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Some Reflections on Innovation and Invention

George H. Heilmeier

One of the reasons technology transfer is problematic in the United States is that "innovation" is misunderstood among members of our technical community

George H. Heilmeier is president and chief executive officer of Bellcore, Inc.
Dr. Heilmeier presented this Founders Award Lecture at the NAE Annual Meeting, 29 September 1992, in Washington, D.C.

I am grateful to the National Academy of Engineering for honoring me with the Founders Award. I confess to being awed by this award. The past recipients are among my heroes, and one has strong feelings of inadequacy in such company.

I'd like to share with you some personal experiences in the saga of liquid crystal display technology, along with some lessons they taught me about invention and innovation.

Looking at innovation on the personal level, it seems to me that most successful innovators have something in common with a successful hockey player. Wayne Gretzky once said that he doesn't skate to where the puck is. He skates to where it's *going to be.*

Innovation depends in part on anticipating where technology and its applications are going in the future and daring to trust that intuition. It's rooted in knowledge, skill, practice, experience, and the courage to act. It is often nurtured by the support and guidance of mentors whose intuition and motivation resonate with yours.

For my own experience in exploring liquid crystal display technology, this passage from the Book of Ecclesiastes is a fitting epigraph:

> I returned and saw under the sun that the race is not to the swift, nor the battle to the strong, neither yet bread to the wise, nor yet riches to men of understanding, nor yet favor to men of skill, but time and chance happens to them all.

In my work with liquid crystals, which I began some 30 years ago, I was certainly favorably blessed by time and chance. Time and chance have also played a role in the subsequent development efforts. I have seen many of my dreams become reality in the hands of the world-class Japanese engineers who are driving liquid crystal display technology today.

In many respects, the story of liquid crystal display technology has all the ingredients of a good novel: excitement, frustration, success, failure, and personal tragedy. I will not bore you with the whole story, but rather will concentrate on how the experience has shaped my thoughts on innovation, invention, technology transfer, and industrial policy. You see, time and chance are not the only players in this drama.

Do Something Different

Every story has a beginning, and this one began at Princeton University in 1961, with my search for a doctoral dissertation topic. I had worked for two years in the then-emerging field of solid-state microwave devices, and it seemed logical that I would do my dissertation in that area. But the field was getting more crowded, competition for new ideas was getting tougher, and I was looking for something more exciting. Through a friend, I had been keeping track of some interesting work on organic semiconductors, and I discussed this with Leon Nergaard, director of the Microwave Laboratory at RCA Laboratories.

His advice changed my career. He said, "Look, George, you may never have another opportunity to try something completely new like this again. . . . Do something different." Professor George Warfield agreed that organic semiconductivity would offer the greatest learning experience for me. And so it was that I left behind the relatively safe world (safe, that is, for an electrical engineer) of inorganic materials.

A few eventful years later, I found myself in the office of Vladimir Zworykin—whom many consider the father of television and who, incidentally, would receive the NAE Founders Award in 1968—explaining how I had stumbled upon the first of several new phenomena; what is known as the guest-host color switching effect in liquid crystals.

Note that around the time we first demonstrated this effect, I had discussed it in a general, hypothetical way with a visiting Nobel laureate, Peter Debye. I was very guarded because of the potential commercial implications. He told me, in essence, that it couldn't be done. It would never work. I didn't tell him that I had already done it.

Innovation depends in part on anticipating where technology and its applications are going in the future and daring to trust that intuition.

Now Dr. Zworykin had summoned me to his office to find out why everyone at RCA Laboratories was so excited about my results. To my comment that I had stumbled upon this breakthrough, he replied: "Stumbled, perhaps . . . but to stumble one must be moving." I will never forget those words. And that was the beginning of an exciting period, from 1964 to mid-1968, during which our liquid crystal research kept moving, and moving fast.

The step that generated the initial excitement had been to dope a nematic liquid crystal with a "guest," pleochroic dye, sandwich the mixture between two glass slides coated with transparent tin-oxide electrodes, place this cell on a hot stage—since at that time there was no material known to have a nematic liquid phase at room temperature—and apply a DC voltage. We watched the cell change from red to colorless as the applied field aligned the dye molecules.

After this discovery of the guest-host effect, we tackled a number of problems on all fronts at once, and we eventually solved most of them. Along the way, we discovered how to control the reflection of light electronically in certain classes of nematic liquid crystals—an effect we called dynamic scattering. We also discovered an electro-optic storage effect when we mixed two different classes of liquid crystals. The transparent material turned milky white in the presence of a DC field, remained in this state after the removal of the field, and became transparent again in the presence of an AC field. Once the first room-temperature nematic materials were prepared, we were able to fabricate a number of crude prototype devices based on dynamic scattering: alphanumeric displays, windows with electronically controlled transparency, static pictorial displays, an all-digital clock with liquid crystal readout, and an e-beam-addressed liquid crystal television (integrated circuit capability did not permit matrix addressing at that time).

He told me that it couldn't be done. I didn't tell him that I had already done it.

A press conference in June 1968 presented our prototype devices to the world and attracted worldwide attention to the potential of liquid crystal displays. This was the end of the beginning.

Opportunities Denied, Lessons Learned

But liquid crystal displays were not to be a commercial success for RCA, for a number of reasons. They were not "silicon." They were "dirty" by semiconductor standards. They were liquids. They were too easily duplicated. They were said to be "too difficult to make." These were among the reasons the product divisions gave for the failure of their commercialization efforts. Liquid crystals were viewed as a threat rather than an opportunity by the product divisions.

Looking back on what is to be learned from the liquid crystal saga, I can think of several lessons.

- Never be afraid to explore something entirely new. Treat intuition as real.
- Don't be deterred by judgments based on incomplete information that "it can't be done."
- Do the difficult experiments first. Don't substitute research for insight. Review older concepts periodically in light of progress made in other areas that might change earlier views.
- Approach problems from an interdisciplinary point of view. Remove the barriers to exploiting the viewpoints of other disciplines, and do not be afraid to be called naive when venturing outside your own professional discipline.
- Have a clear view of what you are trying to do, but be prepared to modify this view in light of new information.
- Clearly understand the limits of current approaches. Understand what is new in your approach and why you think it might succeed.
- Understand the implications of success. Build prototypes so that others can begin to share your vision.

In retrospect, I can't help wondering what would have happened if our team, suitably augmented, had been given the responsibility for developing the business opportunity as well as the technology. We were the innovators, the ones who saw opportunities, not problems—the ones who had no vested interest in the status quo.

History seems to indicate that breakthroughs are usually the result of a small group of capable people fending off a larger group of equally capable people with a stake in the status quo. If one subscribes to this theory, it is not surprising that the Polaroid process was not pioneered by the largest photographic company in the world, that most U.S. vacuum tube companies did not succeed in the transistor business, that office copiers were not pioneered by the giants in the office equipment business, and that jet engines were not pioneered by the piston engine makers.

Would a "national industrial policy" have changed this situation? Industrial policy is a popular topic these days. Politicians talk about it and are lobbied about it. I find it difficult to get a precise definition of what constitutes an industrial policy. For some it seems to include whatever we don't have now. Does industrial policy mean that the government should be the venture capitalist of last resort and pick the emerging businesses of the future?

Where in the world has this aspect of industrial policy really worked? In France? In Japan? Wasn't it their Ministry of International Trade and Industry (MITI) that advocated mainframe computers instead of PCs and workstations, and steel instead of automobiles?

Japanese corporate executives moved into microelectronics and automobiles on their own, with MITI support coming after the fact. Contrary to popular belief, I don't believe that the part of Japan's industrial policy that tries to pick the industries of the future is responsible for Japan's success. Strictly followed, this aspect of Japan's industrial policy would have led Japanese industry down the wrong path, because bureaucratic consensus—anywhere in the world—usually occurs only when looking through a rear-view mirror.

In my view, while there may be merit in the concept of a national industrial policy, it would not have changed what happened in liquid crystal displays. Would government bureaucrats have responded any differently than RCA bureaucrats? I doubt it, but it may have taken them longer to decide. This raises questions in my mind about the general applicability to the innovation process of an industrial policy that plays the role of a market surrogate.

I have to wonder what would have happened to liquid crystal display technology under different circumstances. Suppose it had been fostered by a "domestic DARPA"—another popular "solution" to the competitiveness problem. Again, I believe it would have led to a similar result. The DARPA (Defense Advanced Research Projects Agency) I know and love has performed best when it applied advanced technology to the real problems of a receptive, deep-pocketed customer willing to fully fund the process from conception to deployment. I don't see a "domestic DARPA" in this light at all, because the key ingredients are missing. A "domestic DARPA" would find itself with too many "and" gates in the decision process.

As technologists and engineers, perhaps our energy could be better directed to something each of us could do about promulgating an effective technology transfer policy throughout U.S. industry. In my view, one of the reasons technology transfer is problematic in the United States is that "innovation" is misunderstood among members of our technical community. Innovation is an idea or invention that becomes a *business success.* Liquid crystal displays were an invention and a technological achievement in the 1960s. They became an innovation in the 1970s, and that continues today. Innovation, by definition, must include business success. Unfortunately, it has come to mean "invent something" or "change something." In the process, all too often the context of a business success has been lost.

The "No Excuse" Technology Transfer Policy

The lessons I learned from the liquid crystal display saga are the basis of a policy that I would like to advocate. I call it the *"No Excuse" Technology Transfer Policy.* Implicit in this policy is an understanding that successful "productization" of new technology is very difficult—a contact sport among consenting adults. At the corporate or organizational level, such a policy could short-circuit the self-fulfilling prophecies of those protecting the status quo. It could also break the classic stalemate between responsibility and authority by empowering those most likely to achieve a business success.

Bureaucratic consensus —anywhere in the world— usually occurs only when looking through a rear-view mirror.

The "No Excuse" Policy represents a no-nonsense commitment to technology transfer. There are seven basic tenets:

- Formulate a "catechism":
  - What are you trying to do?
  - How is it done today? What are the limitations of the current practice?
  - What is new in your approach and why do you think it can succeed?
  - Assuming you are successful, what difference does it make?
  - How long will it take? How much will it cost? What are the midterm and final exams?
- Recognize productization as a necessary, crucial activity. Allocate capital and personnel to productization early. Technology, however good, is not enough.
- Identify receivers of the technology and ownership of the transfer early. Provide incentives on both sides.
- To the maximum extent possible, use common equipment in the development laboratory and in early manufacturing.

• Begin the transfer process immediately after demonstrating feasibility in the laboratory. Stay close to marketing.

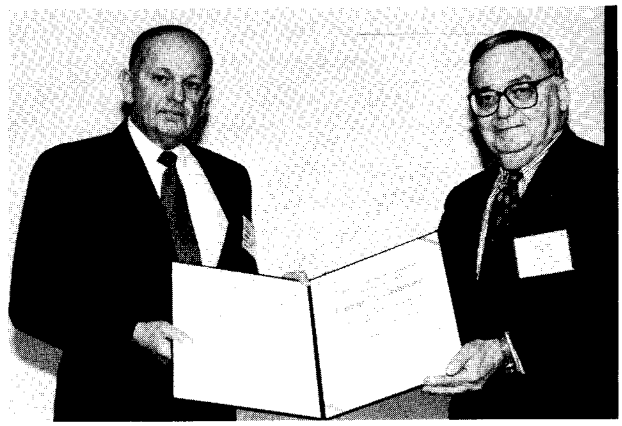
• Manufacturing must prove the methods developed in the laboratory before initiating efforts to improve them.

• Keep the laboratory involved in the productization and manufacturing phases through completion of product qualification and achievement of cost and performance goals.

I've outlined seven tenets of the "No Excuse" Technology Transfer Policy, but there is more: Don't think that you can make it work by doing four of the seven or five of the seven. You must do *all* seven. This is why failures in technology transfer occur. The road to failure is jammed with people who think they can get away with not doing the tough stuff.

I would like to see a *de facto* "No Excuse" Technology Transfer Policy propagate throughout U.S. industry. In many respects, I believe, it might have a greater effect than that which proponents of a national industrial policy hope to achieve.

I wonder what the next step will be. Can U.S. industry learn to "skate to where the puck is going to be"? I think we'd better try. One thing is certain: We'll never find out unless we, as leaders in the profession, try something different—perhaps something completely new, like a "No Excuse" Technology Transfer Policy. As Wayne Gretzky says, "Statistically, 100 percent of the shots you don't take . . . don't go in."



George H. Heilmeier receives the NAE Founders Award certificate from NAE President Robert M. White.