

Kumar Patel

An Interview Conducted by

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23 February 1995

Interview #242

For the

Center for the History of Electrical Engineering  
The Institute of Electrical and Electronics Engineers, Inc.  
and  
Rutgers, the State University of New Jersey

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Kumar Patel, Electrical Engineer, an oral history conducted in 1995 by Frederik Nebeker, IEEE History Center, Rutgers University, New Brunswick, NJ, USA.

Interview: Kumar Patel  
Interviewer: Frederik Nebeker  
Date: 23 February 1995  
Place: UCLA

Nebeker: This is 23 February 1995. I'm talking with Kumar Patel at his office at UCLA.

This is Rik Nebeker. So you were born the second of July, 1938. What was your family background?

Patel: My father was a civil engineer in government service in India, and my mother was a housewife. I have one older brother and one younger sister. My older brother is a medical doctor, and my younger sister is a high school science teacher, so there is some connection with science all along. I suspect much of this came from the background in which I was raised Ñ in which we all were raised.

Nebeker: Your father, I assume, was interested in science.

Patel: That's where this whole thing started.

Nebeker: Was it clear at an early age that you would go into science and technology?

Patel: I'm not sure what was clear. I think the only thing that was clear to me was that science and math were the easiest subjects. Everything else was hard. Just in terms of the time it took to understand the concepts and start working with them, science and math were the easiest topics. You didn't have to read a lot; you just had to understand the concepts!

Nebeker: What was your education? Did you go to public schools?

Patel: Yes. I went to public schools through high school. And then I went to college in India and, of course, most of the colleges are public colleges. At that stage there is very little difference between public and private. I went into engineering, probably because in the Indian structure of education, at least at that time, you

didn't have to be very smart to get a degree in physics, but you had to be very smart to get a degree in engineering.

Nebeker: Is that right? Some places it's just the opposite.

Patel: Yes, some places it is just the opposite.

Nebeker: Why was it this way in India?

Patel: Supply and demand for the graduates in different topics and different fields, and the limited capability at that time for the country to provide that type of education.

Nebeker: I see.

Patel: So if you wanted to go to engineering school you had to be in the top ten percent of your graduating class. If you wanted to just get a degree in sciences, you didn't.

Nebeker: Is that why you chose engineering rather than physics?

Patel: No, no. I was always interested in practical aspects of science as opposed to science for its own sake. And so this was not an issue as far as I was concerned.

Nebeker: So that was at Poona University in electrical engineering?

Patel: Actually the subject in which I got my degree was called telecommunications engineering. The distinction there was made on the basis of electronics versus power engineering. Electrical engineering was mostly power engineering; electronics wasn't telecommunications. I got my degree in telecommunications engineering.

Nebeker: So why did you choose that over power engineering?

Patel: Again it was the matter of supply and demand. It was a new field back in the mid-fifties. Electronics was just becoming a significant voice in society. Not that

power engineering was not going to be important, but this was a new field. So I decided to go into that area, and, again, it was a question of supply and demand.

Nebeker: You hadn't been in amateur radio, for example, as a youngster?

Patel: No, no. I wasn't. I'm sorry not! One of the interesting things I find amusing was that both the school that I decided to go to and the subject matter that I decided to study there were determined principally by how difficult they were to get into. For example, each year this college admitted one hundred new freshmen engineering students, and the number of applicants was somewhere close to a hundred thousand. This was an insane kind of thing! The point was that this was so difficult to do that it might be worth doing! But I had absolutely no desire to pursue engineering or science as my career. That was not in my initial plans.

Nebeker: Is that right?

Patel: My older brother went into medicine and my dad was an engineer, so I said I was going to do something different. This was only a stepping-stone to where something else was. And the something else in my mind was to go to become a career foreign service officer. The unfortunate part was that after I graduated with my bachelor's degree in India, I found out that you had to take a competitive exam for entry into that service, and you had to be twenty-three years old. I was four years too young to take the exam. So my dad said, "It's not bad. There is something you can do in three or four years: you can get a Ph.D." So I said, "All right, I'll do that. I'll do something for three or four years." And so that's how I came to Stanford. It was a stopgap measure to do something worthwhile in

between time.

Nebeker: So telecommunications engineering was a valuable steppingstone to the foreign-service position?

Patel: No, it wasn't. It was just something worth doing; it was sufficiently connected with mathematics and science which I liked; it was sufficiently connected with things of interest to me.

Nebeker: But if your long-term aim was foreign service, one would think there would have been another line of education you would have chosen.

Patel: Perhaps, yes, but the rest of it could be learned. I was having fun doing [engineering] at that time.

Nebeker: And you were good at math and science.

Patel: That was all. I was just going to do it. That's the reason I came to Stanford, not because I was going to become a career physicist. Obviously it was in the back of my mind that at the end of the Ph.D. I would go back to India, take the exam, and become a career foreign service officer.

Nebeker: That was still your intention?

Patel: Then things began to happen and I realized that doing science was fun! And that it was more fun than doing almost anything else that I could possibly imagine.

Nebeker: Was it difficult to get to Stanford?

Patel: No, not particularly. My dad said, "Look, I'll support you for one year. And then you are on your own, and if you can't hack it, come back and get a job here."

Nebeker: You were admitted without any difficulty?

Patel: No. There was no difficulty. But as I mentioned, in the electrical engineering

department, not in physics. What I wanted to do physics couldn't do for me. I wanted to work in the area of new microwave physics and new microwave technologies.

Nebeker: That is something you had gotten interested in as an undergrad?

Patel: Yes, as an undergrad. So I wanted to pursue that, which was all in the EE rather than physics.

Nebeker: Were there particular professors you wanted to work with at Stanford?

Patel: No, no. I was very naive. Again, the notion was to find the most difficult places where you could possibly work and to go there! And there are two advantages to that reasoning. One, you will be mixing with students who are of very high caliber, and, two, you'll be taught by professors who are very knowledgeable.

Nebeker: That was in 1958 that you went to Stanford. How was it there?

Patel: It was difficult. Coming from India with what I consider probably a very good education and being, at least according to the critics, the best student in all of India, it was still hard, because the system worked differently.

Nebeker: Was language any problem?

Patel: No, language was no problem, but the fact that things at Stanford happened every three months was! I mean, you took a course and an exam every three months. You had to work all throughout the year, as opposed to the old British system in India, which is once a year! And so there was no time to rest! You were just constantly going. You know, it took me one quarter to figure out that this is a different way of life!

Nebeker: And the courses were challenging for you?

Patel: Yes, the courses were challenging, but, as things go, it worked out well. After the first year I had a research fellowship.

Nebeker: What professors did you come to work with there?

Patel: I worked with a man named Dean Watkins, who was in the process of setting up Hanson [Company] in Palo Alto at about the time I got there. I happened to be the last graduate student he graduated. Or rather, that he took in. There were a couple of old hammers who graduated later on, but I was the last graduate student to start working with him.

There are a few things which are of interest, and that is something that most people don't give much credit to in their graduate education, which is that we often hear graduate students complain that they don't get enough direction from their faculty supervisors, and the example I cite here is of myself. In the two years I worked for him, I probably saw my thesis professor for a total of an hour or an hour and a half. That was my sole contact with him. The first day I started with him, he gave me a pile of stuff and said, "Go read this stuff and figure out what you want to do for your thesis." And so I went back a couple of weeks later and said, "I don't want to do any of that stuff. Here is something I want to do." He looked at it and said, "That's a good idea, but I can't help you. You need to find somebody else who can advise you." What I wanted to do was in the solid-state part [of electrical engineering] rather than vacuum tubes, which was his expertise. But he said, "I will tell you where to go. And so make sure that whatever you want to learn you'll be able to get from him." So that was my first conversation with him.

Nebeker: But he remained your thesis advisor?

Patel: Oh yes, yes! He said, "If you're not going to have a problem I'm not going to have a problem. And I said, "Fine, you give me an office, you provide me with resources to finish this, I have no problem with it!" And from that point of view he was very good; he never once questioned the resources I needed. He said, "If that's what you need, go ahead and do it." The second time I met him, he called me and said, "I hear from this person [whom he had sent me to for advice] that you have made enormous progress. I want you to tell me what you have done." So I told him what I had done; it took me about an hour and he said, "I think you're done." This other person had called my thesis professor because I had been asked to give an invited paper at a major conference, and he wanted to tell [Watkins] that this was what had happened.

Nebeker: Who was it that you were working with?

Patel: A fellow called, I forget his first name, Mattei. He was with SRI at that time. And so that's how the loop got closed. So that was our second meeting. Our fourth meeting was after everything was done and my thesis results were finished. I was writing the paper for the conference, and I put his name down on my paper and gave it to him to read. And he said, "No, you can't put my name on it. I didn't do anything." I said, "I hope you don't think this is a terrible paper and that's why you don't want your name on it." And he answered, "No that's not what it is! There's something about science; if you don't do it yourself, you can't have your name on it." He taught me two very important things. First, that the

most important thing in science is to have a taste for topics you choose to work on, and second, you must have integrity while doing science. And those two things I have never forgotten. And I think most young people these days don't learn those things from the faculty members.

Nebeker: You're saying maybe they get too much direction?

Patel: Yes, they get too much direction. The most important part of doing science is to have taste and choose good problems. I am willing to admit that most Ph.D.s from good schools have enough scientific or technical knowledge to solve any problem that's given to them, assuming it's not something off-the-wall. But what distinguishes the good from the best is that the best ones can themselves pick problems which are important rather than waiting for somebody to come up with a problem for them to solve. You know, Watkins gave me a pile of stuff and said, "Go read." And I came back with something completely different and he said, "It is a good idea. But it is outside of my field of experience." So many young people don't have that opportunity to start thinking on their own early. What happens more often than not is that they wind up working for professors' ideas.

Nebeker: But it's also extremely important to be able to judge what is feasible. And a person beginning his career may not be in a position to do that.

Patel: That's absolutely right. And I think that's why when I talked to Dean Watkins about what I wanted to do he said, "Look, this sounds good, but you've got to go talk to this other person."

Nebeker: Somebody who can say, "This is just too difficult."

Patel: Too difficult or too trivial. So in some sense I think he provided all the right kind

of advice, which is that the person to whom you are most immediately tied because of the structure may not be the right person to advise you on certain things. The real expert may lie somewhere else. You shouldn't be bound by the structure. Picking a problem is very, very important.

Nebeker: Were there any other Stanford professors that you felt influenced you?

Patel: No, I think in some sense I really was on my own, and so you can almost argue that I started my scientific career not at Bell Labs, but at Stanford. It's entirely possible that had I picked a different thesis professor, I might not be here; I might be back in India. I might have worked on someone else's problems and might not have been turned on by what I thought was the excitement and beauty of doing science.

Nebeker: How did you come to have this interest in the solid-state electronics?

Patel: I didn't! What was given to me here just didn't make sense, so I did some reading on my own in the general field of microwave science and microwave technology. I found out that the magnetic resonance in some of the ferromagnetic materials had some unique properties that just had been uncovered. My view was that here was something that one could build on. And I knew what I didn't want to do!

Nebeker: And you'd seen something that piqued your interest?

Patel: Yes, it piqued my interest, and I think that that, in some sense, probably is more or less the way my career has evolved. I finished at Stanford and went to Bell Laboratories.

Nebeker: You got your degree in 1961?

Patel: Yes, in 1961. I went to Bell Labs for the interview in April 1961. You know, you

tell them what you have done and you tell them what you want to do. The person who eventually hired me asked me, "Well, what do you want to do?" I said, "Look, I know what I have done, and I know what I am good at. Solid-state microwave seems to be something I can work on. But," I said, "Nobody should ever do their thesis over again. And if I were to come here to do [solid-state microwave], that's what I would do! Let me propose to you what I want to do. What I want to do is to do laser spectroscopy, spectroscopies in lasers." Lasers had just been invented a year before, or in that timeframe. So this man looks at me and says, "What do you know about lasers?" And I said, "Nothing. I know what they are, but I haven't got the foggiest notion how you go about doing spectroscopy because laser which can be tuned in its frequency so you can look at its spectrum of resolution which is much higher than anything else." He replied, "It's a good idea, but it will take you a long time to get there." I said, "I've got time! Where am I going to go if you give me a job? I have all kinds of time!" Anyhow the upshot was that after three places that I interviewed, Bell Labs was the lowest paying.

Nebeker: Where were the other places you interviewed?

Patel: IBM and Raytheon.

Nebeker: Did they want you to work on particular projects that they had?

Patel: Yes. And Bell Labs was the lowest paying, but Bell Labs was also the place that was willing to give me the freedom that I was looking for, and so it was quite obvious that that was the place that I should go.

Nebeker: Now at that point you had decided that this was what you wanted to do for a

career?

Patel: That's right. I think by that time it was clear in my mind that this thing really turned me on, and I think that, again, as mentioned earlier, being able to choose my own problems probably had a lot to do with it, rather than working on somebody else's.

Nebeker: Who interviewed you from Bell Labs?

Patel: P.K. Tien was the person who hired me eventually. He had been part of the team that interviewed me. He too was a Stanford graduate, but he had finished several years before I got there. It's entirely possible that he took pity on me because he was Dean Watkins's colleague; they were in the same graduating class. [He thought,] "This guy's crazy! Dean Watkins is crazy, letting him do what he wants to do. Something good will come out of it!"

That's how I got started in lasers. I knew what I wanted to do; I just didn't know how to do it.

Nebeker: How did spectroscopy get your attention?

Patel: It caught my attention only because of the fact that everything I had read about lasers convinced me that the kind of resolution we could achieve, assuming you can have a laser light, would far exceed what you can get from using traditional sources, using spectrometers and everything else. So this was clearly going to open up a new field of studies.

Nebeker: Now masers were six or seven years old at the time....

Patel: Yes, and we had seen some of the things that had happened using masers, though not necessarily spectroscopy because masers were also not tunable. They are

fixed frequency. Spectroscopy with masers had not been developed. And so this was very clear that, you know, here is a tool. And if only you could make this tool do something slightly different--you could have a field to yourself for a while and do really exciting science.

Nebeker: And your objective was the scientific one, to be able to do a much better spectroscopy.

Patel: That's right. At least at that time.

Nebeker: And where in Bell Labs did you work?

Patel: This was at Murray Hill.

Nebeker: How were conditions there when you arrived?

Patel: If you asked me at that time, you would probably have gotten a more correct answer. I don't know how the conditions were then, but it was very clear that it was about as vibrant a scientific enterprise as I had seen anywhere in the following way. The institution was committed to basic research in a finite number of fields, lasers being one of them, because the institution was convinced that something of value would come out of that to further their own business. But the way to get to the end point wasn't by defining what that end business was, but by letting people do the kind of things that were necessary to provide the building blocks in order to get to the final stage. And so from that point of view, what Bell Labs had then was strategic research in its finest form. My definition of strategic research is research in those areas which are strategically important for the long-term commercial interest of the corporation. Communications was their business. As years had gone by, the frequency spectrum in which

communications were carried had kept on going up, so it was quite obvious that after the masers the next thing was going to be the optical spectrum. So lasers were clearly, clearly important to work with. There were a number of very young people there, people just about my age because everybody had been coming out of school about the same time. These were very smart people. All of these people have gone on to become very great people, great scientists. It says something about the quality of people Bell Labs could attract at that time. The year in which I came to Bell Labs, Arno Penzias, [George Ming], [Govenlik], and two or three others were also there. All of us have gone on to do great things in the fields of either spectroscopy, lasers or what have you. I think that of the years of Bell Labs, probably 1961 will go down as the greatest year in terms of the new recruits. This had something to do with the fact that this was four years after Sputnik. The country had gone through an enormous thrust in investing in science and technology, and this was the first output that was coming from that.

Nebeker: Was it an unusually large incoming group in 1961?

Patel: No. Probably I think in all of [the recruits to] Bell Labs no more than fifty or seventy people had Ph.D.s, and officially they had ten or so in research. So it was a small group of people, but it was clear that the company had done its homework and that the recruiters had done their homework. They picked good people.

Nebeker: Were you on your own there at Bell Labs?

Patel: Pretty much! Just as it happened at the university, after I got there, my department said you've got a month....

Nebeker: Who was your immediate supervisor?

Patel: P. K. Tien. He said, "You've got a month to look around. Talk to as many people as you want; you can use my name and say I sent you to talk to them. I'll tell you where the library is and then you're on your own! Come back in a month and tell me what you want to do! I can tell you what this company's business is, so if you come and tell me you want to work on something which is way outside of the framework of reference, I probably will say no, but other than that you're on your own!" And so a month later I came back and told him that what I wanted to do was to find ways by which one can expand the number of laser transitions which had been seen in the gases. At that time there was just the helium neon laser working at 1.1 micron. There were three or four very closely spaced transitions, but that was about it. I said, "You know, if I understand atomic physics at all, there is nothing sacred about helium neon, and so there must be tons of stuff underlying and therefore some of them will have properties of the kind I'm looking for. And some of the others will be waste, but at least you can look at it, and it would be a good thing to know!" And so off it started.

Nebeker: Were there other people at Bell Labs interested in the same question?

Patel: Yes and no. In the first couple of years I was working parallel with a number of individuals: Bill Bennett was one of them, Walter Faust, and McFarlane. I can't remember his first name. Let's call him Robert. But they were in a different research group from mine, and they were working more towards looking at properties of helium neon lasers. They were working on one laser as opposed to my working to go towards different varieties of lasers. They were slightly ahead of me in the following sense: all three of these had done their Ph.D. in optical

spectroscopy, whereas I had done my thesis in the microwave area.

Consequently, they knew the field a little bit more than I. I took advantage of their presence in order to learn more about this field as quickly as I could.

Nebeker: And what form did that take? Did you talk with them daily?

Patel: If not daily, just socially every other day. We shared equipment, because even though Bell Labs was very generous in equipment, it wasn't generous enough. We often had to share equipment, which turned out to be exceedingly useful. One of the exciting things about Bell Labs then was, and it probably still is, that it was a single-owner laboratory, and therefore within the laboratory there was no selfishness among people. People were willing to share information with their colleagues as freely as any place where you can find that. And so, if you wanted to learn something, people would sit down and tell you all about it, or if you wanted to borrow any piece of equipment they were not using, they would gladly do it. You were expected to do the same in reverse. So unlike universities, current universities, where that doesn't happen, this was a unique organization. It probably still is a unique organization.

Nebeker: Why would that happen? I mean, universities should be another environment where there's free exchange.

Patel: Well, yes and no. There is very little sharing of equipment, because the equipment is owned by the individual as opposed to the equipment owned by the institution. There is a big difference here in who is the owner. And the second very important part was the institution's basic belief which was propagated into everybody that more often than not one plus one will come out to more than two.

If two people with complementary expertise get together and share their resources they will be able to accomplish more in a shorter period of time.

As a result of all of this, in the first couple of years of exploring different gas systems and the like, I was able to increase the number of gases in which laser transitions were seen, or the number of laser transitions, by an enormous amount. One of the things you found was that if you had the right discharge conditions, almost any atomic system will lase. There was nothing sacred about helium neon. Everything worked, but you had to have the right conditions. By early 1963, I had published papers which showed [that in certain systems there were] a total number of laser transitions in excess of several thousand. You know, going from one to a thousand in a fairly short period of time.

Nebeker: What was the nature of your work? Was it mainly theoretical?

Patel: No, it was mainly experimental, but some amount of a priori analysis was necessary because otherwise you can spend a lot of time selecting gases and understanding what frequencies would lase. If you knew the atomic spectroscopy you knew where it would. To this day I cannot understand why this group of individuals who worked on helium neon laser chose that system. It is one of the more difficult systems to make work. There are dozens of systems out there which are trivially easy. But it's hard to get back into people's minds and find out why something happened.

Nebeker: Very often it seems in science and technology it turns out that it was extreme good fortune that they happened to hit on the easiest system. Here is the opposite!

Patel: And I'll come to the easy part in just a second. But by 1963 it was clear that,

having shown that to be earlier was to be clever enough to identify an atomic system, the right discharge conditions, and how the thing will work. It was clear by that time that I had to do something different from what had happened in the past. In the meantime, I [began working on spectroscopy with lasers], as I had told my department head I wanted to do. Now, we didn't have a tunable laser yet! But one of the things that came out of all of this was a very strong laser system with the one at 3.5 microns in xenon. The system had so much gain that you could screw it up anywhere you wanted and it would continue lasing no matter what you did. You had to be really smart to prevent it from lasing!

One of the things that I did was to apply a magnetic field to the laser. If you do that you can split the laser transition by the Zeeman effect, which you can tune using a magnetic field! This is the amount of splitting, so the laser transition moves back and forth. And so obviously, now you have a tunable laser. It doesn't tune very far, but one of the things you can do is look at the same gas in another tube to see what kind of absorption features you can identify. And, of course, what I showed was that you could, in fact, carry out laser spectroscopy with resolution finer than the Doppler very easily! This was an exciting study, in a sense, because all you were doing was studying the same signature, doing the same frequency by applying the magnetic field you wanted and keeping the load constant. But it showed the principle that there was something of value here, which only if you had a truly tunable system.

Nebeker: Was that the first tunable laser?

Patel: First tunable laser in any matter of speaking. In a certain sense. I mean, it's not a

very useful tunable system because it doesn't go very far.

Nebeker: But it's useful for the study of that one gas.

Patel: Yes, but it's not a generally usable system. At least it gave me the belief that you could do it. We just had to wait for some better system to come along. But it was not just totally off-the-wall to think of this.

In any case, by early 1963, it had also become clear to me that all of the systems we had looked at had optical power outputs in the region of ten to fifty milliwatts. No matter what you used, you couldn't get past that magical number of roughly fifty milliwatts. While you are waiting for something exciting to happen in terms of discovering something yourself or somebody else discovering a way to get a tunable laser, there are other things which you should be able to do. One is if you have a lot of power coming out from the system, there should be yet another phase space of laser interactions that you can study. And so one of the questions I raised was, "Why is it that gas lasers don't produce a lot of power?" By this time, most people who were working on gas lasers had departed. They decided it was not of much value.

Nebeker: Because of the low power?

Patel: Yes. There were solid-state lasers coming along, which in a small volume could provide a lot of power. By a lot I mean a fraction of a watt which was five times, ten times what you could do from a meter-long tube, so it was the general belief that gas lasers were a curio but not a practical device.

So I spent about six months just trying to understand what it was about the laser systems that I had looked at that made it so. And it became very clear to me that

the principal problem was working with atomic systems. You get a little bit of energy out and the rest of it has to be recycled. It's not only a low efficiency device, but also one you must put a lot of energy into while very little will come out. So the questions that I asked were, "Well then, there has to be a different system where all the energy levels are closer to ground state, if you are ever going to get a lot of power out of gas lasers." When you ask questions that way, you immediately move away from atomic systems. You are now going to go to the molecular systems, because they are the only ones that are not electronic energy levels. These are vibrational rotation levels, but they are closer to ground state! Now, when you put a molecular gas into a discharge, it will just fall apart and you won't have a molecule left. And you can talk about inversion between vibrational levels sufficient so that you can get enough gain to get a lasing action, and of the zillions of molecules that exist there [is] only a finite number of atoms which you can find in a gaseous state, but zillions of molecules: which one will you choose?

Nebeker: Were you alone at Bell Labs in doing this?

Patel: Most people who were working on gases had shifted away!

Nebeker: So you were almost alone in the world!

Patel: That's right! Some of them went back to the books! Books on electrical spectra. One of the first things you recognize is that the smallest molecule you can have is two atoms. So you start with that. And, say, you can't have a diatomic molecule with like atoms, because they don't have any transition moment between vibrational states that is prevented by symmetry considerations. Thus you've got to have molecules like carbon monoxide (CO), for example. And for wrong

reasons, you can almost conclude, you can convince yourself, that you can't get inversion between vibrational states of a diatomic molecule. The reason is very simple. It's wrong, but it's simple. It's that the dipolar moment depends upon the vibrational quantum number. What it says is that as you go to higher and higher states the lifetime gets shorter and shorter, which is just the opposite of what you want. If you want complete inversion you want an upper state that has a longer lifetime than the lower state. Now, that's not strictly true. You can get inversion with an upper state lifetime shorter than the lower state lifetime if you're pumping rate is faster in the upper state.

Be that as it may, we decided that a diatomic molecule is not a good thing to work on, and we should go to the next higher, which is a triatomic molecule. For the reason of stability and simplicity, I chose carbon dioxide, CO<sub>2</sub>, a reasonably well-studied molecule from the point of view of lower inversional state. Simple calculations showed that at 10.6 microns the thing should work like a bomb, and if you look at it for ten seconds, it doesn't take that long; if you look at it you can convince yourself this is it! It goes like a bomb! First time you turn it on and off, it goes!

Nebeker: Right away it worked!

Patel: By this time, of course, everyone had abandoned gases. Everybody said gas would never make large amounts of power, so that was one input. The second major input to this case was my realization that diatomic molecules are kind of unique in that they don't have any dipole moment in excited states. So when you look at stable diatomic molecules, like nitrogen, oxygen, which are hard to break

apart in a gas discharge, nitrogen for example, I found out by looking through the literature that the first excited state has the lifetime of the second. Somebody else, other literature pointed out, found that in a gas discharge one could estimate as much as thirty percent of the total population state. Now this is exciting, because now you can mix molecular nitrogen and carbon dioxide and things should work even better! You have got a storage system which is not going to lase by itself, but the energy levels are so close to each other that there should be a very efficient transfer. So the next thing we did was to put some nitrogen into the system, and we had the first laser producing power greater than a watt. Just like that. And of course then it was just a matter of trying to optimize the variables. By, I believe, mid-1964 I had the first gas laser, the first laser of any kind that produced a hundred watts of continuous power output.

At this point, I got a call from the Defense Department to come and tell them what it was all about. I said I would gladly do it but at that time I decided that this was not a field I wanted to pursue anymore. It was now very clear that people could engineer the systems very quickly up to high power levels, and for one person acting on his own, clearly you're out of the competition.

Nebeker: Bell Labs might have given you a larger group.

Patel: Yes, but it didn't make sense. Why compete in a field where the only parameter of success is, "How much more is possible?" not "How much is new"?

Nebeker: What was the Department of Defense's interest in this?

Patel: Weapons. It was very clear. When people heard that a gas laser, or a laser of any kind, was putting out a hundred watts of power, the picture that the press release

put out was of a brick glowing white.

Nebeker: Some kind of a death ray.

Patel: Yes, a death ray, right. So it was very clear at that point that you had to leave this thing to engineers who were interested in scaling as opposed to people who were interested in the basics. So that, in a sense, is the story of the carbon dioxide laser in a nutshell. It came about because I refused to believe that gas lasers were dead! And of course, I did go back subsequently to the diatomic gases and carbon monoxide. Very soon after I stopped, I decided that I would look at new systems. All of them were again in the molecular system. Carbon monoxide turns out to be the most efficient of molecular lasers, lasers with frequency, lasers have many frequencies. But that aside, if you just want to draw laser power, you can make carbon monoxide lasers which have efficiencies better than sixty percent. Sixty percent of the electrical power comes out as light output, which is not something you can say for any other system.

Nebeker: Is that what you turned to after the CO<sub>2</sub>?

Patel: Yes, but again for a fairly short period of time, because all of this was getting me away from what I wanted to do, which was to use the laser as a tool to do something else. And so at this stage I turned to asking the question "What are some of the unique things you can do with high power outputs which had never been done before, which could have scientific as well as technological impact?" And of course the first thing that comes to your mind, or came to my mind, was nonlinear optics. People had just done nonlinear optics experiments using the ruby laser, but this is a pulse system and it doesn't quite have the same control on

the band that was described with the continuous wave system. One of the advantages of working in a new field is that there is an enormous amount of low fruit, and the same thing happened in the field of nonlinear optics. The first material that I picked out to study in terms of learning the properties at 10.6 microns was tellurium. That has the largest molecular coefficient of any material known to mankind.

Nebeker: And you didn't know that at the time?

Patel: No. I knew it should be approximately the largest, because there were some semi-theoretical arguments that you can use to go from the linear properties to the nonlinear properties, assuming that it has nonlinear properties. The scaling parameter was reasonably normal, reasonably is within a factor of ten, and it happened to hit the high side of ten rather than the low side of ten. The fact of the matter is that there was a first nonlinear optics study in the region which sort of opened up an entirely new field.

Nebeker: Why were people interested in nonlinear optics then?

Patel: By then it was very clear that one way by which you could make tunable lasers is to have a fixed-frequency high-power laser and use some kind of nonlinear material to create parametric gain. You however can downshift the frequency at the same time that now the frequency becomes too narrow.

Nebeker: I see. And that was your interest also.

Patel: Yes. The parametric amplifiers were known from the microwave region, which is something I had known from my earlier days. The question was how to extend them into the visible region or the infrared region. If you had nonlinear materials,

you could use the nonlinear properties to do that. Within the year or so, I went through a large number of nonlinear materials to look at their properties to find the most optimal ones. This was in Bell Laboratories, which had a large number of very smart people working on all different kinds of things. One of the things that happened out of all of this was a proposal by Peter Wolf, which said you can do Raman scattering from electrons in a semiconductor in a presence of a magnetic field.

What happened is the following. What he proposed was that if you apply a magnetic field to a semiconductor, the electrons will be quantized in Landau levels. You can excite electrons from the lowest Landau level to the second Landau level and downshift your laser frequency by that much amount. Where is the beauty of this? The beauty of this arises in the fact that the separation between these levels now is the function of the magnetic field. So that's your dial, if you want to call it that. So I said, "Hey, this is the way to make a tunable laser!" You start with 10.6 microns, going to this black crystal or whatever it is, and if you have done things right, out should come a frequency which is downshifted by the separation between the Landau levels.

Nebeker: And that was variable over a considerable range?

Patel: Yes, by changing magnetic fields this thing could be changed by a very large amount. Now you started off with magnetic field applied to gas itself, which is what I had done earlier--this is applying it to something else whose levels can be shifted by large amounts as opposed to small amounts. Just before that happened I had also discovered the nonlinear behavior of electrons in semiconductors.

What people thought about nonlinear optics was that nonlinearity came principally from the crystalline properties of the material, those static properties. You don't need any free electrons; you don't want any free electrons. And it comes from the crystal structure. What I had discovered before Peter Wolf's article came out is that free electrons in a semiconductor also have nonlinear properties, but they come from a very different mechanism. They come from the fact that the energy momentum relationship of electrons in a semiconductor is not simply quadratic. Energy doesn't go strictly as fast as the velocity, which is what you normally think. It goes slightly slower because of the fact that these are not free electrons but are in a semiconductor. That nonlinearity can be enormous. It is a very large nonlinearity, but it's a third order nonlinearity as opposed to the second order nonlinearity which would give you second [unclear word]. That nonlinearity is also responsible for the Raman scattering that Peter Wolf described. And so by this time I had a couple of individuals who were working with me, as colleagues.

Nebeker: These are Ph.D.s?

Patel: Yes, Schussler and Paul Flurry, and they knew a lot more about solid-state physics than I did, which was a big help in making this transition from gas lasers into Raman scattering. One of the things that happened was that when we did the experiment the first time, not only did we see the transition that Peter Wolf had predicted, but we also saw two more. Peter had predicted going from zero Landau level to second Landau level; we also saw first Landau level to second Landau level, and we saw something which corresponds to the flipping of the spin

at the same Landau level. The surprising part was at the last one, which doesn't quite have the tunability of what Peter Wolf's transition level had, but it has an enormous cross section, so you get a lot more of the scattered light. So you got this very large cross-section that can be tunable. This was early 1967 or thereabouts.

Nebeker: And those two were not foreseen?

Patel: No. By this time I decided that I had found my system that was going to let me do my tunable laser spectroscopy; it was very clear that that this was it. I should also point out that by this time, in the visible region, people had discovered dilasers, so people were already doing spectroscopy in the visible region using dilasers, but the infrared region was still clean as far as anybody else was concerned. I spent two years trying to get the spontaneous Raman scattering to become a laser. I had a post-doc working with me, Alan Shaw. We would prepare the experiment, but nothing would happen. It was either a yes or no experiment! There was nothing in between. We could see the spontaneous Raman scattering, but we could never convert it to stimulated laser action. In the meanwhile, I was also looking at nonlinear effects in gases, looking at some things like silicon transparency, and so forth. While you are doing an impossible experiment, you try doing something to keep your spirits up! But in late 1969 we finally succeeded, and that was I think the first tunable laser in the infrared.

Nebeker: And did you then go on to use it?

Patel: Yes, in spectroscopy, and I spent several years doing pollution detection. One of the natural outcomes of spectroscopy is you detect molecules of interest.

Nebeker: In extremely small quantities.

Patel: Yes. By the way, at the same time I also developed an outdoor acoustic. In this you don't measure what goes in and what comes out--the traditional way of doing spectroscopy. Instead you measure what goes in, and you measure how much energy is left behind in the system. When you do that you are measuring on the top of thermofluctuations, which is a very small quantity, so whatever is left behind can be measured to exquisite precision. That's its advantage. And of course the result of this was that I showed it could detect as few as one hundred million molecules per cubic centimeter for one molecule I chose. That is a mixing ratio of about a part in a trillion.

We looked at pollution coming out of cars by the side of the highway, which was again a trivial experiment because you are talking about levels of detection a thousand times the capability ten thousand times. And one of the questions I asked was, "What can you do with such a really very efficient system of detection?" It became very clear to me that one thing you want to do is measure nitric oxide in the atmosphere in the stratosphere. This was in the early seventies, when people were talking about having a fleet of supersonic transports flying at a hundred thousand feet or something like that. There were various theories floating around about what would happen to the ozone level because of nitric oxide. Of course Boeing and their colleagues said nothing happens. Johnston, a theorist at Berkeley, said that my model showed that it will have an enormous impact. All of that was based on nitric oxide chemistry. No one had measured anything in the stratosphere yet, however, so no one knew what the real

concentration was. So I said that what we really had to do was measure it. Here was a system which detects a hundred million molecules per cubic centimeter.

The expected level from all theories tells you that it repeated about ten times that, so I have got a signal in order to show ten to work with. I said that we should do the experiment!

So we took one of these Raman lasers; this is the tunable laser which we talked about that has a CO laser and a magnet crystal Raman scattering and an acoustic cell and associated electronics. We packaged it up and put it in the stratosphere at a hundred thousand feet. We were the first ones to measure the nitric oxide chemistry in the stratosphere. It showed that unless there is sunlight, there is no nitric oxide, because it is all stored as NO<sub>2</sub>. As the sun comes up it [the NO<sub>2</sub>] dissociates, making ozone as well as making nitric oxide which destroys the ozone. But the whole cycle can easily be amended by measuring how quickly the nitric oxide comes up and how quickly it dies in the evening. You can put bounds on the theories that they were making for it, and, of course, it was very clear that a fleet of SSD would have a significant impact.

Nebeker: How important was Bell Labs as a setting for that?

Patel: It was absolutely important. It was very clear that Bell Labs had recognized by this time that what I was doing was not going to be of any value to communications. But it was of enormous value to Bell Laboratories as a place where basic research is important and as a place where the right kind of people would continue to come and work. So it was very important to Bell Laboratories as a place of basically such enterprise. And you know, periodically things like

opto-acoustic measurement techniques which now have been invented or other environments to measure very low concentrations of gases clearly will fall apart periodically when they get used.

Nebeker: I wanted to ask you about your involvement with IEEE. Do you remember when you joined?

Patel: Let's see, I joined as a student member probably in 1959. That was the IRE at that time. [IRE became IEEE in 1963.] Then I became a full member when I got my Ph.D.

Nebeker: And did IRE and then IEEE do the things that you hoped it would?

Patel: Yes. I think in the early years the Proceedings of the IEEE (which I think was still the IRE if I'm not wrong) was still considered a premier place. The Letters to the Editor was just as much a premier place for publishing new and outstanding results as Physical Letters was. And I think by about 1963 or 1964 things changed. I'm not really clear as to what happened at that time; either that section disappeared or it sort of moved away from lasers and laser technology into something else. Of course, by that time, Applied Physics Letters came along so there was an alternate place.

Nebeker: That became the premier journal?

Patel: Yes, that became the premier place for applied research. But I think if you look at some of my earlier publications a fair number of them are in the Proceedings of the IRE or IEEE.

Nebeker: And since then has IEEE functioned well from your point of view?

Patel: I think it has functioned well, but not in my fields of interest. Because my field of

interest over the years has become more and more towards non-applications-oriented science. It may have applications possibly, but not applications per se.

Over the years I have moved away from the mainstream of IEEE activities. I think there are a number of things that could have been published there and should have been published there, but...

And you know, while research has continued to be important, I spend a fair amount of time thinking about larger issues of science: policy and other things.

Nebeker: Well, I am always interested in people's views about how the organization IEEE could function better.

Patel: I think one of the real trends IEEE has is its very large membership. It is also one of the biggest detriments. IEEE should be more in the public policy arena than it is. I think it is much too conservative an organization. They are too much afraid of being wrong on public policy issues. I think that by and large it hasn't served either the membership or the discipline well.

Nebeker: By not being more outspoken?

Patel: Yes. It is very different than the American Physical Society. (As you know, I am the President of the Society this year.) Over the years the APS's public relations and public policy parts have taken on studies of nationally important issues which have shaped the subsequent debate. I think this is not touting my own contribution, but one of the most exciting studies the APS did was on the science and technology of "Star Wars" technologies. I was the co-chair of that study, and I think with some modesty I can say that I think it was the principal reason why SDI was dismantled. We showed conclusively that much of what was

being proposed would not be done, at least not in the time frame that was being proposed. I think IEEE would have been a lot better place to do it, because it focuses on technology and not basic science. But IEEE has no mechanism by which to do such a study.

Nebeker: They did a study in the seventies of nuclear energy that created such a storm that maybe they're scared off from that.

Patel: Maybe, but it's very clear that here is a body which has a large international membership and a large national membership. It's got to become a driver of policy. You just can't sit by the side because you're too afraid. You make mistakes, but that's the nature of things! But maybe engineers are more worried about making mistakes than scientists are; I don't know! I come from both fields!

Nebeker: I appreciate your commenting on it. Thank you very much!