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A Bold but Practical Man of Science

An interview with Dr. C. Kumar N. Patel, conducted by Dr. Milton M.T. Chang

C. Kumar N. Patel is CEO and chairman of the board of Pranalytica, specializing in technologies for the detection of very low level toxic gases. Before founding the company, he was vice chancellor for research at the University of California, Los Angeles, where he is a professor. He received his BE from the College of Engineering in Poona, India, and his MS and PhD in electrical engineering from Stanford University. Starting his career at AT&T Bell Labs, he rose to executive director of research, materials science, engineering and academic affairs.

Patel is best known as the inventor of the high-power carbon-dioxide laser. A member of the National Academy of Science and the National Academy of Engineering, he has received numerous awards, including the Adolph Lomb and Townes medals, Optical Society of America's Frederic Ives Medal and the Medal of Honor from IEEE. In 1996, he received the National Medal of Science from President Bill Clinton.

Milton Chang is managing director of Incubic Venture Capital, a fund in Silicon Valley that invests in photonics and in businesses related to the physical and life sciences. Former CEO and president of Newport and New Focus, he sits on the boards of a number of companies, including Arcturus Bioscience, OpVista, Rockwell Scientific and YesVideo, and recently was elected to the board of trustees of California Institute of Technology. He is a fellow of OSA and of LIA and is past president of the IEEE Laser Electro-Optical Society. He welcomes your comments and can be reached at miltonchang@incubic.com.

Milton Chang: I would like to learn from your experience as an inventor, a scientist, a manager, an administrator and now an entrepreneur. To start off, tell us about your Bell Labs experience.

C. Kumar N. Patel: When I went to Bell Labs, it was the good old Bell Labs, where they hired good people and let them do pretty much what they wanted to do within a reasonably well-defined framework. I told my supervisor I wanted to do high-resolution spectroscopy using lasers. In 1961, there were only the 1.15-μm helium-neon and the ruby lasers; neither is a very tunable laser. It took me almost 10 years to do tunable laser spectroscopy.

How did you come up with CO_2 as the gas?

Everybody around me at Bell Labs was working on solid-state lasers.

> **"Industry [should treat] a technically trained person as an institutional resource that creates value."**

Being a young kid on the block, I said I should try something different. To make a long story short, going to molecules from atoms was a significant step, driven by the recognition that, to build a high-power laser of any kind, it had to have high quantum efficiency. I picked CO_2 as the first molecule to analyze and convinced myself it is an intrinsically very high efficiency system. It worked the first time around. I put my hand in the beam to see how much power I had; it was a very painful mistake, and that got my attention.

It was the freedom that made Bell Labs excellent?

Bell Labs was clearly a powerhouse; it drove both science and technology in many fields in this country. Most people in research at Bell Labs — if not everybody — knew the company's business was communications. That notion provided a reasonable framework even though the research, per se, was not directed toward applications.

CO_2 lasers could hardly be lumped into communications.

CO_2 lasers never became an important part of communications, but the recognition that high-power lasers can be made was an important milestone, saying that we don't have to rely upon micro- or milliwatt lasers.

That brings us to the classic debate on whether good science can come from applications-directed research.

Keeping the use in mind shapes the kind of research you do. It doesn't make the science any better or worse; it just makes it different. Serendipity is still important even when you have applications in mind. Serendipity requires that you have enough knowledge to recognize what's important when it happens.

I am really probing: What brought about the demise of Bell Labs?

I think the way science was done did not change. The cause was a multitude of events. One is legal, and the second is, bluntly speaking, management at AT&T. It never adapted itself to an unregulated environment. Funding became tighter, and eventually the freewheeling way of thinking about problems got replaced by

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more directed research activities in solving very specific problems.

So how would you manage research?
The right way of doing research, in my mind, is first to have a large goal in mind. Second is to have sufficient knowledge to parse that large goal into small enough pieces to understand where the bottlenecks are. Step No. 3 is to provide some kind of rank ordering of things that need to be done, and the last and most important thing is: Go do it.

C. Kumar N. Patel

Looking forward, where are the technical opportunities?
Even though people go around saying that the 21st century is the century of biology, let's not start with the assumption that all of the new things are going to happen only in biology. Pick a good problem. Work on it as hard as you can. It's good to find out early in the game what's impossible within the framework of where the current science and the current technology stand. And don't keep bashing your head against this solid wall if it doesn't turn out to be getting you anywhere.

How about mentioning a few specific technical areas?
Solid-state physics continues to be important, material sciences, radiation interacting with materials, and materials used in new ways. As we know, every time a new material is invented, discovered or synthesized, completely new areas of both science and technology become possible.

Aren't you implying "nano"?
I think nano is good. Understanding materials at the molecular and submolecular level was there before the word "nano" got invented. One of the real tragedies of American science and the way it is done has something to do with the funding mechanisms that very often require a practical application or a catchy name. This is what I consider a shortfall of current thinking about nanotechnology. It should be called "nanoscience" rather than "nanotechnology." There's lots of very good science that needs to be done, but its commercial impact may take 10 to15 years.

That gets back to the earlier discussion about having a framework but not being closely directed.
When politicians ask the question, "What have you done for me lately?" they're looking for a gizmo. The National Science Foundation is a very good organization, and it is doing what it's supposed to do, which is to support science in a broad range of activities. But we don't have a bolder way of thinking that says, "Here are a half a dozen or a dozen things that we think are important," define a broad area of science and technology that is important and then take one step back and ask what set of scientific things I need to do to get to that stage. That way you not only have the piece parts, but also have the overall framework for synthesis.

Any entrepreneurial insights to share with our readers?
One of the things I have learned in my foray into entrepreneurship is that science and technology are important, but markets are more important. Eventually, somebody has to pay for what you make. The other thing I've learned from calling on customers is that you'll be making the wrong pitch if you don't understand your customer's hot buttons; he/she wants to look good in the boss's eyes. That's where the next raise is.

What are your hot buttons? On anything.
In the USA, much of the training we provide to graduate students is much too narrow. The student must learn to think as a scientist as opposed to just being a person working on the professor's contract or grant. The other problem is that industry employs PhDs to solve very specific problems instead of treating a technically trained person as an institutional resource that creates value. My last hot button is the fact that young scientists today do not connect well with the politics of science. I'm concerned that, 10 years from now, politicians will be making scientific decisions without the help of scientists who have chosen to straddle the line of science and policy.

What's that one message you teach your children?
Whatever you do, do to the best of your ability. This basically comes out of Bhagwat Gita, the Indian book of scriptures, which can be paraphrased as, "Don't worry about reward. Do the right thing."