Labs thought I was nuts to leave a place like Bell Labs to go to Rockwell, which wasn't considered a peer group. Whether they were or were not isn't important.

What happened is that that part of that work did become important. Now half, roughly, of the compound semiconductor materials and devices made in the world are made using that process. It's still the commercial go-to technology for diode lasers and this is thirty-five years later. That's a long time for any technology to last. I went there to get in on the ground floor with that particular group. I didn't go to work for Rockwell in general necessarily, but that group and that effort was a real gem.

AK: Hindsight can be comforting, but at the time there must have been some risk involved for you in making that move?

JC: There definitely was. I was young and I didn't think about that. I didn't know about retirement plans or care about that stuff. I figured if it didn't pan out there would be others. The business was healthy. The others, the RCAs and HPs and Xerox, many of those no longer have labs like that now, but during the late seventies and early eighties that industry didn't seem like it was going away.

I wasn't too worried about Rockwell being such a failure that I would do damage to my career. In fact, Rockwell ended up wasting their opportunity by deciding to dismantle that group and relocate us in Thousand Oaks. At the end of the day most of the people were in that group left and Rockwell's not a force in the semiconductor, photonics or optical electronics business anymore. I did leave in four years time and it turns out I chose the University of Illinois, but there were other options.

AK: Had you gone to Thousand Oaks?

JC: I never went to Thousand Oaks. I was organizationally at Thousand Oaks for several years and I still have on my bulletin board a Thousand Oaks ID badge. But I never physically left because the equipment wasn't easy to move. By the time we planned things out and got ready to move the equipment, I just moved to Illinois.

AK: Not to make too much of it but as you were describing your interest in this metalorganic chemical vapor deposition, what's the right word, technique, technology? I was reminded a little bit about how you were taken with the tubes in the radio, that there's something there you just really wanted to find out about.

JC: I don't think I ever made the connection between the tubes in the radios and MOCVD. But I think your observation is right, that there is that connection of deciding something is interesting, not necessarily of value, that would great too, but absolutely interesting and wanting to know everything about it. There are many other examples of that. I'm a bit OCD about things. If I decide it's something I want to know about, I really want to know everything about it, and that was certainly true for MOCVD and that aspect of crystal growth.

AK: I think a certain amount of self-deprecation is acceptable with OCD, but it also strikes me as a real intellectual determination, to follow up on an interest. (Laughter.) This was a habit with you, and you followed it out of Rockwell to the University of Illinois.

JC: Yes. By the early eighties it was pretty clear that MOCVD and the other technologies, to be fair to the Bell Labs people, MBE, molecular beam epitaxy – by itself, that's an interesting story. MOCVD and MBE, invented in different places at almost exactly the same time. The patents were in process in the patent office at the same time. It's rare to have two superb technologies that are completely different occupy more or less half of the industry each and go on for thirty-five years with no sign of something coming along to bump them off.

But they weren't in universities much. They were at the high end of the ticket price for admission. Commercial equipment was not particularly available, so you couldn't buy an MOCVD or an MBE. You had to hire and MBE guy or an MOCVD guy. I think that's what I brought to Illinois. I was bringing the technology and then we built out of parts our first two MOCVD machines because there was nobody who could make them for us. Nobody was selling them. There wasn't a place you could place an order.

In the first years when I started at Illinois in '82 my students and I were welding stainless steel tubing to make up the first MOCVD reactor, which not surprisingly looked a lot like the last reactor I worked on at Rockwell. That's what I was doing was transferring the technology effectively. So that's correct. Bringing the technology was probably the main focus.

AK: Again, you decided to go into a university setting rather than some other setting. Tell me a little bit about your thinking in that regard.

JC: I may be embarrassed to admit this, but the thinking in going to a university was possessiveness about the work that I did. I liked the idea of being able to publish and have my name on things in the public domain. If I left Illinois or I left Rockwell or I left

Bell Labs that the record would still be a public record of what I did. I don't know why I feel that way, or why I did feel that way, but I definitely did. I did not want to be in an environment where my good work was lost in the anonymity of the employees at a specific company. Illinois gave me an opportunity to do that.

Any university probably would have, but Illinois at the time already had a distinguished history in the semiconductor business. They had hired John Bardeen, the co-inventor of the transistor, to set up a semiconductor program in 1950 when most universities didn't have anybody like that on their radar screen. They were continuing to grow; they had interest in investing in it, and in the first ten years I was there, we went from a 100-year old building to a new building, new laboratory building; new facilities.

There was an excitement there and Illinois has, like the other top engineering schools, this never-ending stream of brilliant young people who want to come and work there and they all make us look really good. So it was easy not only to get the resources I needed but the students, the human resources you have to have as a professor to be successful in a technical career.

AK: Your observation about sort of personal recognition for the work you were doing, I've heard it said that scientists are often not motivated by financial gain. The primary motivation is in fact the personal recognition that one gets from discovery or invention or creation.

JC: Absolutely, that's a factor most of the time. I don't know if it's for all of us, but I know it's true for the most successful. They want to do good work and they like what they're doing. They want to understand how things work. There's a difference between what you learn and you understand and what you teach the rest of the world and that's the part that gets you the recognition or the visibility. There are some unbelievably brilliant scientists who were happy to learn it. Once they learned it then they would just go on to the next thing.

A good friend of mine Karl Hess, who's now retired, would refer to that as killing the mouse. You get the mouse cornered, and some people are happy, say okay, well I got the mouse cornered, and others want to kill the mouse. Those of us who seek the recognition or want that public thing added that bit at the end. Once we figured something out, then we wanted to make sure the publications, the patents, the presentations, the conferences, that those things are just as important as getting the understanding or the knowledge.

AK: Spreading the information.

JC: It's not just to learn something; it's to teach somebody else. There's a level of excitement with knowing you figured out something that nobody's been able to figure out before. On the other hand, it's a tempest in a teapot, if you don't put that out and give the presentation or write the paper or submit the patent disclosure and those things. Don't get me wrong. I provided the energy there. The intellectual part still requires lots of people.

There are many scientists, Nick Holonyak is a good example of one, who was very single-minded and had so many ideas that it didn't matter whether his students had good ideas. If they just hung onto the bus, good things were going to happen. I had a lot of good ideas, but my students made all the bad ideas into good ideas or helped sort out, "You can't do this." I couldn't do this without special colleagues, but the students who really did this kind of work.

AK: It's an interesting observation about how research works in a university, that you get this magnifying effect with all the students who are taking projects that perhaps you thought of and they spin them off in various directions that, who knows where it goes?

JC: Students play a special role in their innocence, because they don't know what they can't do. The older you get as a professor, the easier it is for you to talk yourself out of things because you think, well, that's just crazy. The smartest thing you can do is not tell the students that they can't do something. Let them think they can do it, because they probably will fail, you probably do have the right instincts to know that it's not going to work. But that's only what they're specifically doing.

In the process of specifically doing what you're thinking at the moment won't work, they're going to try some things that will open up some new surprising areas. If you recognize that, and you aren't wedded to your own ideas, you go in that direction and can be opportunistic and take advantage of this new concept. If that's a better one, it's okay with me; doesn't have to be my idea.

AK: Your career from going back to the seventies till now really corresponds with the whole fiber optics revolution. Just looking back, a stunning amount has happened. What a field. Could you tell me a little bit about, it's a big question, but the areas that you have worked on that you think have been sort of milestones or significant contributions in that explosion of knowledge?

JC: The interconnections that I think you're asking me about, I think that the opportunities I've had have arisen because I've been in the open area between materials and devices. It's a mass and spring problem. When you develop something new in materials it triggers some opportunities in devices. But the device also works too because when you want it to do something and it can't, you start to consider other materials.

There are people on either side of that, people who are purely materials or purely devices or mostly one or the other and I think I'm more devices. It's more interesting to me. I want to do enough materials to move on and put it into the devices, but I wasn't afraid either to try the materials things. So the milestones come in taking risks. They're not really risks, it's trying something that you have reason to be suspicious won't work, so it's not a real risk in that sense. But trying some things that maybe people don't expect to work, or even you don't expect to work, but taking a fly at it and see what you can get.

When I was working on the lasers at Bell Labs, those materials were never going to be satisfactory. Growing with liquid phase epitaxy, I just could not see that, no matter how good a device I made. Even the rest of the devices on a wafer weren't exactly the same, and so this MOCVD process – at that time they were growing on two-inch sapphire wafers – it sounded phenomenal. That was like ten times the size of what I was able to do with LPE, whereas gallium arsenide wafers are much bigger than that now.

That was the risk, was trying this new material system. Once we found that the new material system was working and controllable, and controllable at a more nearly atomic level, then that led to practical examples of quantum wells, and quantum well lasers are now part of this. Now, that was certainly not me. I was in the boat with 500 other people pulling on oars, but the quantum well became to the point where there aren't any lasers that aren't quantum well lasers in the semiconductor business.

These things come back and forth. We started working on novel materials to do integrated photonic devices. That led to a new way of growing things using selective epitaxy and that's a parallel development that we've been working on now and even today we're making selective area epitaxy; we're just doing it at the nanometer scale instead of at the micron scale. The same is true for the strain layer work that is recognized in the Tyndall Award this year. We were trying to develop a material system to move into a wavelength range that was not available from lattice-matched materials so we took a chance on something that most people would have thought is not going to work.

AK: I think SPIE, the International Society for Optics and Photonics, honored you with a technology achievement award in 2011 for your work on selective area epitaxy.

JC: Yes, SPIE gave me that award, and we have almost twenty years of work on it and we tried all kinds of bad ways to do that. But the idea was to address epitaxy. We wanted to be able to do what the silicone industry does, which is put a transistor, a transistor, whatever, on a wafer and then cut it up later. Most laser diodes now are all the same and you've got a single device when you cut it up. What if you could put some of the functionality onto chips? Well, Infinera and others are doing that now and they're a major force in the market.

We tried some ways using lasers to stimulate the epitaxy and learned a lot; mostly learned it wouldn't work. We did some shadow masking with silicone dioxide and we found that worked better, and then as we developed it now it's used in lots of technologies. Again, we weren't the only ones by far. There were many others who had some major contributions to that work.

AK: You talk about trying something out with the thought in the back of your mind that it might not work, or even probably won't work. You're raising in my mind the tension that can often exist, particularly under budgetary situations with the tension between basic and applied research, and how sure do you have to be of the applicability of something, maybe even particularly the commercial applicability of something, versus what you seem to be describing, which is go see what makes it tick.

JC: You point out a never-ending tension in our lives which is the intersection between accounting and science, where science is gambling and risk-taking and intuition, all of which are not easily explained to someone who's looking to account for your effort or milestones or things like that. Albert Einstein said if we knew what we were doing they wouldn't call it research, and that's the truth.

How do you handle that? Actually, I think most of us do it the same way. We do low risk things for part of our time that aren't very interesting and we hang in there and do it because they'll meet the milestones. Every chance we get we risk this other stuff. The truth is that the accountants don't even know we're doing that, because the risky stuff that turns out to work becomes a different proposal for a different program with milestones on it later in time.

Every university professor, every industrial scientist has got their stuff in their pockets that they're working on, that they're not talking to their management about until it starts to work. That's the only way you can creatively manage that tension. If you only do the milestones you inch along. Nothing revolutionary comes from that.

AK: Then I suppose there's perhaps some middle ground where you can't see anything immediately applicable, but you can imagine that maybe one day there could be. You said something in relation to, in an interview I came across, quantum lasers, that by the laws of quantum mechanics, or the principles of quantum mechanics, a piece of information once sent out, upon reception is by definition destroyed. What an interesting concept for security applications.

JC: That's exactly right. That's the single photon process, the active detecting of photons by – you can't detect it unless you destroy it. If you're sending out photons and they're not being received at the other end they know somebody's listening. How do you use that as encryption? In fact, the ultimate encryption can be done using quantum interference because you send out two pairs of identical photons. If the receiver gets the two photons and they don't match, there's been interference because no one can replicate the photon by the laws of quantum mechanics. The usual way of doing this is I send it, you intercept it and you just resend it once you know what it is. Well, you can't resend those two photons that I sent by different paths. You can only send one and it's not going to match the entangled photon on the other side.

That is fascinating, almost meta-science because it seems so Star Trek or something like that. But there are people demonstrating this on tabletops and whether it becomes a practical system or it doesn't have other limitations remains to be seen. It certainly is, if I were doing a PhD, I think I'd be looking for somebody doing something like that to try to work with.

AK: This gets really fascinating, this business of devices and materials when you get down to the nano level. Another thing that you said I came across was that each time we move downward in size, we really move upward in complexity.

JC: Actually, my original thought on that is actually from a colleague who said I notice that you have this microelectronics laboratory and you're going to change it to the nanoelectronics, nanotechnology laboratory, but you have to build and add another $10 million worth of construction. How come you need a bigger building if you're going to make things that are smaller anyway? That is the truth, because when you go down, it doesn't have to be even near the atomic size, but when you get to clumps of material that are tens of nanometers which may have 10,000 atoms in there, 10,000 atoms is a small number compared to what we typically do even with the fastest integrated circuits.

If you have a few percent error in 10,000 atoms you're going to start noticing it quantum mechanically in a way where you never would in a previous generation. That's an imposing constraint on the people who are trying to make the bits of material or the small devices. I mentioned Dan Dapkus, and I told him I was thinking about how could we make a single photon generator and he of course knows that an average laser has millions of photons. He says, I'll tell you what. I'll give you something that'll make you a million photons. You pick the one you want, knowing that I can't do that.

It is absolutely true that with the complexity and the cost of our tools, we are moving into a new era. We used to make things with one tool and characterize it with another. Now the characterization *is* the assembly, and people are using scanning tunneling microscopes not to just go down and have a look at the atoms, but when they see the one they want moving it over into some other place. You can't really separate anymore the assembly from the characterization.

That's ordinary. That's even true in everything we do today, that you can't distinguish anymore between what's computation and what's communications. We used to have the computer and then we do stuff and communicate to some other computer. But now the communication even within the chip or between chips on a board is part of the computation. We can't neglect it anymore, we can't talk about computation as something we do and then shift the results off. That's very primitive compared to what we do in building an integrated circuit or building a fiber optics link or a telecommunications link, or connecting to the cloud.

AK: I want to mention the work that you are being recognized for with the Tyndall Award and have you tell me a little bit about that. I'll just quote from what I've read about the award, "It's for your contributions to semiconductor lasers and photonic materials, the processing and device designs, including high reliability strained layer lasers." It also recognizes your "research on the development of semiconductor lasers grown by this MOCVD technique." It was Rich Linke, the executive director of IEEE Photonic Society, who said, "Jim's breakthrough research demonstrated the reliability of strained layer lasers in conditions previously thought impossible."

JC: That's almost completely true. In the era where any of us started working on semiconductor lasers, beginning from the first double-header structure lasers in around 1970 up until the late eighties, there were three basic rules. One is, death, taxes, and the third one is that your layers must have the same lattice as the substrate and each other. Lattice matched; everything must be lattice-matched.

The metallurgical observation behind that was very clear. That is, any time there was a departure in solid materials from lattice constant, the mismatch would yield so much strain that that strain would yield dislocations and defects that were unacceptable and incompatible with either electronic or photonic devices. So it was a mantra. You could do whatever you want with any materials you want, but they had to be lattice-matched.

A couple of visionary people had predicted a different outcome in a theoretical way. One is Eli Yablonovich, who is now at Berkeley, but he was at Bell at the time. Another was Alf Adams at the University of Surrey. He said, look, semiconductors are not exactly inflexible. They have a tiny amount of flexibility. They have the ability to distort a little bit before the dislocations form, and if you could make your layers thin enough then the defects won't form. There were some early papers by them, but they were theoretical papers and they got very little traction because the simple answer was at that time we weren't making layers thin enough so that you could observe this.

In the late eighties, we got a new MOCVD reactor in my group at Illinois. We'd moved into a new laboratory complex and we were shaking it down. I wanted to calibrate a material, add indium to an otherwise gallium arsenide base structure. I knew that it would be strain material and I knew that the strain material would end up with dislocations. But what I wanted to do was make some of it just so I could calibrate growth rates and do some basic materials work on it.

So we wanted to measure the wavelengths and the thicknesses, and the thicknesses at that time was a tour de force in transmission electron microscopy to measure it, but we had colleagues who could do that. We wanted to measure the composition which was even harder to do chemically so I said let's make them into some lasers. They won't last long, but if we measure them fast we'll at least be able to tell from the emission wavelength what the materials were.

The students put together some of these lasers and we expected them to be worse than flash bulbs, go fast, but I figured if we were clever we could capture the information before they died. But they didn't die; they not only lived, they lived as long as we wanted them to live. They were no problem at all, and more than that they had characteristics that were better than comparable lasers with only gallium arsenide in them. There was something about the indium that not only was not killing them but was allowing them to have really high characteristics of parameter space that allowed them to be lower current and higher efficiency. They were better lasers.

Without going through the details of how we got there, what we realized is that the few people who had tried to do that hadn't really made them thin enough to observe what had been predicted earlier theoretically. What we started doing is saying okay, well that's great, but they won't really last long enough to be a commercial product. By coincidence we had a pretty good working relationship at that time with a company, McDonnell Douglas, that had bought a startup in Elmsford, New York. Most of McDonnell Douglas was an aircraft company in St. Louis. But they had a startup, a small semiconductor laser company in Westchester County just north of New York City.

We started working with them for other reasons. They wanted to learn about MOCVD and we had worked with them to transfer that technology. They said well, we have lots of ability to measure the lifetime reliability in these things. Let's put some of these things on there. They started taking our lasers and doing life tests on them. What we found out was that they were not only living as long as other lasers, they were living longer than other lasers.

Absolutely counter to your intuition or all previous suggestions was that the laser would work, that the strain effects can be in a metastable state that's stable enough that they will last a long time even under operating conditions, higher temperatures, higher current and all of that. That alone would have been interesting, but completely separate from us there was another group of people that started working on putting erbium into optical fibers to make an optical fiber amplifier. Instead of having a detector and a bunch of electronic circuitry and then another laser to do a repeater, you could just take a piece of special fiber and the light pulse coming in would come out amplified.

The reason it was able to do that is you would pump it with a high energy light, and then that would set up this fiber as an optical amplifier. You needed to have pump lasers for that special erbium built fiber. It turns out that was in a wavelength range that was not available for gallium arsenide. The recipes to success are trying something that might not be able to work, then getting some good collaborators to test it; but it really helps to have a killer app out there that you didn't even know about. The EDFA, the erbium built fiber amplifier, now is a critical component. There are probably fifty companies over in the convention center who sell them or sell the components for them. The 980 pump lasers that we could make by using strain layers are essentially not available for many other material systems.

So the breakthrough was not one of these intentional things. This was clearly serendipitous. We were trying to do one thing that was meant to be a transient, and when it worked so much more favorably, the smartest thing we did was not dismiss it. We realized that it was worth taking another closer look and figuring out what it was.

AK: Did what you find square with theory but not accepted wisdom, or did it also not square with theory?

JC: It absolutely squared with the theory, but the theory never addressed the issue of reliability. It just said look, this is the physics and this is how it should operate, and the issue of reliability, long-term lifetime, all that was not even part of the discussion. The conventional wisdom said that we know we can't make lasers with strain in them because they fail, so we won't make lasers with strain in them and then they still might fail but at least they won't fail as quickly as if they have a strain in them.

The theory is completely right. In fact a couple of papers that were written have it exactly right and there's no reason to improve them. There are some minor changes but the two separate groups coincidentally published these papers within weeks of each other, completely separately. That's another interesting thing that's always amazed me about our areas. You can think you're way out in left field and there's enough other brilliant people out there, some other guys is thinking the same thing at exactly the same time and yet you never communicated or even knew about it.

So the theory was right, but neither of those groups thought anything about addressing the issue of reliability and we did it to start with because it was part of a collaboration we had going anyway, and secondly, if this application was to be effected they had to be long-lived lasers. In part, those lasers often live underneath the ocean. The repeaters that we drop in the ocean require that the lasers last a long time.

AK: Those are the ones that you, the strained lasers.

JC: They're used everywhere, but in particular the transoceanic cables. They're hard to go and repair.

AK: The interface between the science and the engineering, to the degree that that distinction is worth pursuing anymore I don't know but it's fascinating. But I want to leave that area just for a minute and ask you a little bit about your perspective on the field as president of the Photonic Society in 2010-2011. It's the same field but I would imagine a different perspective on the issues? What did that give you, that experience?

JC: The experience of being president of the Photonic Society has multiple facets, most of which are extremely positive, though it's also a time commitment and cuts into the other things that you can do. Was it worth it, absolutely, no question about it. The time I invested was completely worth it, and further I strongly believe that all of us who are professionals owe something of our time and something of our fortune to be returned to that profession which has treated us pretty well. I think every one of us has to give back something in some way to their profession and there are many different ways to do it, and volunteering for a professional society is just one of them.

The business of photonics has changed, and as you said, my career about matches the duration of the business. It really was a laboratory curiosity while I was in college, and now we've already gone through the big bubble. We had this unbelievable crazy growth in the nineties, and then the bottom dropped out in the early 2000s. Now we're back to growing in a more sane and sensible way. Smartphones and video on demand and many of the things that are consumer technologies are bringing things back.

I think back to the nineties and I'm surprised that we didn't see the bubble coming because some of that growth had to be unsustainable, but you get caught up in it and it's pretty exciting, and there's a lot of money involved. Our business has matured. I can't capture for my students the vista I had as a student. I was working on something that didn't really have a large marketplace, and maybe had some hope, but it was anything but certain. There were many large companies invested in small ways while they made their money on other things. Xerox may have been interested in semiconductor lasers for printing but they were selling a lot of copiers that didn't use lasers as the print engine, same with the phone company, selling land lines, but looking for fiber optics.

It is different now. It's a business, and it's a big business, and it's dominated by small companies that sometimes go away and sometimes turn into big companies and sometimes get swallowed up by big companies. The students who work in the discipline are facing a very different set of players, and it's not even the same as it was five years ago. It's not going away. There's going to more and ever increasing need for bandwidth and bandwidth comes from us, from photonics.

Some of the basic things should never change, and I try to do this with my students. They don't leave with factual knowledge; that's transient. What they need to leave with is a toolbox, and so I try to get them to do some engineering and some science. You started out by mentioning the intersection between science and engineering. The best scientists understand what engineers do and the best engineers understand what scientists have to do. I've intentionally tried to have projects in my laboratory that are more immediate, call it engineering, and more far out, call that the science, but try to make sure that understanding is what leaves with the student, not just recipes or things like that.

I don't know what I would do if I were a student, but the intersection of materials in quantum mechanics and electromagnetics still is absolutely interesting to me. It's a long way from over so I wouldn't tell any student that there's a better chance somewhere else. There may be a better fit, but we're going anywhere anytime soon.

AK: It's been a pleasure listening to your story, a very, very interesting one, very engaging. I appreciate your spending the time with us. If there's anything we didn't cover, and there may not be, but anything else you would like to say?

JC: I can't think of anything specific. It's interesting, you start as a young person and you hope to get an abstract accepted at a conference, or you hope to get a paper printed. Then you start doing invited papers and talks, and then review papers and talks, and then they start talking to you about history stuff. Then you realize that you're getting old. That's okay. I've got no choice but to get old, but I still like going to my job every day.

AK: Well, one can do it gracefully, I've heard.

JC: We'll see. That experiment's still in progress.

AK: Thanks very much.

[End of Interview]