Jack H. Winters

Reflections on Industrial Research

I joined Bell Labs in 1981, fresh out of graduate school, because I had been told that I would be free to work on whatever I wished. As part of the Bell System, I was told that the company, as well as my position there, was virtually permanent. Indeed, after a couple of months on the job (during which I wrote a paper on my Ph.D. dissertation and master's thesis), I went to my department head and asked him on what I should now be working. "I don't know," was his response. "Go ask [another department member] down the hall." My, how times have changed!

RESEARCH DRIVERS

The definition of research that I most resonate with is that it is work that changes the way people think. A more scientific view is the statement that one is doing good research if about 20% of what one works on actually succeeds and may eventually become a product. If more that 20% of one's research succeeds, then the work is too much like development and one is not taking enough risk. On the other hand, if much less than 20% of what one works on succeeds, then perhaps one should think about a different field (before the money runs out)!

MOTIVATION

I have found the first definition to be a great motivator in my own research. Particularly at Bell Labs, where I was often awed by the insight of the researchers, the possibility to prove an accomplished researcher incorrect was a great driver, as it was certain to change the way people think and do things. As an example, when I first joined Bell Labs, fueled by my dissertation on the use of adaptive arrays in military systems, I looked at ways that adaptive arrays could be used in commercial systems. The real motivation, though, came when I read a well-known mobile radio book that stated that adaptive arrays were of no use in mobile radio because the multipath resulted in too many reflected rays for the array to handle. That is, an antenna array could not put enough main beams in the direction of the desired signal rays, and nulls in all the directions of the reflected interfering signals, to be effective. Although this at first seemed intuitively obvious looking at the array in terms of its antenna pattern, from a mathematical viewpoint this is not true; so, I was motivated to show that adaptive arrays were in fact effective in multipath. Of course, further research showed that not only could adaptive arrays work in a multipath environment but the multipath could also be useful, as in the current explosive growth in multiple-input, multiple-output (MIMO) systems with spatial multiplexing.

I was particularly motivated by statements made by directors at Bell Labs. Even though these statements might have been made without much deep thought, again the motivation to show one's management incorrect (and therefore change the way they thought) was great. (Indeed, others at Bell Labs also used this as a motivator, with a result that one director said that he would not make any such statements.) One example was a director involved in optical fiber research who stated that a dispersion in fibers, polarization mode dispersion (PMD), could not be mitigated because it changed with time. Of course, many others who were working on adaptive equalizers for wired and wireless communication systems knew otherwise. But they generally did not have knowledge of fiber optic impairments. Likewise, the researchers in fiber optics did not have a background in adaptive equalization. This left an opening for me. I just tried to make sure that only optics researchers saw my research, since although they were impressed, anyone who was building a several hundred tap equalizer for DSL would quickly dismiss my work on a three-tap equalizer for optical fibers.

Another motivating statement was that high-temperature superconductors were

I am very pleased to have Jack Winters, chief scientist at Motia, Inc., provide us with his insight and wisdom on research. Jack has a unique and very distinguished background that includes working at Bell Labs back when there was a "work on anything you want" atmosphere (we all know what has happened since then). Jack's career has spanned over 25 years and has included groundbreaking research experience in adaptive filtering, wireless communications, and smart antennas. He defines good research as work that changes the way people think, and he gives specific examples and ideas. He points out that whether in a large industrial research organization, academia, or in a startup, good research involves taking risks to apply one's expertise to a variety of areas where the results are uncertain, as well as expanding one's area of expertise. The best research generally requires the interaction of researchers with different expertise, interaction with the leadership to determine promising problems, and the effective coordination of these researchers.

—Arye Nehorai
"Leadership Reflections" Editor

1053-5888/05/$20.00©2005IEEE

only useful for short distances. This spurred work on superconducting waveguides and coaxial cables, but more on that later.

Another motivator for research is to try to predict where technology is headed and attempt to derive techniques that might be useful 10–20 years in the future. A prime example of this is Moore's law, which has been very good at predicting the increase in integrated circuit complexity and decrease in cost over the last four decades. The topic of MIMO fits in very well with this idea because, although the algorithms and performance for MIMO were first developed over 20 years ago, it was quite clear then that the signal processing capability at the time meant that the techniques were not practical except for expensive military systems. With Moore's law, though, the complexity would be practical for commercial systems in ten years or so, and that spurred the research.

PREDICTIONS

Unfortunately, predicting where technology is heading is a tricky business. For instance, when we began research on electronic dispersion compensation, fiber optic systems operated at 2.5 Gb/s. It seemed easy to predict that the next step would be 10 Gb/s systems and then 40 Gb/s systems (these were even in development at the time), which meant that the dispersion (which increases with data rate) would be a severe problem in the near future. However, wavelength division multiplexing was developed, which used multiple 2.5-Gb/s streams to achieve higher data rates. Dispersion compensation took a backseat for another decade before 10-Gb/s systems were considered.

Worse yet was making predictions on high-temperature superconductors. In the mid-1980s, superconductors were discovered that operated at liquid nitrogen, rather than liquid helium, temperatures. It seemed that every month brought higher temperature superconductors. It therefore seemed reasonable to assume that room temperature superconductors were on the horizon, so we began research on what this could mean. Well, such room temperature superconductors were never developed, and so the research on superconducting waveguides and coaxial cables fell outside the 20% category mentioned earlier. Fortunately, it is easy to hide such research, as such papers are never referenced and therefore quickly forgotten, whereas pioneering research is often referenced, making it appear that the researcher wrote only ground-breaking papers.

A further anecdote on prediction is what happened at Bell Labs in the area of wireless. In the mid-1980s, a highly regarded marketing study was conducted concerning the predicted growth of wireless. At that time, few people (mainly only executives) had cellular phones; the portable ones were the size and weight of large bricks, and the ones in cars cost more than US$1,000. As one might imagine, when the average person was asked if they were considering buying a cell phone, most said no, and the marketing study predicted an annual growth of only 2–3% in the cellular market. Thus, research on next-generation cellular systems, including MIMO, was halted, as an order-of-magnitude increase in capacity was clearly not required for decades. However, the analysts missed one point. Many rental car agencies put cell phones in their higher-end cars. These cars were sold after they were a couple of years old, and people were then purchasing cars that already had cell phones in them. Thus, people that would not have bought a cell phone for their car as an add-on now had them (and these were even people who bought used cars). This increased the number of cell phones in the marketplace to a critical mass and pushed buyers of new cars to install them as well. Thus, mobile radio grew much faster than expected. This just demonstrates how unpredictable demand can be.

Of course, a researcher who follows the predictions often finds that many others will be doing similar types of research. If one believes that he/she is one of the best researchers, then this approach may be promising. However, to truly be a research leader, one usually needs to look at areas and take approaches that are not the mainstream. As mentioned earlier, this may mean that the research will often not be fruitful, but when success is achieved, it can be truly innovative. One approach to this method is to take results from one area and apply it to another area where the same set of techniques can be used with the constraints only altered slightly. Specifically, a researcher may first determine where his/her expertise lies and then look at problems outside this area that can be reasonably well understood in a short time (because of some similarity to the first area) by the researcher (through a tutorial, e.g.), who would then be unique in having expertise in the first area and an understanding in the second area. As an example, my dissertation was on adaptive arrays for military systems, where cost and complexity were not of great concern but the interferer tried to do the greatest harm. This expertise was then applied to commercial mobile radio systems (which were just being introduced). Here, cost and complexity were the constraints (although as stated above, it could be assumed that these would be relaxed in the future), but the interferer could actually try to do the least harm. Using the same adaptive array techniques, but looking at the problem from a different perspective, provided a rich research area for smart antennas for wireless.

Another area was the use of adaptive signal processing in fiber optic systems. Although the impairments of fibers appear to be completely different from those of wireless systems, one impairment, PMD, is similar to frequency-selective fading in wireless systems. Indeed, the time delay of PMD has a Maxwellian distribution, which is the square root of the sum of three Gaussian random variables. PMD has a uniform distribution in amplitude, whereas multipath fading in wireless systems has a Rayleigh distribution, which is the square root of the sum of two Gaussian random variables, and a uniform phase. Realizing the similarities of PMD and multipath fading opened the door to consideration of a wide range of adaptive signal processing techniques that had not been previously considered by fiber optic researchers.

RESEARCH LEADERSHIP

Researchers are by their very nature generally independent. Therefore, leading researchers and getting them to collaborate can be challenging. As I have often heard, management can't simply dictate that researchers make a certain number of fundamental breakthroughs per year or expect good results if they force particular researchers with disparate backgrounds to work together. More indirect methods are needed. For example, one effective method is to bring a number of researchers together for a seminar and then see if some of the researchers discern a piece of the overall problem that they can handle. Often, they might feel that their part of the problem is easy, whereas the other parts are impossible. However, we've seen cases where all the parts were covered by different people, each feeling this way; the collaboration of these people solved a problem that no one else believed was solvable.

Another strategy is to formulate a problem in different ways for different people. For example, one can cast a problem as either a theoretical, systems, or implementation problem to different people based on their skills. Just going through this process can often lead to new insights. It also makes each researcher more comfortable with the problem, even though they may each feel that they aren't solving the overall problem. That is, the theoretician may feel that they can solve the problem mathematically, but it is impossible to implement, and the implementer may feel that he/she can program the solution but not understand its significance. The breakthrough is then made by putting the pieces together.

A further strategy is to try to transform each researcher into a champion for the idea. Generally, if a solution to a problem is presented in detail by one researcher to a fellow researcher, the latter may not be motivated to do the research because it is the other person's idea and the originator usually gets most of the credit. However, if the problem, rather than the solution, is presented in enough detail that the researcher can see the start of a solution, then the researcher 1) will usually have a much deeper insight into the problem, 2) will feel that the solution is more his/her own, and 3) is more likely to become a champion for the idea. [Of course, the degree to which the original presenter knows the details of the solution (rather than just the fact that a researcher may be able to derive a solution based on his/her background) varies widely, often leaving in doubt later on exactly who invented what.]

CONCLUSIONS

Good research generally requires the ability to quickly determine the key bottlenecks to finding a solution and applying a variety of techniques to overcome these bottlenecks. This can mean applying well-known techniques to a new problem, applying a new technique to an old problem, or even applying a new technique to a new problem (which has the most risk, but the greatest potential for changing the way people think). This can result in a large number of potential research topics. Tactfully getting researchers with different expertise together to cooperatively attack the most promising of these topics will help spur technological breakthroughs. I have found this to generally hold true both in large research organizations and in startups.

AUTHOR

Jack H. Winters is chief scientist at Motia, Inc., where he is involved with smart antennas for wireless systems. He received his Ph.D. in electrical engineering from The Ohio State University in 1981. He was then with AT&T for more than 20 years, and he was division manager of the Wireless Systems Research Division of AT&T Labs-Research. He is a Fellow of the IEEE, a Distinguished Lecturer for the IEEE Communications and the IEEE Vehicular Technology Societies, area editor for transmission systems for *IEEE Transactions on Communications*, and a New Jersey Inventor of the Year for 2001. [SP]