A Random Walk through 50 Years of Optics and Spectroscopy

W. M. Doyle

The transcript of a talk delivered on the occasion of being granted the
Williams Wright Award for contributions to industrial vibrational spectroscopy
at the Pittsburgh Conference on Analytical Chemistry
and Applied Spectroscopy, Chicago,
March 5, 2014

A Random Walk through 50 Years of Optics and Spectroscopy

W. Michael Doyle

Abstract

During a career spanning over fifty years and ranging from the halcyon days of atomic and nuclear physics, through the early days of laser development, the spooky days of cold war infrared science, and on to the rapid growth of FTIR spectroscopy and eventually spectroscopic process analysis, I've sometimes found myself on the periphery of interesting and significant developments and in the shadow of great ones who came before. My talk will view this era through a lens distorted by the random events that have determined the course of my own journey through science, technology, and business.

Preface:

Below is an adaptation of a talk that I gave at the Pittsburgh Conference on Analytical Chemistry and Applied Spectroscopy (Pittcon) held in Chicago from March 3 through March 6, 2014. The occasion was my being granted the Williams-Wright Award for contributions to the field of industrial vibrational spectroscopy.

I was given the opportunity of organizing the award session so I chose four individuals who themselves are pioneers in the field and asked them to speak about the history of vibrational spectroscopy from their unique perspectives. All of the talks were well received. And each of the authors had something positive, or at least interesting, to say about me. Warren Vidrine, for example, noted that a number of my past inventions are in use today by various companies, while Richard Kramer included a photo of me hiking with him in the Sierras and closed his talk with a photo of me playing pool.

The speakers in the award session, in presenting order, were:

Prof. Peter Griffiths, retired Chairman of the Chemistry Department at the University of Idaho: A remarkably high percentage of the scientists currently working in the field of FTIR spectroscopy got their PhDs under Prof. Griffiths. He was also involved in the early days of FTIR at Block Engineering and Digilab and is the author of the two most comprehensive text books on the subject.

Dr. D. Warren Vidrine, independent consultant: Warren is an acknowledged leading expert in industrial FTIR spectroscopy. He was the key applications scientist and product manager during the early days of FTIR at Nicolet Instruments. He later took over my duties as chief scientist at Analect Instruments.

Gerry Auth, President, Midac Corp. Gerry was involved in the early development of FTIR spectroscopy at Block Engineering and Digilab and knew all of the very strange cast of characters that were involved. He subsequently was a founder of Eocom, Corp and developed the Eocom high resolution FTIR spectrometer. This product line was eventually purchased by Nicolet Corp. and quickly became the industry's largest selling FTIR spectrometer. Gerry later won a follow-on contract to the Aberdeen airborne FTIR program mentioned in my talk. This led to the development of the current line of Midac spectrometers.

Richard Kramer, President Applied Chemometrics. Richard is one of the leading experts in Chemometrics and related software techniques used in modern spectroscopy and is a key member of the national committees developing standards for the use of chemometric software.

The Chairman of the awards session was John Coates, who had received William Wright Award the previous year.

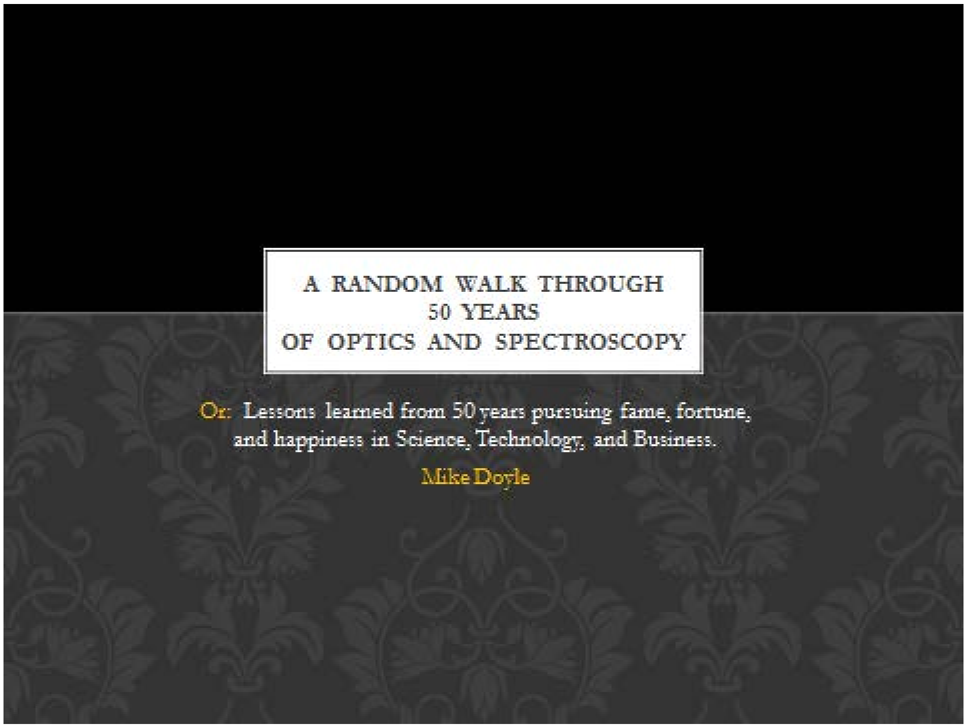
By the way, for those of you not familiar with the field, vibrational spectroscopy includes four distinct spectroscopic fields: mid-infrared, near-infrared, Raman, and to a lesser extent, UV - visible. Each of these, in its own distinct way, provides information about the nature and interactions of organic chemicals by using broad spectrum optical radiation to study the vibrations corresponding to individual chemical bonds.

The award session was attended by approximately 75 individuals, most of whom are themselves experts in vibrational spectroscopy. During the introductory comments, before I started my talk, I noted that four of us involved in this session – three of the speakers plus John Coates – had been together for a very memorable dinner twenty eight years earlier at an FTIR conference in Ottawa, Canada. In fact, Peter Griffiths and Warren Vidrine competed in a curious contest at the dinner known as the "Hirshfeld measure".

For this adaptation, I have inserted my graphic PowerPoint slides as illustrations within the body of the text. Most the word slides have simply been replaced by additions to the text, as appropriate.

TheTalk:

Chicago, March 5, 2014



Well, needless to say, I'm responsible for the theme of this session, "The history of vibrational spectroscopy". And, of course, I chose the speakers. But as I started to organize my own talk, I realized that the other speakers know more about the history of the field than I do. After all, I'm a newcomer, only getting involved in FTIR in particular in the late 1970's. So I decided to take a different slant, making my talk more personal and less specific to the main topic. Hence the title of my talk.

But the title could also have been: "Lessons learned from 50 years pursuing fame, fortune, and happiness in Science, Technology, and Business".

"Lessons learned" is one of the recurring themes of my talk. Other themes include:

"Change" both as the backdrop of all of our lives but also as the source of our greatest opportunities.

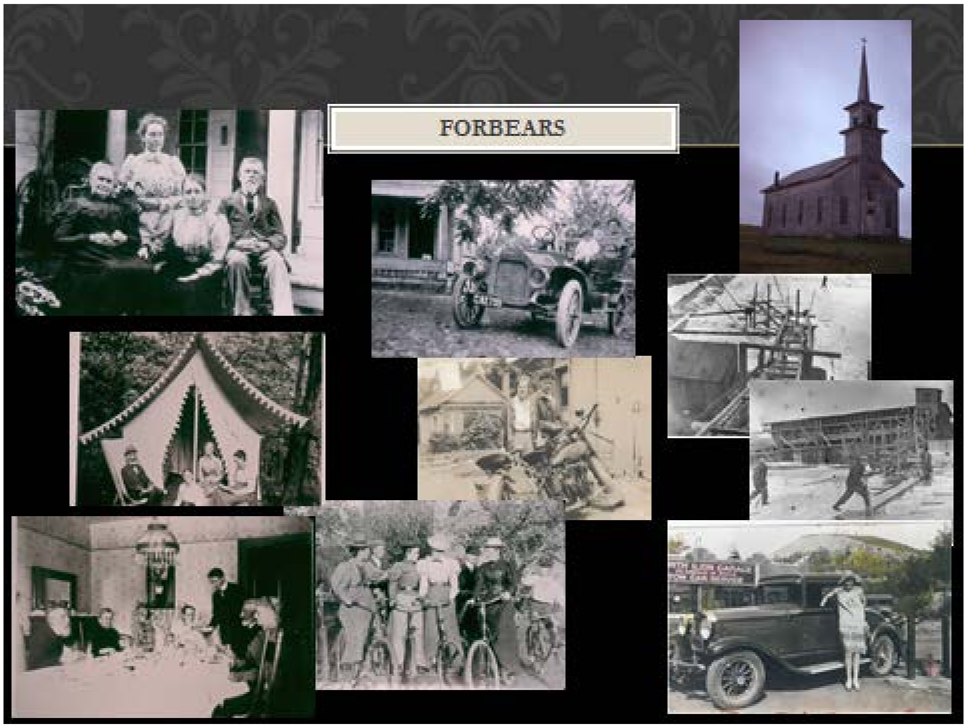
"Randomness." The fact that we're all together in this room today is the result of an infinite number of random events. In my case, a few are especially important.

Finally, there is the theme suggested by a line in my abstract: "in the shadow of the great ones who came before". Here, I'll touch on some of the famous people who have influenced the course of my life, using them as mileposts as I wander through my narrative, and viewing some of them from my unique, perhaps distorted, perspective.

I'll start by taking a look at some of technological changes we've seen. In fact, let's go back to my parents' era. After all, the whole 20'th century was a time of astounding change, starting with developments such as the first automobiles, airplanes, and radios, and accelerating exponentially during WW2. For example, my mother grew up driving a horse and buggy to school – at least in the summer. (In the winter, they'd take the wheels off and replace them with runners, converting it into a cutter.) She escaped the farm to go to college and became an English teacher - only to end up fixing radios and other electronic devices during the war.

My mother wasn't the first one in her family to gravitate toward business. In fact, when she took a job teaching, it was only the second time anyone in her family, going back several generations, had worked for a wage. They all farmed and ran small business. My grandmother was the other exception to the rule. She left the farm at age 21 and took a job in Chicago. But she soon found that she didn't fit in and so left her job to join her sister in starting a business manufacturing dolls. Her Columbian Dolls are now highly prized by doll collectors. In fact, my sister recently completed a book on the subject, which has won rave reviews within he doll collecting community.

We have lots of photos of the Adams branch of our family since my grandmother was an avid photographer even developing her own photographs.



My grandmother married late, eventually having four children. Her husband, my grandfather was also an entrepreneur, running the largest cider mill in Oswego County.

My father, on the other hand, got his start in electronics in the 1920's building his own radios. But then he ran afoul of the great depression, losing his garage and Harley Davidson dealership and having to scrape by fixing whatever needed repair - radios, refrigerators, air conditioners, and even pinball machines.

His father had been the General Manager of one of the largest paper mills in North America at the end of the 19'Th century. (His title was actually Boss Paper Maker.) But then he contracted TB about the time my father was born and died when my father was 11.

My father's mother subsequently married her second husband (or perhaps it was her third). No one seems to be sure. But at any rate, the only grandfather I knew on my father's side was Charlie Wheelock who owned the ice harvesting business shown on this slide.

As you can see, I didn't grow up knowing what it was like to work for someone else. So later, when I took jobs first at GE and then later at Hughes Aircraft and Philco-Ford, I more or less behaved as if I was working for myself.

I was born before WW2 – the ultimate wellspring for much of the technological change that has occurred since. And, I was exposed to technology at a fairly young age.

The War saw the early development of many of the technologies that are still with us today, including the first spectrometers, and even more important, the first successful semiconductors.

Before the War, there were no commercial infrared spectrometers. In fact, the first recognition of relationship between functional groups and IR absorption bands, by Frank Rose, didn't occur until 1937, the year I was born.

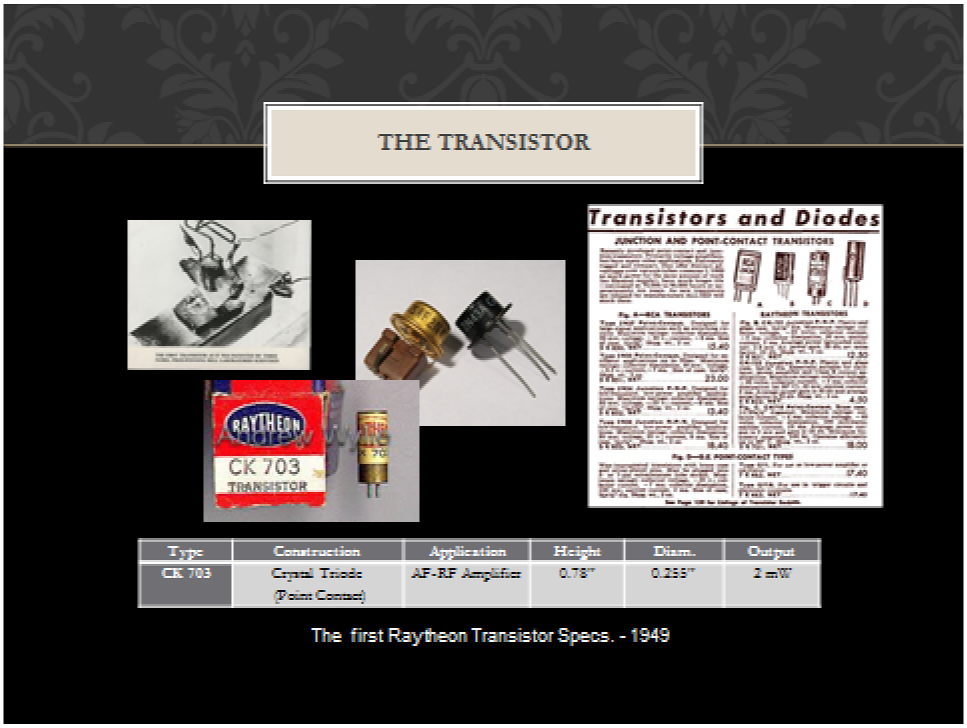
During this time, there were two main groups in the US pursuing significant work in vibrational spectroscopy: The group at Johns Hopkins University studied correlations between IR bands and functional groups, while Coblenz and his coworkers at NBS explored analytical applications. In addition, the theoretical development of the basis for IR spectroscopy was being pursued by two groups in the UK.

But then the situation changed abruptly with the coming of World War Two. In particular, a crash program was launched to develop IR spectrometers for the production of synthetic rubber, aviation fuel, and penicillin. This led to the first commercial infrared instruments, the Beckman IR-1 (1942) – designed by Robert Brattain of Shell Development and the Perkin Elmer 12 (1944) - originally developed at American Cyanamid.

The semiconductor field was also jump started by the War, driven largely by the need for high frequency detectors for microwave radar. The ultimate need was to push radar up to higher frequencies so as to increase its resolution and make it suitable for airborne operation. Much of this work was carried out at Bell Labs.

This work led eventually to the invention of the transistor in 1947 – by the same group at Bell Labs that had worked on microwave diodes during the War. There were so many really remarkable developments during the 1940's that it's almost impossible to single out one as being especially significant. However, a good candidate for the title of most significant might be the transistor. Its development formed the basis for the eventual development of the integrated circuits that dominate our lives today.

The development of high frequency semiconductor diodes during the war also eventually led to the invention of the first diode laser in 1962, ultimately leading to fiber-optic communications, which has been essential to facilitating the internet.

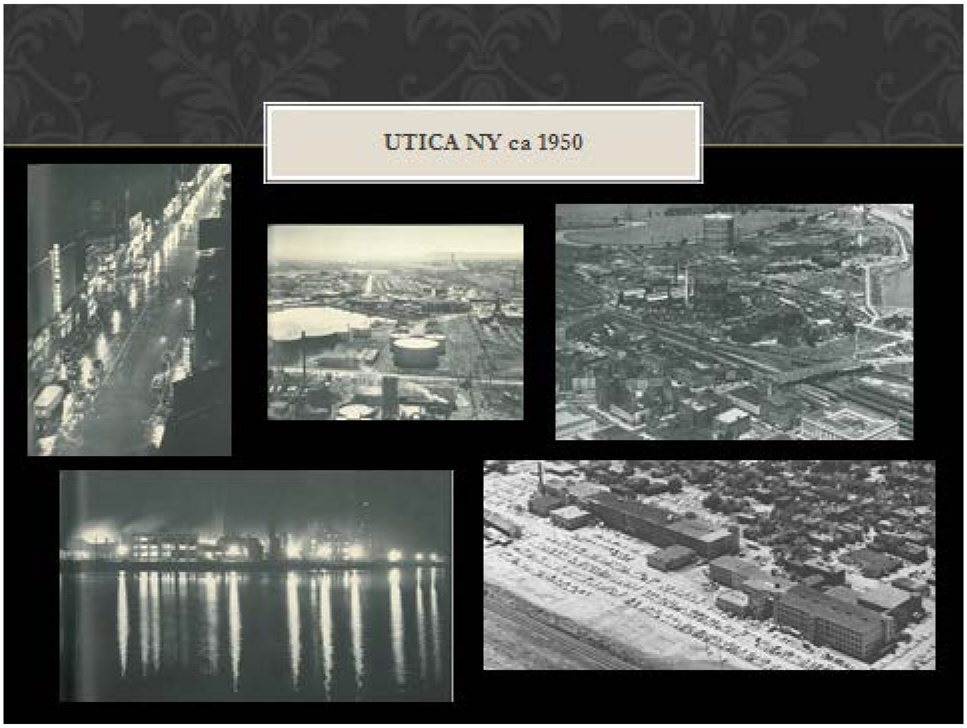


The first commercial transistors were introduced in 1949 by Raytheon. These were Germanium point contact devices – rather noisy and unreliable compared to the later Silicon junction transistors that eventually morphed into integrated circuits becoming the basis for the semiconductor revolution. Oh and the price – typically $ 15 - 20 each – in 1949 dollars.

In 1949 I was twelve years old – and for the first time I became seriously interested in technology. It was also the year that I started working part time in the family business fixing radios, and later, TVs.

Well, somehow I was able to get my hands on one of those very first transistors. To tell the truth, my father had taken a job at Rome Air Development Center and he was able to borrow one of the early Raytheon prototypes. I promptly used it to construct a miniature battery operated radio transmitter.

I recall making a portable antenna by taping a wire to a broomstick. I had one of my friends listen to my shortwave radio at home while I walked down the street reporting my location as I went. I got as far as the cutlery factory two blocks from my home where I was able to enhance my signal by connecting my ground wire to the chain linked fence protecting the factory.

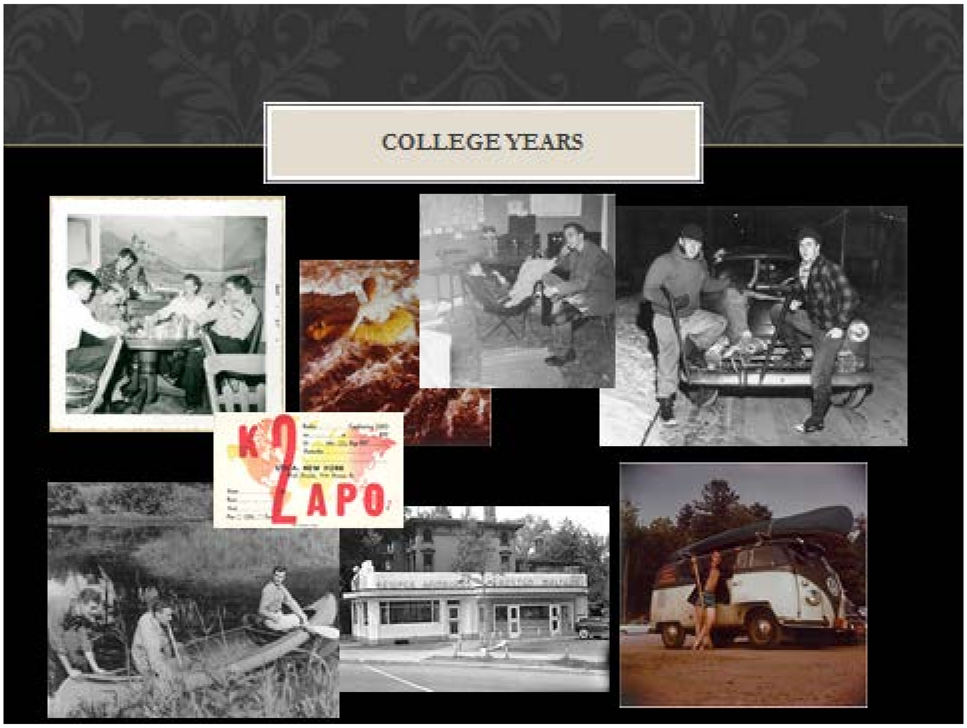


Oh yes, the factory down the street: In 1950, Utica, New York was a mill town. The Mohawk River flowing through the city was an open sewer. And you could tell which way the wind was blowing by the smell of the smoke in the air: whether from the perpetually burning city dump to the west, the coal gas (coke) plant to the north, – mixed with the smell of hops from the brewery – or the dense smoke from the coal fired boilers at the various cotton mills, largely to the east. This was the way it was. And I don't think that any of us living there at the time thought that it would ever change. It was yet to be some 25 years before the clean air and water acts.

How times have changed. The Mohawk is now a fine trout stream.

But I survived. And eventually the smoke started to subside as the knitting mills closed one by one – leaving a lot of vacant manufacturing space. In fact, the departure of the mills turned out to be a lucky break for kids like me as the city fathers were able to attract several high tech businesses such a General Electric, Sperry Univac, and Bendix to fill the space. At one time, Utica was the headquarters of three different GE divisions and a major operating location of a fourth.

By the time I was in college, the building in the lower right of the above illustration was an operation of the GE Heavy Military Electronics Department. I worked there during my last year in college as a Component Engineer.



And somehow I got a college education, attending Utica College (a branch campus of Syracuse University), which at the time was an inner city school three blocks from my home. How did I spend my college years? Hunting, fishing, hiking, exploring, amateur radio, frequenting the local night clubs (the drinking age was 18 then.), playing in a country western band – The Canadian Stars – and, of course, working.

This didn't leave much time for my studies. But I did manage to graduate. In fact, as I mentioned a minute ago, I was able to accelerate my course work enough to hold down a full time job as an electrical engineer at GE during my last year of school – taking my last few courses at night.

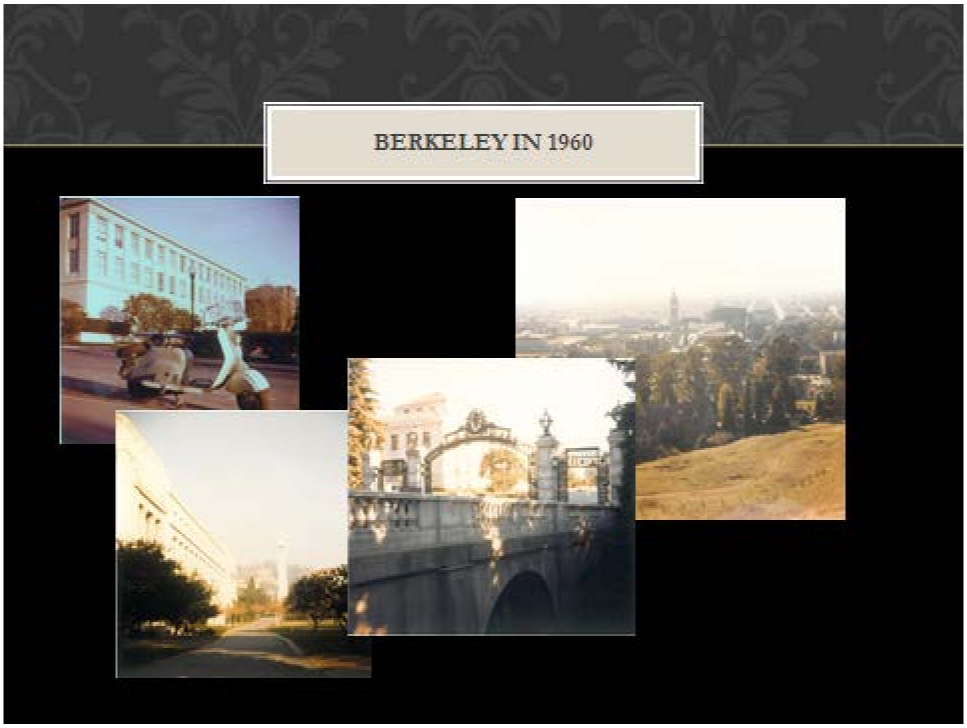
(In the early days of the Cold War, it seems that almost anyone could get a job as an engineer.)

Well, in reality, I was hoping to go to graduate school. So I desperately needed enough money to support myself for at least one year away from home. I thus applied at GE at the end of my junior year and took the exam that they gave to prospective engineers. I passed with a high enough grade to be hired as a component engineer.

The first of the many random events that did so much to determine the course of my career, and lead me to eventually be speaking to you today, was the result of a seemingly trivial pleasantry.

As my German class was letting out one evening in June, my professor, Karl Wernert, asked me what I was going to do after graduation. I said that I was planning to go to graduate school and had been accepted at Brown University. "Brown? Why Brown?", he said, "Why don't you go to Berkeley? My nephew's a professor at Berkeley and says that it's wonderful school." "Gee", I said, "That's long way away." And besides, it was June all the schools had closed their admissions in March. "Don't worry." He said, "Give me your application." Well two weeks later I was accepted at Berkeley. Needless to say that was the first of several major forks in the road – which totally changed the direction of my life.

Karl Wernert's nephew turned out to be Bob Karplus who got his PhD at Harvard at age 21 and was already a famous theoretical physicist and a full professor at age 26.



When I got to Berkeley, I quickly realized that I had only been accepted because of my personal connection. I was nowhere near qualified – coming from a two bit college with a mediocre average – and lacking three of the undergraduate courses required for a BA in Physics. (I took electronics courses instead.) And to make matters worse, I was required to take a series of do or die preliminary exams at the end of my first semester covering all of the required undergraduate courses.

Needless to say, I had a lot of catching up to do that first semester – learning three undergraduate subjects on my own while taking a full load of graduate courses.

But fortunately, Bob Karplus and I hit it off very well. In fact, he gave me the A in first semester quantum mechanics that made it possible to maintain the B average necessary to stay in graduate school. At the end of the semester, I somehow managed to pass both the written and aural preliminary exams. (About 33 % of the graduate students didn't.)   After that, I managed to get straight A's. And, I was able to finish my PhD in a little over three years. Some sort of a record, at least for the group I was in. I also wrote the shortest thesis to date for anyone getting a PhD in the group. (I was always in a hurry.)

But let's fast forward to the second famous person that I got to know, my thesis advisor, Bill Nierenberg.

Nierenberg was indeed very famous, but I had trouble figuring out what for. During the time that I was in his group, he was seldom around, spending most of his time in Europe as science advisor to NATO. He seemed to be much more attuned to the politics of science than to the science itself.

This was brought home to me one day when I walked into his office to have him sign my final papers to get my PhD. He said, "Well, if it isn't the boy scientist." (He always called me that.) "Have a seat." Now that you're about to go out into the world, let me give you some advice. Do you see what I am doing?" I said, "No." "Well, I have in front of me the latest issue of Physical Review Letters. I'm writing letters to each of the authors praising them for their work." It really pays off.

To this day, I haven't found the time to follow his advice. But I'm sure the approach was effective since he subsequently spent over 20 years as the director of the Scripts Institute of Oceanography.

Nierenberg pushed me very hard to stay in academia. In fact he was instrumental in getting me an offer of an Assistant Professorship at his alma mater, Columbia University. The offer was at $ 7,000 a year. When I wrote back to Columbia and said that I couldn't live in New York City for $ 7,000 a year, they increased the offer to $ 8,000. But by then I had received an offer to start a gas laser program at Hughes Aircraft for $ 14,000 per year. So I set out down another fork in the road.

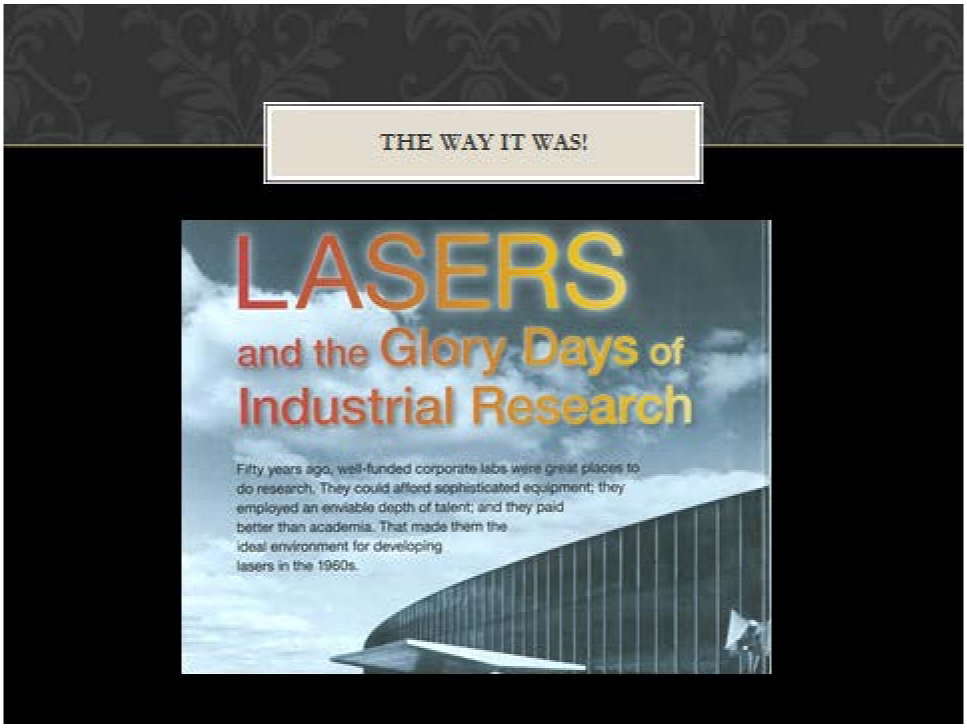


Illustration taken from the lead article in the May 2000 issue of Optics and Photonics News.

Laser development was certainly the place to be during the early 1960s. But my brief career (9 months) at Hughes Aircraft is most remarkable for another of the famous people who have effected my life - my technician.

Ted Maiman had built the first successful laser at Hughes 3 years before I had arrived. And, as a result, the company had already obtained some substantial contracts to work on laser radar and weapons development. I was hired to start a gas laser program to broaden the Company's offerings.

But, when I showed up at Hughes after completing some post doc work at Berkeley, I found that the budget for my program had vanished. The company's largest contract had been cut back. So they cut out all discretionary funds in order to support the existing staff. That included my capital equipment budget, my funds for supplies, and for my staff. Only my salary was secure.

But, it turned out that I did have some good luck. The same day that I reported for work, another new hire, John Ward, was assigned the office next to mine. John was a full Professor at Johns Hopkins University who had decided to take a job in the aerospace industry during his sabbatical. Aerospace companies liked to put famous people on their staff as window dressing to help them get contracts. (I later learned that John had gained fame in theoretical high energy physics for something called "The Ward Identity".)

So, when John and I compared notes, we found that we were a perfect match. I had an assignment but no funds or staff. John had a position, but nothing to do. So he volunteered to work for me.

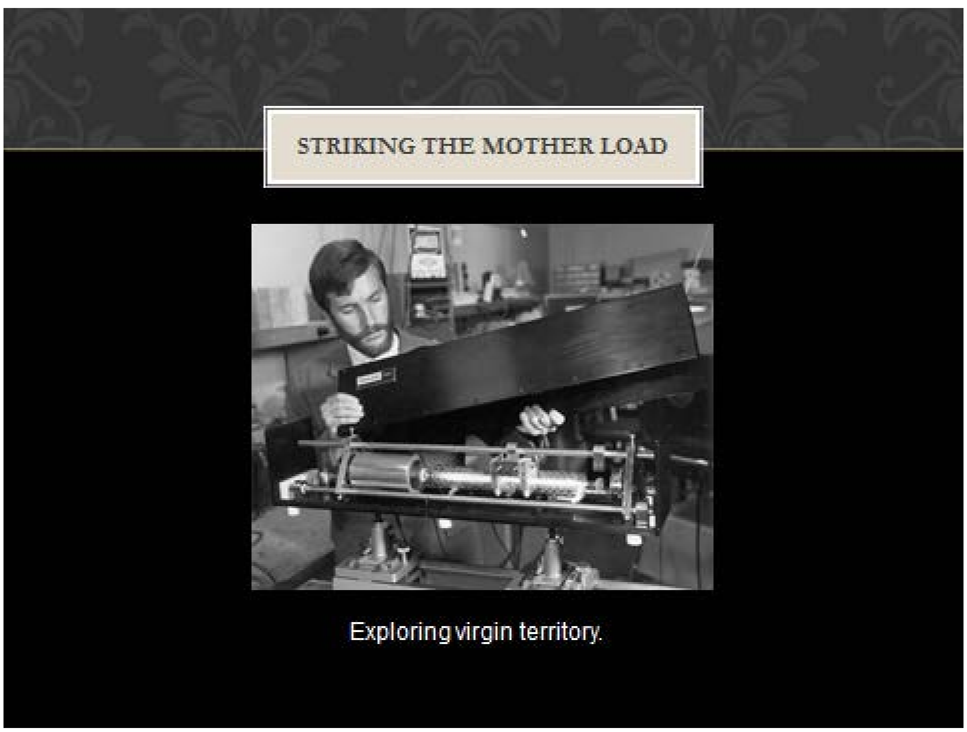
So I told john that our first task was to spend at least two weeks in the library studying the tables of atomic energy levels and excitation cross sections so as to come up with some candidates, for building new lasers. " No!", he said. "That's your job. I'm on my sabbatical to escape from theory. I'll spend my time learning to be a technician." So John went off to study glass blowing, optical polishing, and diamond machining, while I poured through journals and books.

Well to cut to the result, John was able to build the structure for a laser by cutting apart and reassembling a travelling wave tube. He was very upset when I insisted on removing the spiral electrode that ran the length of the tube. But I pointed out that it would short circuit our gas discharge.

We made a couple of abortive but interesting attempts at building lasers. (In one case, we accidently learned how to polymerize cyanogen, C_2N_2.) But then we found a system involving mercury that yielded some fairly strong pulsed laser lines. I had trouble finding atomic transitions that matched the frequencies of the new lines, but eventually assigned them to some rather obscure lines of mercury – and published a paper.

Soon after that, John decided to leave for the University of Madras in India where he had an outstanding offer to give a series of lectures, and I decided to accept an offer to join a friend already working at Philco Ford Research Labs.

On my last day at Hughes, Bill Bridges came down from Hughes Research Labs and asked if he could have my equipment. Soon after that, he reinterpreted my new laser lines and announced the discovery of the first ion laser. So much for a fleeting chance at fame! But I was still wet under the ears – less than a year out of graduate school.



My new job at Philco Ford represented a much greater opportunity. Not only did I have a reasonable budget, but I was partnered with my friend from graduate school, Matt White whose talents were a very good complement to mine. (I kept coming up with crazy ideas while he used his considerable math skills trying to prove me wrong.) And, best of all, our initial assignment, the development of a laser gyroscope, took us into a rich vein of ore, an area of science and technology that had barley been explored.

Until we got involved, most gas lasers were constructed in such a way that their operation was restricted to a single linear polarization. And the theory explaining their operation, which was developed at Yale

University by Nobel laureate Willis Lamb, was similarly restricted to a single polarization state. Despite this limitation, Lamb's theory was quite profound, and he certainly paved the way for the work that Matt and I subsequently carried out. So, in this respect, even though I had only met him once, Willis Lamb was certainly one of the great people who had a significant influence on my life.

For our gyroscope, we needed two separate laser oscillations travelling in operations around a ring. And, we needed a frequency shift between the two oscillations. This would provide an extremely accurate measure of the speed of rotation of the device.

To accomplish this, we eliminated all polarizing elements in the laser cavity and inserted a weak birefringent element. Once we got the ring laser running, we immediately started observing some very interesting phenomena. Not only were the oscillations restricted to orthogonal polarization states, but there proved to be some rather unexpected and interesting interactions between these states.

At this point we temporarily set aside the gyroscope project and constructed a straight (non-ring) laser that we could fill with a variety of different gases having laser transitions. We quickly observed that the oscillations, as well as the interactions between them, varied according to the detailed nature of the atomic energy states involved. And none of this was predicted by the current theory of gas laser operation. Well! A rich vein of ore indeed.

Ultimately, our discovery of the dual polarization laser led to some 8 patents as well as some rather significant papers.

In order to explain the various phenomena that we were observing, Matt and I developed a new theory of laser operation which took into consideration all possible polarizations states. This took several months and required both of our individual skill sets. In particular, Matt was able to master the intricacies of the Gibbsian statistics that were central to the Lamb theory. I could never make sense out of this business even though when I took the subject in graduate school, I got the second highest grade in a class of 70 students. Quite a fluke!

My part of the project was handling the three dimensional aspects of the task and, in particular, sorting out the really messy equations that resulted.

The success of our work during this period brings to light some of the important lessons that I've learned over the years.

- Get in early but avoid popular trends
- Go where few others have tread
- Look for phenomenological richness
- Be audacious

My penchant for doing things differently has always extended to my personal life as well as my work. When it came to vacations I managed to talk the management into letting me accumulate my vacation time for two years and then add on another week for weekends and evenings worked. That would give me enough time to do something interesting like travelling behind the iron curtain. This could be rather interesting in those early days.

I even happened to be in Prague on August 21, 1968 when the Russians invaded Czechoslovakia. And, unlike most of the tourists who were visiting Prague at the time, my wife and I weren't comfortably situated in a hotel. No! We were out on the street pulling suitcases and negotiating our way between tanks and armored personnel carriers. Since we had been staying at a private home in the outskirts, my wife and I were not able to reach the downtown area where the U.S. embassy was located. (The main road through the grounds of the presidential palace had already been blocked off by the Red Army.) But we did eventually find our way to the home of the American ambassador, where we ended up doing some sightseeing in the midst of the Soviet army with the ambassador's wife and a Czech houseboy who spoke some Russian. The blond teenagers riding on top of the tanks thought that they were on maneuvers in Lithuania. They were mostly country boys who had never before seen a large modern city like Prague.

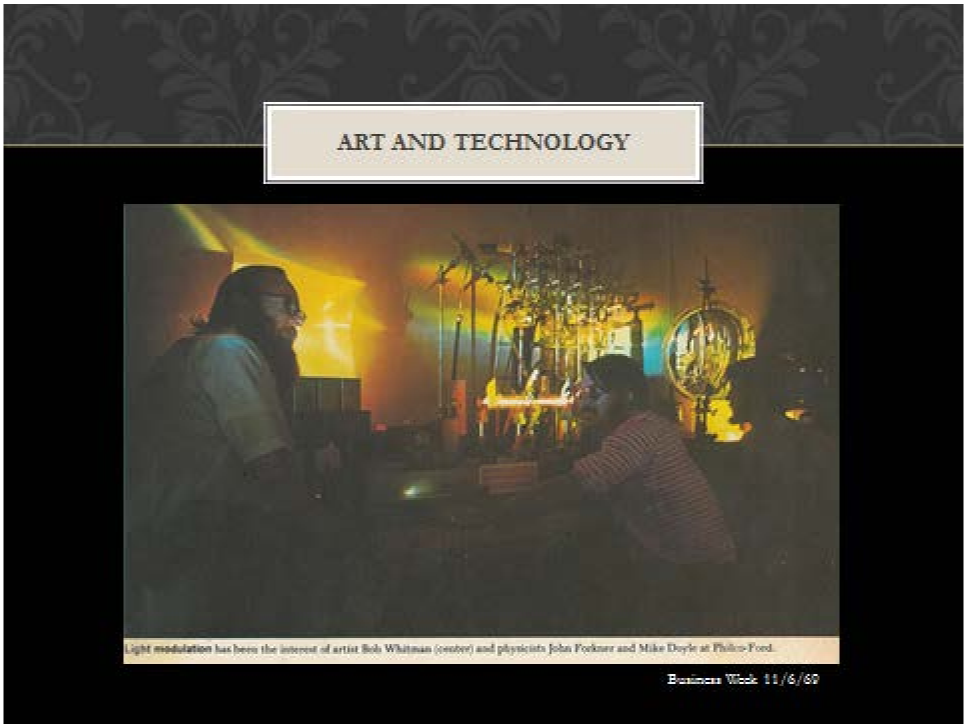
Later, during the same trip, we found ourselves in Izmir, Turkey in the midst of anti-American riots.

During the 1950's and early 1960's it seemed that every company worth a plug nickel had to have a research lab. And the government was very generous about funding the research through the Independent Research and Development (IR&D) program.

By the late 1960's much of the aura that surrounded industrial research had worn off. And the sponsors were starting to demand that development be directly tied to a specific weapons program.

In the case of Philco Ford Research Lab, the staff had shrunk from some 300 scientist when I joined the organization to a mere handful. And, by 1969, I was the last person in the organization with an IR&D budget. So even though I still had a couple of government contracts that I could use to supplement my IR&D budget, I could see that I would soon be under pressure to become more relevant. So I started thinking about new directions.

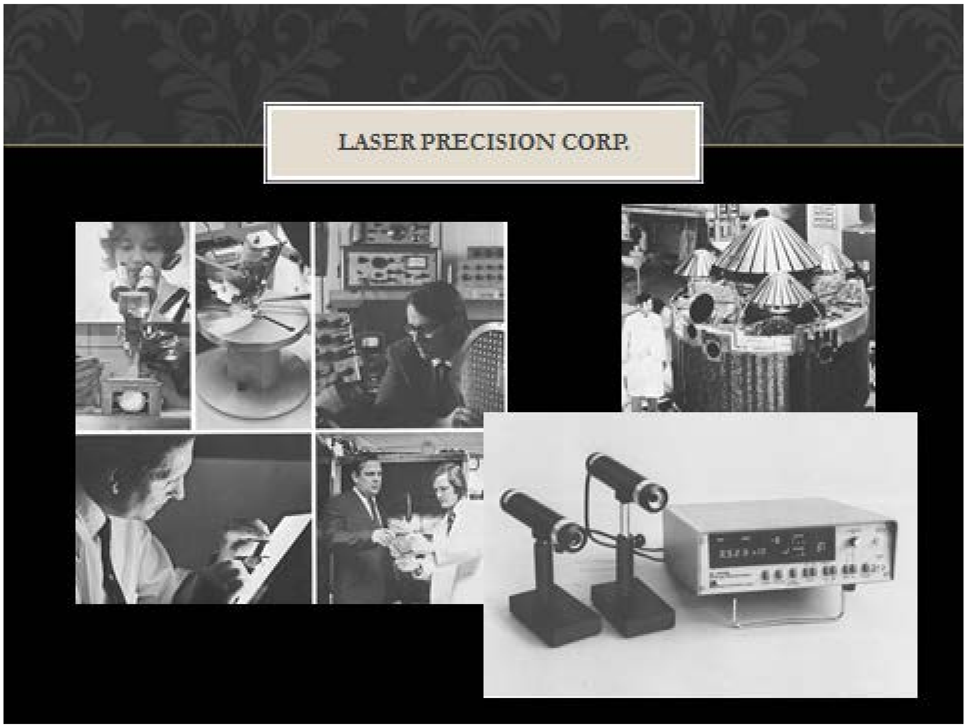
But, in the meantime, a new diversion came along. I was chosen to be company's representative for a nationwide program integrating technology and art. (I guess that I was the only person in the research lab with a beard.) My first task was to interview various artists and select one to be our artist in residence. I chose a young guy named Bob Whitman who wore pink tinted sun glasses and stripped polo shirts. He said that he wantd his presence to be disruptive to the organization. I also enlisted one of our staff members, John Forkner, who actually had some experience combining science and art.



This photo had nothing to do with the project that we eventually developed. But the folks from Business Week wanted something colorful. So we lit up my lab.

The result of our endeavor was the creation of a very clever optical environment. If you walked into it you would see your reflection as if you were somehow turned inside out. In other words, you nose would appear to be the most distant part of your face. I left before the project was finished. But it was quite a complicated contraption and took a large crew of volunteers to assemble. It ended up being used as the focal point of the Pepsi exhibit at the Tokyo World's Fair.

For reasons that seem rather arbitrary in retrospect, I decided to move back to my home town, Utica, NY, and join an old college friend who wanted to start a business. He had some good financial contacts which made it possible to raise the needed seed money. But he didn't have any realistic product concepts. That's where I came in.



I don't want take the time to bore you with the early history of Laser Precision Corp. But let's just say that the business was a bit tenuous for the first two or three years. Oh! The name? The investment bankers insisted that the name include the word "Laser". It was quite the buzz word in those days.

The early products of LPC were largely based on pyroelectric detection – another fairly new field at the time. PE detectors were already starting to be used in the early FTIR spectrometer. We pioneered their use for radiometry and laser instrumentation. In addition to building instruments such as laser power and energy meters, we obtained several government contracts to build specialized instrumentation for the Air Force and NASA. These included the design and manufacturer of the scanning spectral radiometric arrays for the Pioneer Venus and Galileo Jupiter space vehicles.

So let's finally fast forward to the next random disconnect, the story of how I happened to get into Fourier Transform Infrared Spectroscopy (FTIR).

It all started with an argument. Now, I've always been rather mild mannered – almost never getting into arguments. But it just happened that around 1975 or 76 I became rather upset with my business partner, the guy who was the company President. I don't want to go into details about this. However, his response to my bitching was to start looking for someone else to head up product development.

By this time, I had moved the product development activities to California to escape the harsh Utica winters.

Well what should happen but one day a guy walked in the door of the Utica facility looking for financial support to develop a new product -- an industrial FTIR spectrometer. My partner sent the plans to me to review. My conclusion was that the proposed design didn't make sense optically. Never the less, my partner was determined to get rid of me so he hired the new guy anyway.

After several months it had become apparent that lots of components had been purchased but nothing actually built. So I announced my intention doing a thorough project review during my next trip east. Well, when I arrived, my erstwhile replacement had vanished. All I found was a large cardboard box of parts but no drawings, circuit diagrams, or other plans. I promptly sent a letter to my replacement's last known address telling him not to return.

What to do now? We had invested quite a bit of money in the project and had nothing to show for it. But by now I had taken the time to read through the Proceedings of the Aspen Conference. So I guess I was hooked. – Another audacious decision made for all the wrong reasons.

And so, I returned to Irvine California and told my coworker, Bruce McIntosh, that we were going to drop the various other projects that we were working on and switch to building FTIR spectrometers. And, as they say, the rest is history.

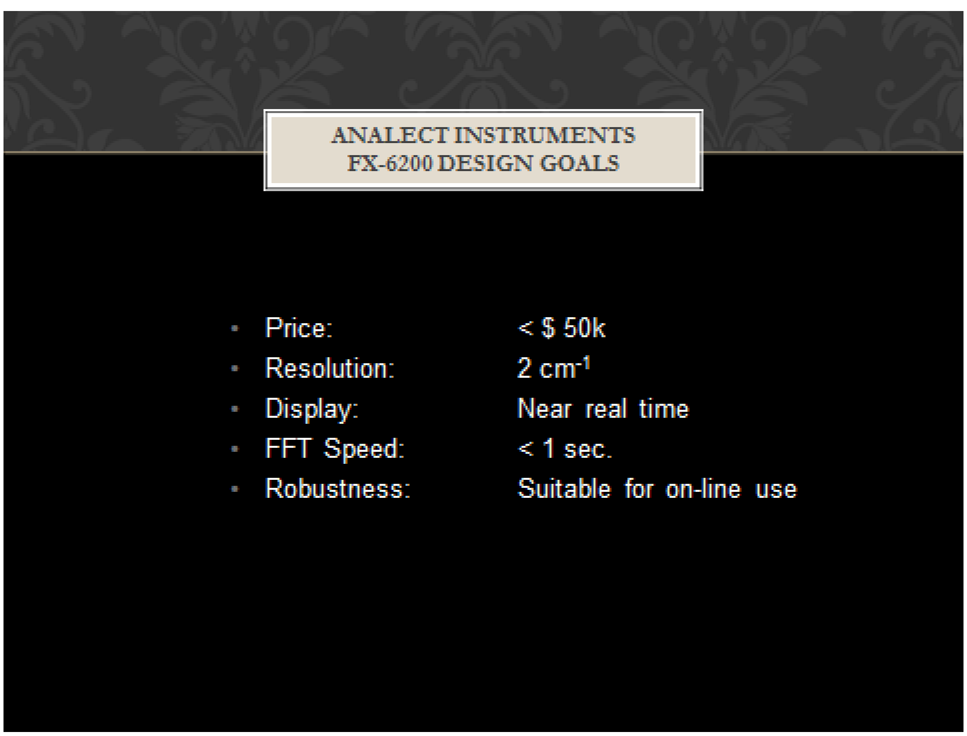
I'll have to admit that the decision to get into FTIR didn't come completely out of the blue. After all, I had been involved in the rather spooky world of infrared science for several years and already knew several of the players in FTIR such as Gerry Auth, Tom Dunn, Ray Milword, and Heinz Lossau. So I guess I knew something about what I was getting into.

Regarding the Spookyness of IR: It's hard to believe in retrospect that everything to do with infrared was considered a subject for secrecy in the 60's and 70's. To even have access to information on the subject, you needed to have secret clearance and be a member of IRIS - The Infrared Information Symposium. Needless to say, there were some rather unique characters in this business. Gerry Auth will have some comments about a few of them in his talk.

Yet, my decision to enter the FTIR field was audacious, especially in view of the fact that Laser Precision was always strapped for cash and had sales of only around $ 1 million – of course in 1976 dollars. But to have any chance at all of breaking into FTIR, We needed to have a number of clear cut advantages.

At that time, the only significant competitors were Digilab (the original FTIR manufacturer) and Nicolet (who had purchased Gerry Auth's design from EOCOM). And the only FTIR instruments were high resolution, minicomputer based models selling for over $ 100k.

So our design goals were clear:



Meeting these requirements necessitated a few innovations:

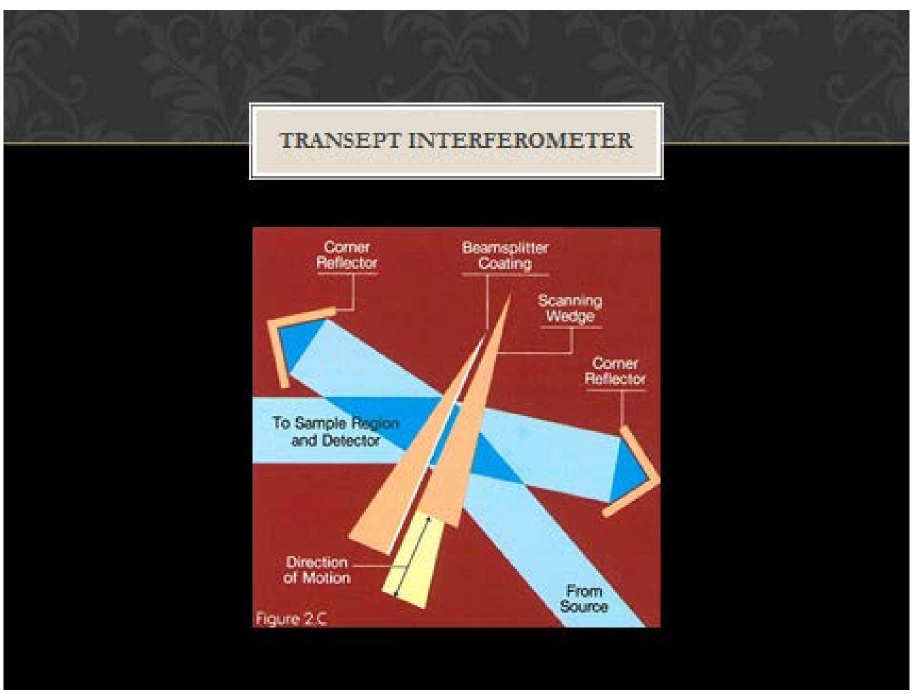
- The first microprocessor based FTIR spectrometer
- Hard wired Fast Fourier Transform (FFT) processor
- Refractive scanning
- Array-mapped display

The use of microprocessors seemed obvious to us at the time since this was clearly the wave of the future. But given the performance of the early microprocessors, it took at least four of them to accomplish everything we needed. Indeed it was to be several years before any other manufacturer made the transition to using microprocessors.

The Fast Fourier Transform (FFT) was a real challenge. There was no way one could perform an FFT using the personal computers of that era in less than a few minutes. The solution to this problem involved some luck – in the form of William L. Clarke. Bill Clarke was a recent physics graduate from UC Irvine who happened to answer our ad for a design engineer. Not only did he turn out to brilliant hardware and software designer, but he was also in the process of designing is own music synthesizer based on a hardware implementation of the fast Fourier transform algorithm. In principle, all he had to do was to expand the size of his FFT board by something like a factor of 4 or 8 to build a hard-wired FFT board for our spectrometer.

We solved the problem of achieving robustness by using a moving refractive element rather than a moving mirror so as greatly reduce the sensitivity of the scanning mechanism to spurious vibrations. I'd like to be able to report that the eventual design was the result of a moment of illumination. But in reality, it resulted from a series of iterations.

Here's the final result, which I called the transept interferometer. (In his book on FTIR, Peter Griffiths referred to it as the "Doyle" interferometer.)



The reason for the unique appearance is the need to have the two arms of the interferometer fully compensated (i.e. balanced) to eliminate beam movement and refractive distortions during scanning.

The desire to have a near-real-time display presented real challenge since the early bit mapped computer displays were simply too slow to meet our needs. We solved this problem in two stages. The first was to use an oscilloscope as the display as was the practice with the expensive “research grade” instruments then on the market. The second stage was the development of the Array Mapped Display, which I’ll touch on below. Our first FTIR was the FX-6200.

As noted above, for this first model we used an oscilloscope display. This provided all the speed we need with minimal required engineering. A couple of years later, we replaced the oscilloscope with an even more innovative design – the Array Mapped Display. Here it is in the RFX-75.

I don't want to take the time to discuss the Array Mapped Display in detail. But a key element (in addition to some very unique electronics) was using a conventional CRT tipped on its side so as to achieve required Y axis resolution.

Our experience in the early days of Analect Instruments again illustrates the benefits of getting into a new field of endeavor early – in this case FTIR in general and refractive scanning in particular. There were still plenty of opportunities for innovation. As a result, we were able to obtain a total of 10 patents governing FTIR instrument design and another 13 governing related systems and accessories.

But all of this is really getting ahead of my story. First we had to solve the problem of funding an ambitious development program. And we didn't feel that we could justify trying to raise investment capital for such an early stage venture.

My solution was to look for a much larger partner. The approach was simple. I simply created a list of some 14 major corporations that might have interest in a robust industrial FTIR spectrometer. I then contacted either the Marketing VP or R&D VP of each company. I seem to recall that I was able to contact most of them by phone.

Fortunately, Bendix Corp. was interested in bidding on an Army contract for the development and deployment of helicopter mounted FTIR spectrometers. It was an ideal match for our proposed capabilities. We signed a deal with Bendix which gave them the rights to this particular application in exchange for a substantial down payment and continuing engineering support for two or three years. This was adequate to fund the prototype development for the Army project as well as for our commercial instrument.

Eventually, we lost out in the bidding to a team headed by Honeywell with support from Nicolet. Apparently, they must have had problems meeting the requirements since, about a year later, Honeywell tried to hire me. I told them that they'd have to buy the company to get me.

To show what a small world this is, Gerry Auth eventually won a follow-on contract to the Honeywell program. It is my understanding that this led to the first compact Midac FTIR spectrometer.

But going back to the commercial side, we exhibited a prototype of our FX-6200 FTIR at the 1979 Pittcon. However, we weren't ready to go into production until a year later. In the meantime, Nicolet Instruments introduced their first moderately priced instrument the MX-1. So they had a one year jump on us when we started producing the FX-6200.

From the standpoint of performance, the FX-6200 had a number of advantages over the MX-1.

- Microprocessor vs. minicomputer
- Near-real-time CRT display vs. chart recorder
- Spectral output in 12 seconds vs. 1 – 2 minutes
- Robustness due to refractive scanning vs. a moving mirror

Despite these advantages the sales level of the FX-6200 in 1980 was roughly $ 1 million vs. $ 4 million for the MX-1. This result highlights the value of a one year head start and a much larger and aggressive sales force.

At this point, I'd like to fast forward, skipping over the next several years of history at Laser Precision Corp and Analect Instruments. I might comment, however, that I became the President of Laser Precision in 1978 and oversaw the Company's growth at a compound rate of 40%/year from sales of about $ 1 million per year to a level of $ 25 million per year when I eventually left ten years later in 1988.

The rapid growth was not only due to our expansion into FTIR spectroscopy. At same time, John Gentile of our Utica facility developed one of the first instruments for locating faults in the fiber-optic telecommunications systems just then starting to be installed. This instrument, called an Optical Time Domain Reflectometer (or OTDR) quickly established Laser Precision as a leading producer of fiber-optic test equipment.

As the business started to expand, we needed successive rounds of financing to fund inventory buildup, a direct sales force, and expanded product development. We met these needs with a combination of joint ventures and equity financing.

One joint venture is particular relevant to the subsequent history of our industry. It resulted from the fact that we didn't have the resources to adequately pursue both the laboratory and the process market. So we were willing to cooperate with a larger firm in the laboratory market in order to help finance our penetration of the process market.

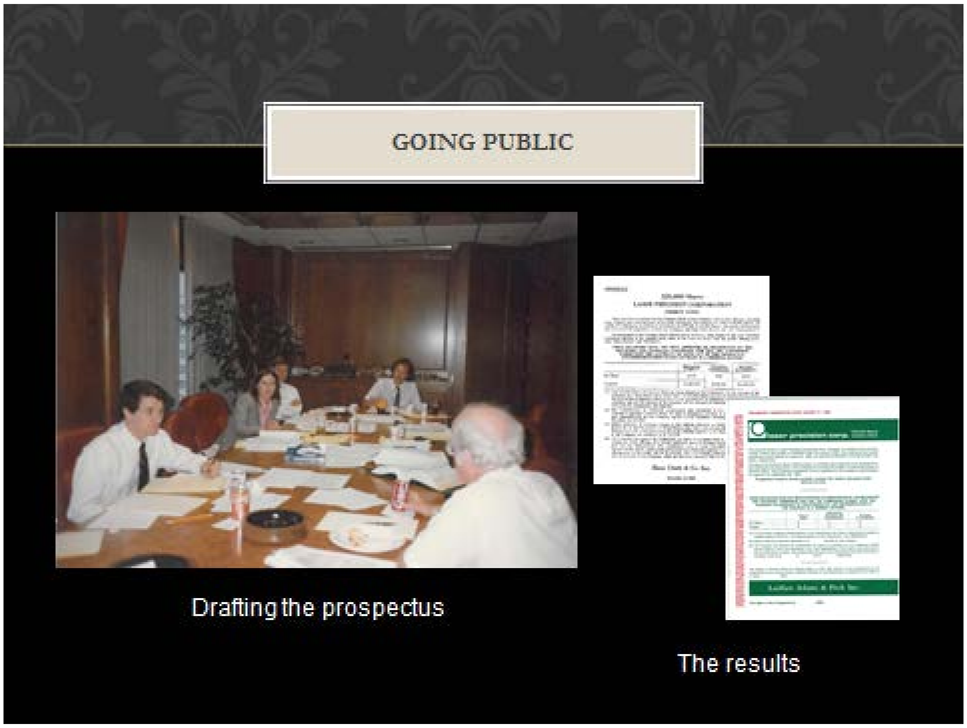
There were only two realistic candidates, Perkin Elmer and Beckman Instruments. These two firms had long had the dispersive IR market to themselves. But neither had yet introduced an FTIR. And, Beckman was only about a mile from us in Irvine CA.

After several preliminary meetings, the Beckman management called us in for one final meeting during which the Marketing VP lectured us at length about the broad capabilities of Beckman and the fact that they had no real need to do a deal with little Analect Instruments. But he allowed that they might be willing to cooperate with us if we would drop our requirement for a half $ million down payment.

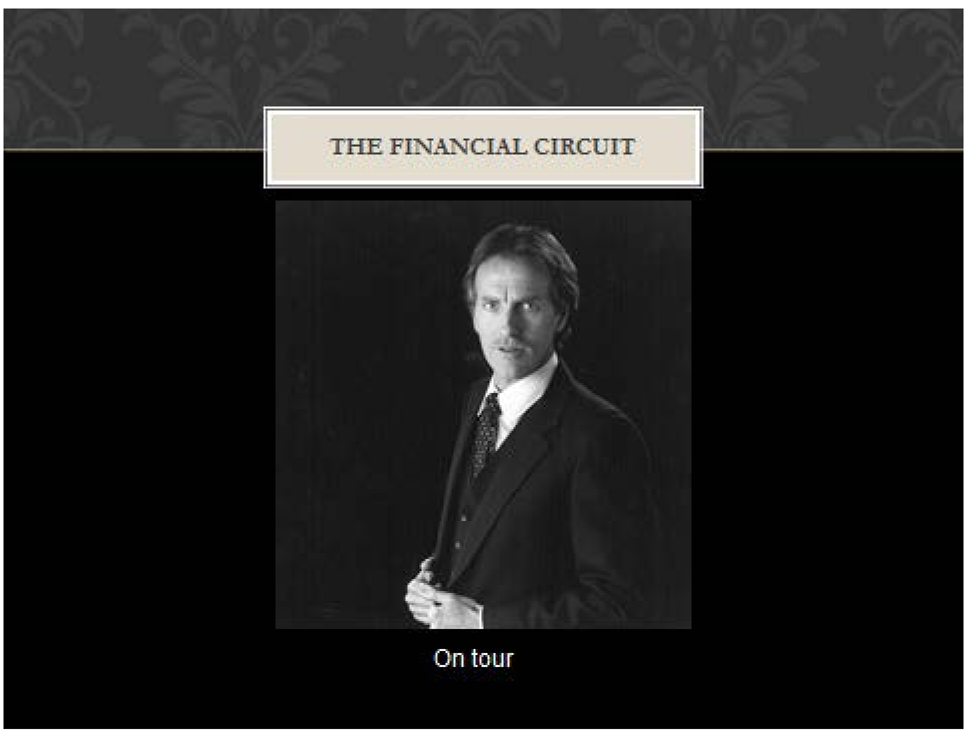
When he finally finished his diatribe, I stood up and said simply that if Beckman Instruments did not do this deal on our terms, they would be out of the IR business within three years. They were!

A few days after this meeting, I made one of my frequent trips to our Utica NY facility. While there, I received a phone call from a manager at Perkin Elmer asking about the possibility of obtaining a nonexclusive license to our FTIR technology. Well I said that I was leaving for California the next day and that I was about to sign a deal with another firm. However, I said that I was willing to change my travel plans to spend a couple of hours in Norwalk on the way back and that, if we could sign a letter of intent right away under appropriate terms, we might be able to do a deal.

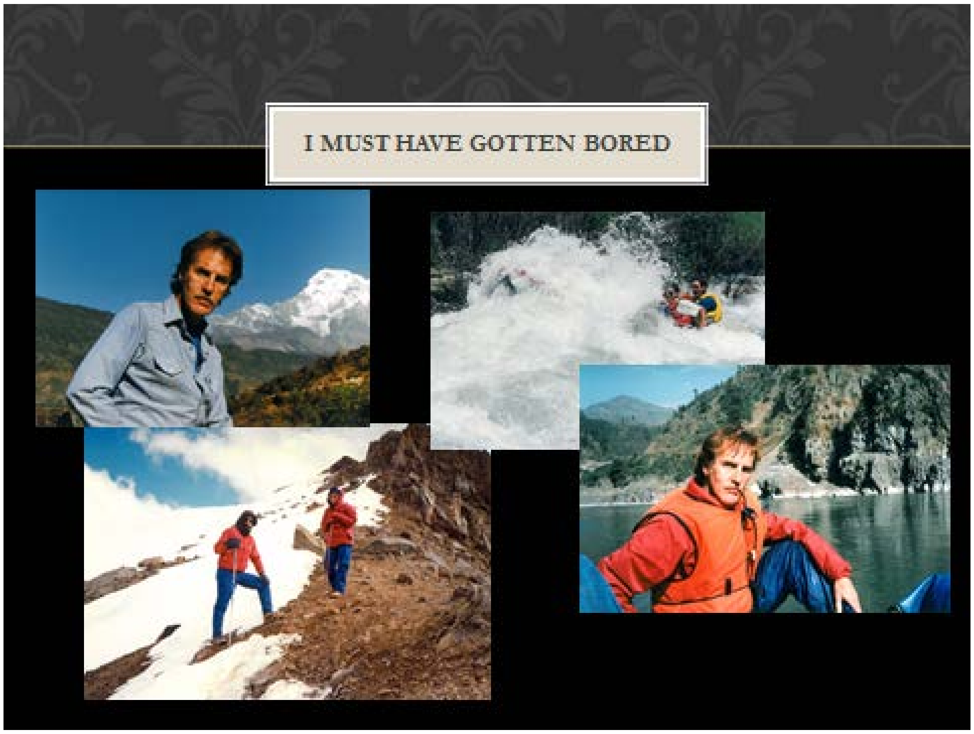
I drafted a brief hand written set of terms on the one hour flight to White Planes NY, essentially doubling all the numbers in my rejected proposal to Beckman. When I left Norwalk a few hours later, I had a check in my pocket for several hundred $k for an option on a licensing agreement. When PE exercised the option a few months later, the total came to well over a $ 1 million down payment plus ongoing payments for royalties and purchases of equipment.



Our equity funding efforts started with a public offering in 1980 and continuing with another round in 1983. Needless to say, I had to spend a lot more time than I would have liked speaking to groups of stock brokers, venture capitalists, and investment bankers. And, of course, I had a new, more serious image to project.



To counter the tedium of the financial circuit, I started taking more adventurous vacation trips. Here I am (clockwise from upper left) trekking in the Himalayas, river rafting in the Sierras, rafting in Nepal, and climbing Mount Kenya at over 16,000 feet. Imagine! Glaciers within ten miles of the equator.



Of course, with raising money, came an increase in the influence of the financiers, with the result that they came to dominate the Board of Directors. The end result was a coup de tat in 1988 during which the outside directors took over the business and dismissed the three inside directors and other senior officers. Needless to say, we felt that their motives were questionable. And, the profits decreased drastically starting the following year – partly due to the large consulting contracts awarded to each of the usurping directors by the new CEO.

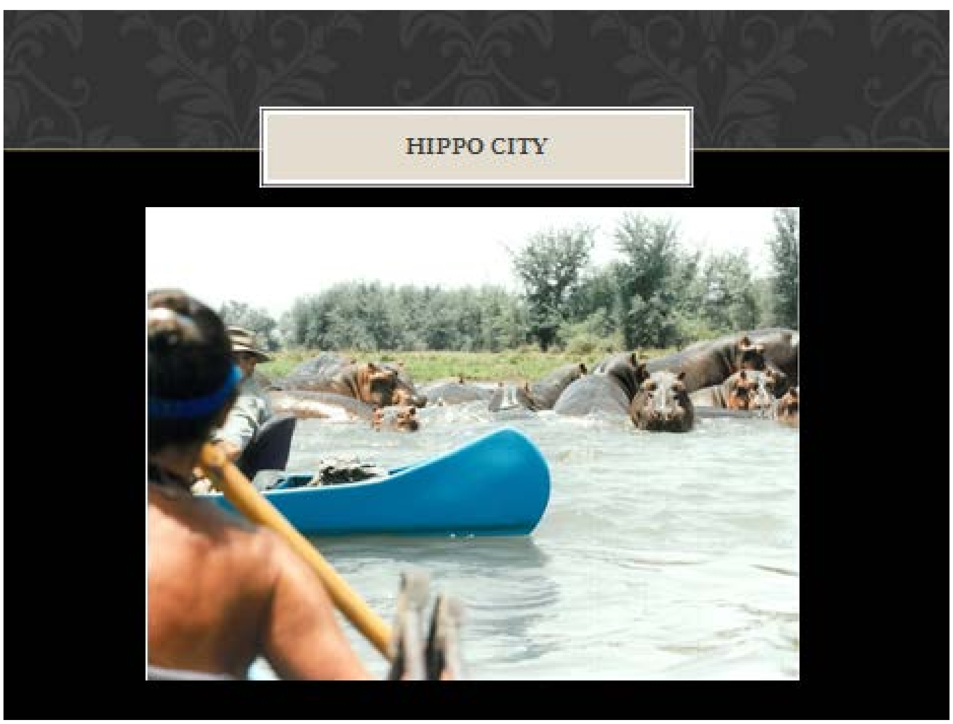
The circumstance surrounding our departure were rather interesting. But that's a story for another day.

Fortunately, Norm Jennings (my second in command at LPC) and I were able to leave on terms that provided us with the resources to start Axiom Analytical. And this time, we used a business model that minimized fixed costs and eliminated the need for outside investors.

The financial settlement also provided me with the means exercise my penchant for alternative travel – starting with a four day hike over the Milford Trek in New Zealand in late 1988. Even before this, I had started taking more adventurous vacations – as noted above - including a trip to the Amazon rain forest in 1984, Eastern and Southern Africa in 1985 and87, and trekking in Nepal in 1986. But I had really gotten hooked on Africa.

I eventually made a total of eleven trips to Africa, mostly to Zimbabwe, Botswana, and Zambia – each lasting typically 5 weeks. And I quickly learned that I really enjoyed getting face to face with all matter of animals whether hiking to remote bush camps or canoeing on the lower Zambezi River. I did 6 or 7 canoe trips of 4 or 5 days each on various stretches of the river. And most of these included some adrenalin pumping encounters with hippos, crocs, and even lions. Adrenalin does tend to be addictive.

There are lots of stories I could tell. But I'll content with just one snap shot. Hippo city!



Within less than a minute of taking this photo, I came quite close to being ground into the resemblance of well scattered hippo dung.

Immediately after taking the photo, I let go of the camera, grabbed the paddle, and made a hard left turn around a fallen tree to avoid running directly into the pod of hippos. However, the turn took us right into a sand bar. My wife, Dolores, and I had to get out of the canoe to pull the canoe across the fairly wide bar. Not being particularly smart, the dominant male must have decided that we were after his womenfolk. So he started to charge us. He stopped a few yards away, backed off a bit, snorted a few times, and charged again. He did this three times before we got safely across the bar and into the deeper water on the other side. In the meantime, our guide was paddling widely back up the river toward us yelling for us to leave the canoe and run for the shore furthest from the hippos. This just didn't seem like a reasonable option since we would still need the canoe to continue our trip. (No vehicle support on this stretch of the river.)

In fact, that was only the beginning of a very wild day.

Almost immediately after escaping the irate hippo, we came around a bend in the channel and almost ran right into a mixed group lions and crocs feeding on a dead buffalo in the edge of the water. The lions kept trying to chase the crocs away – with little effect. My wife was steadying the canoe by holding onto the water hyacinths while I videotaped the scene.

After that we had another run in with a pod of hippos. They came at us under water, first flipping the canoe right next to mine and then coming after us. I escaped by driving my canoe into the branches of an overhanging thorn tree – and got quite a number of lacerations to show for it.

And the fun continued. Next, we had to chase a couple of elephants out of our camp that afternoon before we could occupy the site. And then toward evening, we were hit by a freak wind storm which almost blew down our tents. While we were fighting to save the tents, the wind blew over a kerosene lantern setting our tent on fire and scorching my backpack.

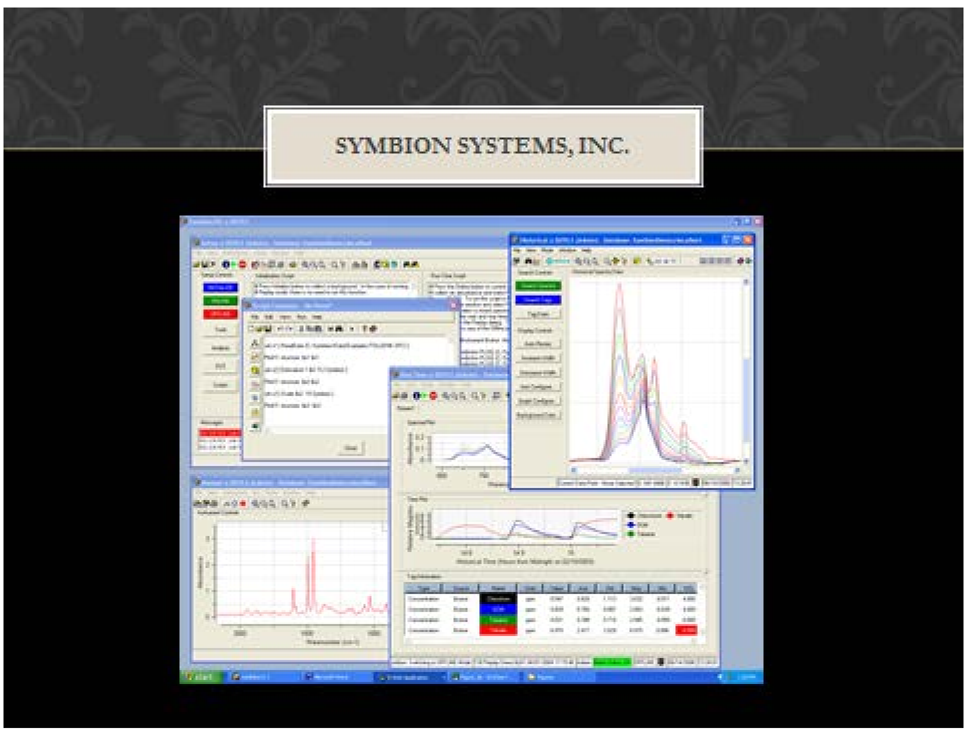
The story of our eventful day on the river quickly made the circuit of the bush telegraph, so that a week and a half later, when we were at another safari camp a few hundred miles away, we heard people talking around the fire about an especially wild canoe trip they had heard about. Needless to say, we were able to fill in more of the details.

Meanwhile, back to business. The other eleven months of the year were devoted to Axiom Analytical.

Although, Axiom started out manufacturing lab sampling accessories, much of our business quickly migrated to our first love, industrial process monitoring. And once again, just as in my forays into laser research at Philco Ford, radiometric instrumentation (during the early days of Laser Precision), and then FTIR spectroscopy with the development of Analect Instruments, we found ourselves in largely unexplored territory. As a result, we were able to establish strong patent positions in areas such as mid-IR ATR probes, fiber-optically coupled near-IR transmission probes, and fiber-optic multiplexers.

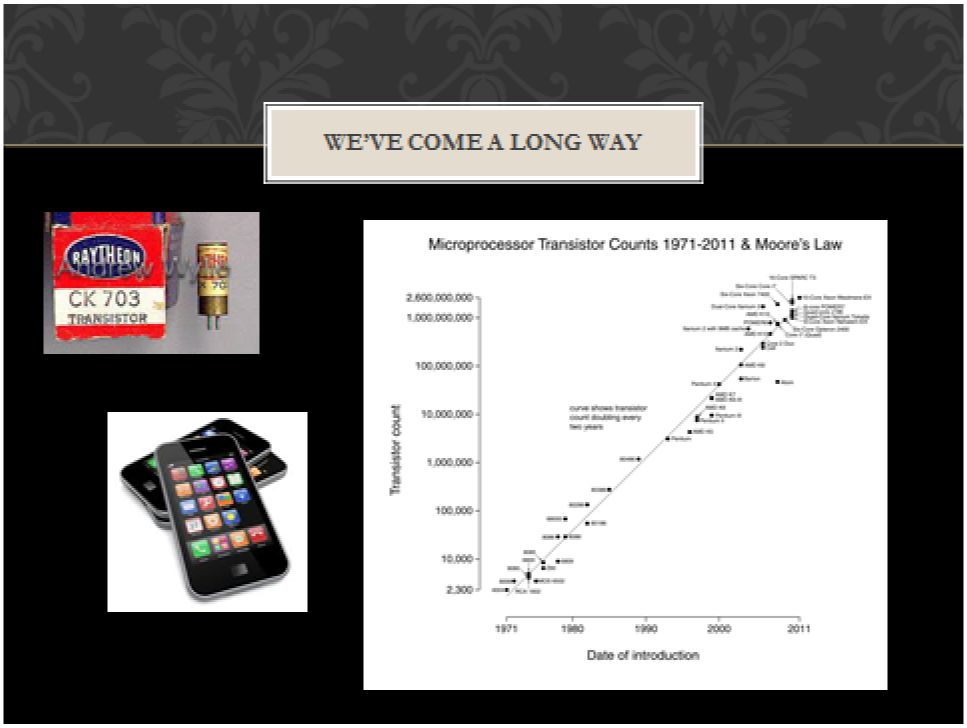
To date, Axiom has obtained over 20 patents. And both Axiom and Symbion Systems are in the process of filing several more right now. Undoubtedly the most important of our existing patents is not one of my inventions but rather is due to my business partner, Norm Jennings. This is the welded metal window seal that enables us to build probes that are virtually failsafe under extreme conditions of pressure, temperature, aggressive chemistries, and thermal shock.

Several years back, Norm and I became a bit bored just making analytical hardware. So we decided to start a second company, Symbion Systems, to produce instrument control, networking, and process monitoring software for the same market as our hardware products. We were very fortunate to find Mike Power to head up our software development activities. Richard Kramer, one of the speakers on the program today, has also contributed significantly to the development of the Symbion products.



In common with all of my ventures, launching Symbion required an influx of more cash than Norm and I were willing to invest. Once again we looked for a partner, eventually entering into an arrangement with the process automation business of General Electric. After three or four years we terminated the exclusive aspects of the GE deal since they did not meet their obligations.

The Symbion product family is continuing to expand, as we once again launch into virgin territory. Our latest venture takes advantage of recent developments in cloud computing to enable the secure global deployment of analytical instrumentation using mobile technology.



Well here we are today, each of us carrying a gadget in our pocket that contains something like a few hundred thousand transistors. I started my odyssey some 65 years ago building a radio transmitter with a single transistor that measured something like 3/8" long and a price in the tens of dollars. But, somehow, It doesn't really seem that different. And, of course, I am still having fun.

But, what have I learned? Certainly, not what you learn if you read any of the popular books on business strategy. The usual advice is not to be a pioneer. I don't know how many times I've heard VC's or investment bankers say: "You know what happens to pioneers. They get arrows in their backs." Now that I think of it, it was the board member who led the coup at Laser Precision (and eventually took over the business) who used to say that to me the most. Perhaps it was his way of letting me know what he was up to.

No! The conventional wisdom in venture capital circles is that you don't want to be a pioneer. Instead, the superior strategy is to practice "creative imitation". In other words, you learn from the pioneers and let them validate the market opportunity, and then move in with superior marketing and financial resources and take over the market. This was the approach advocated by Peter Druker in his highly influential book, "Innovation and Entrepreneurship".

THREE TYPES OF ENTREPRENEURSHIP (according to Druker):

- **"The fustest with the mostest":** E. g. Steve Jobs with the Apple computer. But being first leaves much room for error – allowing the entry of imitators.

- **Creative imitation:** E.g. IBM's entry into the PC market. Often requires vast resources.

- **Entrepreneurial judo:** E.g. Cannon's exploitation of gaps in Xerox' product line. Really another form of creative imitation.

Sure, creative imitation can work if you have access to lots of capital and are willing to sell your sole to the venture capitalists. But, keep in mind that there's a good chance you'll be out on the street in three years. Note also that Druker was in his 80's when he wrote the book. And he had only studied innovation in large companies.

Both out of necessity and inclination, I've taken a different approach. Besides I think that pioneering is fun. You certainly can feel pride in what you are doing. And you may even make a decent living and be able to take enough time off to spend a month a year tracking lions and leopards in Africa.

But don't expect to make a fortune – although there's always a remote chance you may get really lucky. And watch out for those guys with a pocket full cash and a quiver full of arrows. Too much success can make you a prime target.

So, if any of you feel like trying some pioneering, here is my advice:

- **Go where few have tread:** With luck, you might strike a rich vein of ore.

- **Look for fertility:** It allows you to keep your options open.

- **Keep an open mind and learn to tolerate uncertainty:** You will never have all the information needed to make a <u>safe</u> decision.

- **Don't bet too much on one roll of the dice:** No matter what, you must survive.

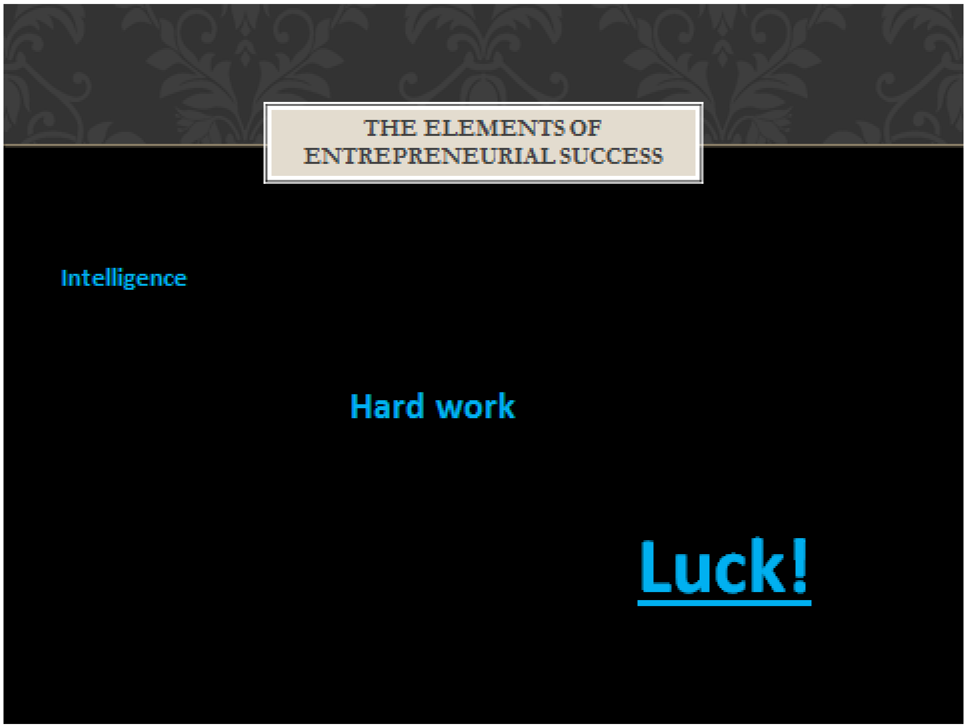
- **Be audacious!**

In addition, here are a couple more lessons that I've learned.

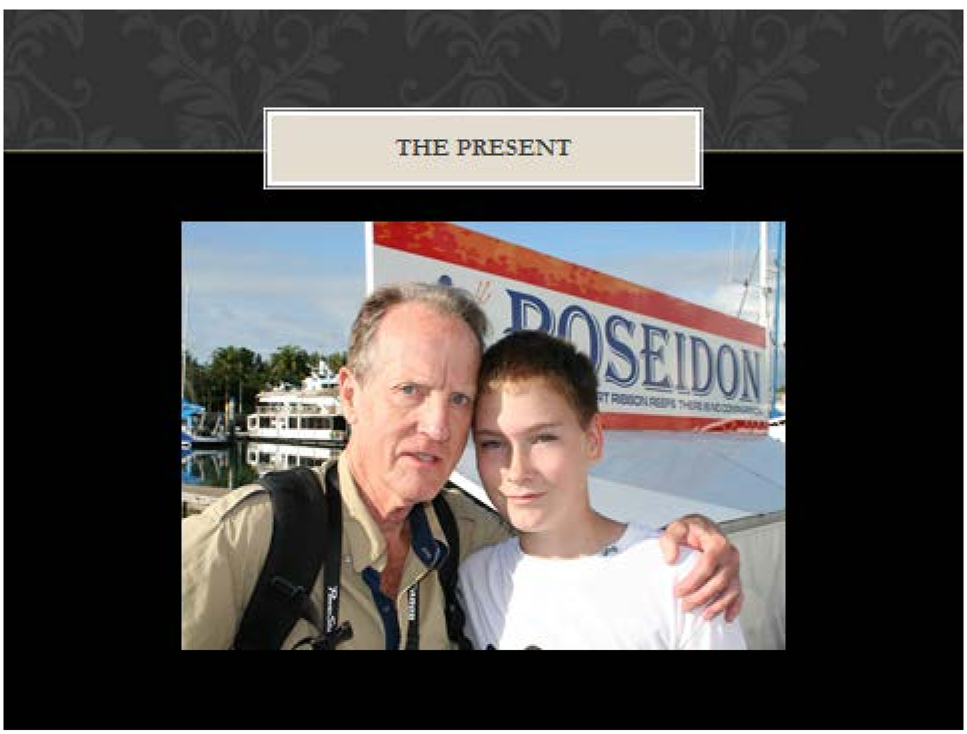
- **Don't be overly impressed by any one person or institution**. Over the years, I learned that more often than not, my competitors have more problems than I do.

- **Play in the biggest league you can.** My arrival at Berkeley is the prime example. I did much better there than at little Utica College.

Finally, as I've said many times over the past fifty years, there are three basic elements of success. And you can guess which one I think is the most important. Hence my advice not to bet everything on one roll of the dice.



OH! You may be wondering why I stopped travelling to Africa. Well, about 16 years ago, I remarried and had a child – my first and only. Now I find it more rewarding to travel with my son, James Patrick. And he doesn't want to visit Africa – Yet!



In closing, I'd like to acknowledge a few of my many collaborators.