50 Years of Holographic Interferometry

A monograph by Karl A. Stetson

Chapter 1. The Road Not Taken

The final months of the year 2014 marked the 50th anniversary of the discovery of the phenomenon of holographic interferometry. As the only person to work in the field for those entire fifty years, I felt moved to write a memoir to describe the events that led me to be at the right place at the right time to make this discovery and to describe the path my life took as a result. "The Road Not Taken" is a well-known poem by the American poet, Robert Frost, and it is applicable to my story. The poem goes as follows:

Two roads diverged in a yellow wood,
And sorry I could not travel both
And be one traveler, long I stood
And looked down one as far I could
To where it bent in the undergrowth;

Then took the other, just as fair,
And having perhaps the better claim,
Because it was grassy and wanted wear;
Though as for that the passing there
Had worn them really about the same,

And both that morning equally lay
In leaves no step had trodden black.
Oh, I kept the first for another day!
Yet knowing how way leads on to way,
I doubted if I should ever come back.

I shall be telling this with a sigh
Somewhere ages and ages hence;
Two roads diverged in a wood, and I –
I took the one less traveled by,
And that has made all the difference.

This poem has always spoken to me, and I feel it describes my life and career. For me, the road not taken, was back at the end of my senior year in undergraduate school. I was graduating from what was then Lowell Technological Institute (renamed from Lowell Textile Institute, subsequently the University of Lowell, and now the University of Massachusetts at Lowell) and I had an offer to work at Sylvania Electric Co. in Waltham, Mass. I don't recall exactly how, but I learned that Sprague Electric in North Adams, MA, was offering to interview engineers. It was late spring, the idea of a trip to the Berkshires seemed appealing, and so I agreed to interview and took the bus out for an overnight stay. The work they were doing was quite interesting. It was a primitive version of what would become integrated circuitry, and the offer they made was actually a higher salary that Sylvania had offered, but none-the-less I declined it. I had decided to try to go to graduate school at the Department of Applied Physics at Harvard, and the prospect of spending the summer near the Harvard Yard in Cambridge won over the mill town of North Adams. Had I taken the job at Sprague, my life would have followed a completely different path. That was my road not taken.

I had hoped that living near Harvard would allow me to be in contact with their admissions office, but that didn't work out, because they were only open during the week while I was at work. The room I rented on Mount Auburn Street was quite convenient and I had many interesting experiences. One of the noticeable buildings there was the Unitarian Church. What little religious training I had had as a young person was at the Unitarian Church in Concord, MA, so I was interested in what activities they had. One of them was a trip out to Wellesley, MA, to a production of George Bernard Shaw's play *Man and Superman.* I had encountered a recording in the form of a two LP set of the Hell Scene from that play the summer before, and its philosophy had made a strong impression on me. Accordingly, I was very interested in seeing the play. As those of us who had signed up for tickets gathered for the ride out, I met a young woman named Elaine Burns who was going to the Harvard summer school. I was young, ardent, and interested in poetry and music in addition to mathematics and engineering, and the play was a total revelation for me, for suddenly I saw the Hell Scene in the context of the whole play. I think my interest in the play, my experience with its fragment, and my general enthusiasm made a strong impression on her.

We began seeing each other and quickly formed a bond. Soon, it became clear that we wanted to live together. She had one more year for her bachelor's degree at the University of Michigan in Ann Arbor, MI, so I quickly applied to graduate school there. Her summer course ended, and she stayed on for a couple of weeks or so and then went back to New Jersey. I eventually packed up my belongings, took them to Railway Express to ship to the address where she was staying in Ann Arbor. I then took the train to Newark, met her parents, and flew out to Ann Arbor with her. I didn't even know if I had been accepted and only found out that I was when I got there. It turned out to be most practical for us to get married immediately so that we would qualify for a student apartment just off campus. I remember the address as 1444 University Terrace.

So it was that we spent the first year of our marriage as students. My first semester was very difficult because my classes were spread out over the week, and it seemed I was never able to find time to study properly. The second semester, I organized my courses so that they were on Monday, Wednesday, and Friday, leaving Tuesday and Thursday for studying. Elaine was finishing up a degree in elementary education, which would qualify her to teach in public schools. She was having difficulties studying, however. It turned out that she had severe far sightedness, especially in her right eye, and that the stress of close work affected her entire physiology. This came to light from a visit to a doctor in Ann Arbor and thus began a change in her life style under his treatment, and this continued to pull us back to Ann Arbor. We both graduated in 1960, she with a BA and I with an MSE.

Let us fast forward to September of 1962. We returned to Ann Arbor to live, and I had interviewed at the Radar Laboratory at the Institute of Science and Technology located at the Willow Run Airport east of Ann Arbor in Ypsilanti, MI. We rented a house on Jorn Court, in Ann Arbor, and after one year moved to an apartment closer to the campus on Michigan Avenue, a street that was only two blocks long and one block away from Packard Street. Before this, I had been working for Bell Aerosystems (formerly Bell Helicopter) in Niagara Falls, NY. They had been bidding for contracts for space flight simulators, and these required analysis of TV systems from the spatial resolution of their cameras and displays through to the temporal bandwidth of the electronics. It was thus that I got grounded in spatial frequency analysis, and learned practical applications of the Fourier transform theory I had learned during in a master's degree course taught by Wilfred Kaplan from a mimeographed copy of his book manuscript. The salary I was offered at the IST was slightly less than I had received at Bell Aerosystems, but I felt it was a good opportunity.

One of the first things I did was to accompany the head of the group, Wendell Blicken, on a trip to Newport, Rhode Island, to visit the Eppley Laboratories. We were buying one of their thermopiles in order to measure the output radiation of a laser that had been ordered and was expected shortly. The trip was interesting, and I remember on the way back we sat next to the concert pianist Julius Katchen who was very talkative and with whom I had a very enjoyable conversation about music. After we returned, we received the thermopile and soon after that the laser, which was sold as a cooperative product between the newly formed company in California, Spectra Physics, and the established optics company, Perkin Elmer, in Connecticut. PE provided the mirrors while SP provided the rest of the system, a collaboration that lasted for one year. This was the model 110 laser, which had a radio frequency generator mounted in the center of the unit which generated a 40 MHz field that ionized the Helium-Neon mixture in the discharge tube. The tube was terminated by two inclined windows set to Brewster's angle for zero reflection of vertical polarization. Outside the tube, two mirrors were mounted, one a flat high reflectance mirror and the other a spherical mirror with slightly less reflectance, so that most of the light came out that end of the laser. I recall that this laser cost $8,000 back in 1962 and emitted about one milliwatt of laser light. At some point, Wendell Blicken left the group and Emmett Leith took his place.

The main activity of the laboratory I had joined was optical processing of data from side-looking, synthetic aperture radar. In the 1950s, an idea had been proposed to solve a problem with airborne radar. Distance resolution was easily achieved by transmitting very short pulses, or frequency chirped pulses that could be compressed upon their return. Lateral resolution could only be improved by using a very large antenna so that the width of the beam of radiation could be made narrow, but an airplane could not carry a large antenna. It was well known that an array of antennas could be used to create the effect of a single large antenna, so, the idea was proposed to record the radar data from individual pulses and process them to synthesize the effect of a large antenna. The oscillator for the pulses was kept running continuously so that the signals from the pulses bore a constant phase relationship to one another. By 1962, this idea had been tested and demonstrated to work very well.

Back then there was only one storage medium that could capture the amount of data required for this synthetic aperture process, and that was photographic film. So, the radar returns were used to modulate a cathode ray tube beam, and this was imaged onto a film strip. The film was advanced continuously so that each return was recorded next to the previous one, and the data was processed optically. It turns out that for coherent light, the field at the back focal plane of a lens is the Fourier transform of the field at the front focal plane. This led to the possibility of doing convolutions and other operations that could convert the patterns on the film into images of the ground scanned by the radar system. Because of their radar orientation, they preferred to hire electrical engineers and train them in optics rather than the other way around. At the time I worked there, this material was classified, but it was eventually declassified and published. [1, 2]

Emmett Leith was a major figure in the evolution of synthetic aperture processing. He was a very shy person back then, quite reticent, and I remember his flannel shirts with a tuft of black chest hair peeking through the collar that made him seem like someone from an Upper Peninsula lumber camp. None-the-less, I soon learned that I could depend upon him to give me the correct answer to any question I might have. A story I once heard, probably apocryphal, was that he had been assigned to analyze the optical processing of the radar signals because it was considered completely understood and would keep him safely occupied. Whether this was true or not, the fact is that he perceived the optical processing of radar data as an imaging process. The images in radar space were converted to images in an optical space where they could be manipulated by lenses, and this revolutionized the design and implementation of optical processors. After having this insight, he discovered the reference to Denis Gabor's hologram process in the classic text, *Principles of Optics* by Max Born and Emil Wolf. Gabor had proposed the hologram as a way convert electron microscopy to optical wavelengths so that problems such as spherical aberration could be more easily dealt with, something that never occurred.

Optical processing of radar data required light that was at least partially coherent, that is, which appeared to come from a single point source and had only one wavelength. Before lasers, this meant focusing a mercury arc lamp through a pin hole and filtering out its single green spectral line. The light levels that could be obtained were very low, and this made processing quite slow. He-Ne gas laser had just been invented, and they promised to provide much more coherent light. Soon, the first of a number of these lasers arrived at our laboratory, and I became the person who kept them properly cleaned and adjusted. I remember we got one from a competing company on demonstration, but its performance was considerably poorer than the SP/PE lasers. The introduction of these lasers into the optical processors was fraught with problems, however. The partially coherent light from the mercury arc lamps was much more forgiving of dirt and reflections from the lenses, whereas the perfectly coherent laser light required everything to be spotlessly clean and perfectly antireflection coated. As a result, there was a strong push to eliminate any optical element that was not absolutely essential. Emmett was quite good at that, and the joke was passed around that someday, he would figure out a way to eliminate the photographic film. Ironically, in this age of digital data storage and processing, that happened.

After recognizing the correspondence between optical processing of radar data and Gabor's holograms, Emmett had one of the engineers, Juris Upatnieks, record holograms using one of the mercury arc lamp sources used for radar processing. They had learned that it was important for the radar data recording to avoid having the antenna point directly to the side of the aircraft. Instead, it was pointed either ahead or behind at a small angle. (Alternatively, the same thing was accomplished by running the local oscillator, against which the return signals were compared, at a slightly different frequency from signal transmitted.) The return from any one object point, if tracked from pulse to pulse, would cycle like a scan through a Fresnel zone plate. Pointing the antenna ahead or behind eliminated the broad region in the center of the zone plate, and it also eliminated the twin image problem that had plagued Gabor's process. For recording an optical hologram, this required having the reference beam at an angle to the object beam, i.e. having an off-axis reference beam. With this configuration, they were able to show hologram reconstructions of letters that were not overlaid with twin images. [3]

Now, however, they had lasers, and these put out light that was very coherent and well suited to recording holograms. I remember that in December of 1962, we had the choice of taking as a day off either Christmas Eve or New Year's Eve, and I chose the latter. Thus it was that I was in the laboratory on Christmas Eve doing some work where Emmett and Juris were also working. At one point, Emmett called me over to look into a microscope eyepiece mounted on an optical rail. I saw only a swirly pattern of red He-Ne laser light, but as Emmett moved a carriage along the rail closer to me, an image appeared of one of our secretaries. Emmett then explained to me what they had done, i.e. recorded the interference between a light beam passing through the transparency and a reference beam and then illuminated that recording with the original reference beam to reconstruct by diffraction the original object beam with its continuous tone image. I was young, naïve, and brash, and said that the result they had obtained was exactly what I would expect. Emmett got very huffy, and said that if I had read about holograms in Born and Wolf I would know what they had just achieved. I went back and read that section and understood his excitement. This was the beginning of the off-axis laser hologram.

Chapter 2. The Problem of the Holograms

The building in which we had our laboratory was on the east side of the airport and was a windowless block house with two floors. What, exactly, it was originally used for I don't remember. The Willow Run Airport had been built in 1941 during World War II by Ford Motor Company for production of military aircraft, and the soil in that location was not well suited for runways. I heard that this had required an extensive substructure that resulted in a really excellent set of runways, which are still there today. Shortly after 1962, Detroit began building the Detroit Metropolitan Airport about halfway between Willow Run and Detroit, and the location of the new airport made the old one unusable for commercial air traffic. I believe that the airplanes used for our synthetic aperture radar used the Willow Run Airport.

The building had, as I recall, a central laboratory area that occupied both floors, and offices and labs extending around it. Most of the ground floor rooms were labs and the upper floor were mainly offices with some more labs as well. There was a central office area where the secretaries sat on the second floor and there was an electronics laboratory off this area. Some of the people I recall were Adam Kozma, Anthony van der Lugt, Norm Massey, Carl Alexoff, Ken Haines, Percy Hildebrand, Fred Rotz, and Al Friesem. Shortly after I started work there, Arthur Funkhouser joined the laboratory and shared my office. He and I commuted to work from Ann Arbor for a while. I remember there being a partial eclipse of the sun in the summer of 1963, and we borrowed some lenses from the laboratory to try to form images of it. He had been working under George Stroke at MIT, and Stroke was supposed to have come to U of Mich. in the fall of 1962, but delayed that until 1963 for some technical reason.

Work progressed on the synthetic aperture radar, and I made a few contributions. One of them was essentially a lensless processor. When the radar signals were chirped, the resulting recordings of single reflecting objects took the form of elliptical Fresnel zone plates. I got the idea that perhaps if the film plane were tilted relative to the illumination beam, the ellipses might act as circles and generate focused images without the need for lenses. Before this, it was axiomatic that the film be placed at right angles to the illuminating beam. My idea worked, but gave good images only for one depth in the field of view, because the eccentricity of the zone plates varied with range. This innovation did give rise to the idea of tilting the film plane, which led to the tilted-plane optical processor. [2] Perkin Elmer Corporation was contracted to design and fabricate a new concept in optical processors using what are called anamorphic telescopic systems. The first unit was called the Precision Optical Processor, POP, and was followed by the Michigan Analyzer, MA.

I attended a meeting of the American Physical Society in 1963, where I happened to attend a presentation by Corning Glass Works of their new pyroceramic materials, including photochromic glass. I contacted the authors and found out that these materials were capable of very high spatial resolution. I got some samples and used them to record holograms. The samples were exposed to ultraviolet light to darken them, and then exposed to the red He-Ne laser light to erase them selectively. I directed two laser beams to intersect in the samples, one with a transparency in it, and when I blocked the beam with the transparency, the other beam was diffracted into a replica of the blocked beam, together with an image of the transparency. Unfortunately, the readout beam would then gradually erase the hologram. I did a fair bit of work with these materials.

As I think back, I don't remember how it was that I maintained contact with Robert Powell, who had been one of my professors back at Lowell Tech. He had left Lowell and was working at an IBM research facility in Owego, NY, and I remember visiting him there while I was working at Bell Aerosystems. Elias Snitzer, who was his friend and another of my professors, had also left Lowell and was working at American Optical in Sturbridge, MA. Eli had obtained fame for publishing the first waveguide solution for circular glass fibers and inventing the Neodymium glass laser. Powell got the idea to solve the analogous problem for the rectangular glass waveguide and launched himself into that. Many years later, I saw a publication of a solution to that problem which seemed to have no difficulties, but somehow Powell encountered some mathematical obstructions that caused him to bog down on this problem. I know Elaine and I had visited him after my move to Michigan, and acquainted him with the concepts, as published, of coherent optical processing. Consequently, when the time came for him to make a change, he applied to our laboratory among other places. In the end he chose us and moved to Ann Arbor in the fall of 1964.

By 1964, the recording of holograms had progressed from continuous tone transparencies to transparencies illuminated by diffuse light, to three-dimensional objects. Around September of that year, Elaine and I decided to take a vacation through Canada to Montreal, Quebec, and down to Massachusetts where my parents lived. Before I left, Emmett told me that when I got back he wanted me to work on the problem of the holograms. Puzzled, I asked him what was the problem of the holograms. He explained that the diffraction efficiency of holograms with diffused light illuminating a transparency was about half what you got for a transparency without the diffuser. The corresponding diffraction efficiency for a hologram of a three-dimensional object was about one tenth of that. They had one hologram, made of a model train, that was far brighter than any of the others, and they were unable to duplicate it. In my memory, that hologram had been recorded during the demonstration of an experimental laser from Spectra Physics that was much more powerful than the 1 mw lasers we were generally using. That experimental laser eventually became the model 125 that was rated for 50 milliwatts, but normally put out about 90.

When I returned, I found Robert Powell and Juris Upatnieks working in one of the downstairs laboratories making holograms. This laboratory had two large granite tables, about a foot and a half thick, on rubber isolation pads. Powell had not brought with him the necessary paper work to get his clearance transferred immediately, so the only project on which he could work was an unclassified holography contract. Bob and I began to work together, and Juris left us to work on his own in one of the upstairs labs. I remember we did a number of things to make our work more effective. One was to buy a set of dark room dishes so that we could develop our holograms right there in the laboratory rather than having to wait for the dark room to become available. We also used small one inch high plates to record test holograms in order to spare the more expensive four by five inch plates. The photographic material was Kodak 649F, which was a spectrographic emulsion with very high resolution but extremely insensitive to light. With the mere one milliwatt of power available, our exposures routinely took over a minute, and this, together with the fact that the tables were not on air suspension vibration isolators and our room was next to the air conditioners, was the real problem of the holograms. We eventually solved this problem, and could make very bright holograms of the model train very consistently. At one point, I found that Emmett was removing our holograms for safe keeping. When I queried him about it, he commented that they were valuable and he didn't want them broken. I said that if I broke one I would just make another. He said he would lock me in the lab until I did, and I replied that I wouldn't miss lunch. At one point after that exchange, I took a morning to record half a dozen or so high quality holograms and gave them to Emmett. It was working to solve this problem that we discovered holographic interferometry. At this point, it is worth making some comments about holography and holographic interferometry.

When holograms of three-dimensional objects were first published in 1964 by Emmett Leith and Juris Upatnieks [4] the technical community responded with enthusiasm. Here was a process that gave truly three-dimensional images, complete with parallax. Many ideas were put forward for applications, 3D television, 3D movies, dazzling displays in store windows, etc. Little of this came to pass, as has been documented in the book, *Holographic Visions* by Sean Johnston. [5] Of the applications that did manifest, holographic interferometry stands out as the one that makes fullest use of a hologram's unique ability to reproduce the optical field reflected or transmitted by an object. Holographic interferometry utilizes two unforeseen capabilities of a hologram. First, it can record fields that are incoherent and reconstruct them coherently so that they can interfere. They may be incoherent because they are generated by different laser wavelengths, they may not have existed at the same time, or they may result from object motion such as vibration. Second, a hologram can reconstruct the optical field from an object with such fidelity that it can interfere with the original field from the object when the hologram is relocated in its original position.

In the section of *Holographic Visions* devoted to it, [6] Sean Johnston does cite the Willow Run Laboratories of the Institute of Science and Technology of the University of Michigan as the place where this discovery was made. From December 24, 1962, when the first laser hologram was made until December of 1964, we had the field pretty much to ourselves. What is disputable is the priority Johnston gives to the discovery of holographic interferometry. He cites Emmett Leith and Juris Upatnieks as the first discoverers and Robert Powell and me as having rediscovered it. Johnston supports this position by email communications from Leith and Upatnieks, both dated June 2003. They read as follows:

Leith: "One of the objects we used was a sheet from a paper calendar, measuring about a few inches on a side. To give stability, we pasted the calendar sheet to a block of aluminum. The resulting holographic image was good except for a circular black spot about a half-inch or less in diameter, which covered up one of the numbers on the calendar. We made another hologram and yet another. It was always the same story – just one defect, and always in the same place. Examination of the object revealed that the aluminum block had a circular hole in it, just at the place where the image defect occurred. Juris remarked that this would be a good method to measure object motion."

The date of this hologram is cited as 19 December 1963.

Upatnieks: "One of the earliest holograms showed a dark spot where a piece of paper, cemented to an aluminum block, traversed a hole in the block. It was obvious that the paper had moved. In a later hologram, cardboard was cemented to a metal block with an edge extending beyond the block. This part showed fringes that are typical of moving or vibrating objects."

The date of the "later hologram" is not given, but a 2014 email communication with J. Upatnieks sets it at about February or March of 1964.

Johnston then goes on to recount the discovery made by Robert Powell and me. Johnston's account is based upon a letter dated 14 Oct. 1976 by Powell to P. Jackson at what was then the Museum of Holography in New York. (This letter is now at the MIT museum with the rest of the MOH collection.) Johnston relates, "They had used hot wax to fasten their holographic subject, another small metal train model, to a metal base. The result had been a reconstructed image obscured by black marks across part of the object. After a couple of days' reflection, they realized that the metal base had cooled during the exposure causing contraction of the metal and yielding visible bands of interference."

My memory of this is quite different. I had settled on using bee's wax as an adhesive for our setups because I was concerned that any glue might have an unknown curing time during which an object might change position during a recording. The wax was available at the U of M chemistry store and formed a good bond with the objects we used it on. Once it cooled, it stayed put exactly, and I recall never seeing degradation of a hologram due to cooling bee's wax. The hologram to which Powell referred was made when we were searching for a holder for the holographic plates. I found a U-shaped frame slightly larger than our 4"x5" plates, and wedged the hologram plate into it against a piece of rubber. This proved to be unstable, and in the reconstruction we saw an ill-defined set of diagonal bands that looked somewhat like fringes. I quickly abandoned that holder and we had a better one made. That hologram was set aside without further consideration at the time. Anything we might have written back then is long gone and I can only substantiate my memory by pointing out that of the two of us, I was the primary experimentalist and did the bulk of equipment manipulation.

What spurred our discovery was when I entered our laboratory and found a colleague, Fred Rotz, using a spectrometer to monitor the laser's coherence. He and others were speculating that the laser might not be as coherent as the manufacturers claimed. I had been told by the Spectra-Physics representative that these lasers emitted about 3 longitudinal modes, and thus their longitudinal coherence would be periodic in twice the cavity length, which was 60 cm. I decided we should demonstrate that, and set up an experiment where the unexpanded object beam was allowed to strike an object set between two mirrors. We positioned our hologram plate so that we could look through it over one mirror into the other and see a sequence of images, each 10 cm further in path length. We recorded several holograms with the zero path length point lying at different points in that sequence of dots, and all of these holograms exhibited a periodic variation in image brightness as a function of path length just as we expected.

The crucial point came when we noticed that in one of these holograms there was what looked like a black fringe in the nearest image of the object beam spot. More important, this fringe moved as we raised or lowered our view through the hologram, so it was clearly not something on the object itself. We immediately set up an experiment wherein we contained the entire object beam on a white surface, and captured the entire reference beam on the hologram plate. This recording gave us fringes that moved depending on what part of the hologram plate we looked through. I checked the laser and found that it was operating in more than one transverse mode. The confocal mirror configuration of these lasers was a spherical output mirror with a flat high reflectance mirror located at its center of curvature. There was a longitudinal adjustment for the flat mirror, and a single TEM_{00} mode was only obtained when the distance was set correctly. We initially had a combination of a 00 mode and a 20 mode and, after adjusting the laser, recorded a hologram with a 00 mode and a 10 mode.

The question was: What gave us these fringes? I suggested to Bob that the photographic plate was recording separate holograms for each mode combination of object and reference beam, and these two recordings were superposed. When the hologram was reconstructed, however, both recordings would reconstruct their respective object beams, but since both fields were now being generated by the same reconstruction beam, the two reconstructed fields could interfere. The shifting of the fringe positions would be due to phase variations between the two transverse mode recordings. Bob found this very disturbing, because it implied that fields that were incoherent during the recording of a hologram were being made coherent in its reconstruction. It was at this point that I referred back to the hologram we had made with the plate wedged against the rubber pad as proof of what I was suggesting. Bob was still unconvinced, and I said something like, "look, if we record a double-exposure hologram and give the object as small rotation between the exposures, wouldn't we expect to see fringes in the reconstruction?" That question spurred immediate action. It took us some time to learn how much rotation to give the object, hologram plate, or reference beam, but we got this to work and showed double-exposure interference patterns in hologram reconstructions. This was a heady experience, and we worked to the wee hours several times because we could not wait to find out what would happen next.

As with Leith and Upatnieks, our object was a sheet of paper cemented to an aluminum plate, and in one of the reconstructions, we noticed a perturbation of the fringes in a region where there was a small bolt hole. In retrospect, I'm sure the paper had statically deformed between the two exposures, but at the time we wondered if the effect could be vibration. Initially, I set up a one-dimensional object, a wire, but that was unsatisfactory, so we set up a 35 mm film can with a solenoid mounted under it. This is what generated the remarkable set of J_0 fringe patterns which we presented at the 1965 spring meeting of the Optical Society of America and published in the December 1965 issue of JOSA.[9] My record books are long gone, but to the best of my memory, we did the coherence function experiments in October 1964, the double-exposure holograms in November 1964, and the vibration holograms in early December 1964.

The point of this is that Robert Powell and I observed a phenomenon, studied it in detail, proposed an explanation, and then tested and confirmed that explanation. By contrast, Leith and Upatnieks appear only to have observed something that could be explained as holographic interferometry once holographic interferometry was conceptualized. I feel that observation of a phenomenon does not count as a discovery. Human beings observed for many centuries that the planets moved relative to the stars, but it was Copernicus who discovered that this motion was best explained by a heliocentric solar system. People observed lightning for all of human history, but Benjamin Franklin discovered that it was an electrical discharge. So the question is: Did Leith and Upatnieks perceive the two properties of holograms that I defined above and thus understand the concept of holographic interferometry? I believe they did not, and evidence for this is found in the patent filed under their names for holographic vibration analysis, US patent number 3548643, which I will discuss later.

As we submitted our paper to the OSA for the spring meeting of the Optical Society of America at Dallas, TX, in April 1965, I proposed to Emmett that we should also present a paper on the entire set of experiments we had done defining holographic interferometry. He vetoed the idea saying that one paper was adequate. This was an unfortunate decision, and it became clear to me in 1970 that the reason for it lay in the patent disclosures he had filed with Juris. We presented this additional material at the OSA annual meeting in Philadelphia in October 1965, and published it in JOSA in September 1966. [7]

After our remarkable holograms of a vibrating object, Bob and I searched for prior work using interferometry for vibration analysis and came up with the paper by Harold Osterberg, [8] which identified the fringes we saw in our reconstructions as a zero-order Bessel function of the first kind. It also suggested to us that it would be useful to have a method for real-time vibration analysis. Just after the spring meeting of the OSA, the idea occurred to me that if we could replace a hologram back in its holder accurately enough, its reconstruction ought to interfere with the field from the original object. We tried this and it worked, giving us a real-time vibration measurement technique, a method of evaluating the quality of a hologram reconstruction, and measurement of object deformation in real-time. We submitted a letter to the editor in JOSA, which appeared in the same issue as our vibration analysis paper. [9, 10]

As it turned out, Kenneth Haines and Percy Hildebrand were also thinking about holography in April 1965. They borrowed our real-time holography setup to record the holograms they used for real-time interferometry showing deformation of a plate due to bolt tightening. At the time, Bob and I thought that it was good to have more people participating in this work, but as the years passed and Haines and Hildebrand were referenced for double-exposure holography and our contributions only for vibration analysis, I realized that this had not been a good idea. I have long regretted that we didn't put a dc current through the solenoid with which we had excited vibrations on our 35 mm film can and showed its static deformation, both in double-exposure and real-time holographic interferometry.

In April 1966, Haines and Hildebrand published a paper [11] that presented the use of holograms for real-time interferometry between an object and its reconstruction as a "new method" solely attributable to them. The prior work by Robert Powell and me [10] is described by the sentence "A related technique for vibration measurement was used by Powell and Stetson." It is very hard to assert that they did not know of the prior work Powell and I had done defining the general concept of holographic interferometry, which we had presented at the OSA meeting in October 1965, and of our prior discovery of real-time interferometry, our setup for which they used for their experiments. The truly original content of this paper was their analysis of the fringes and fringe localization. Unfortunately, both this paper and a nearly identical paper following it [12] were mathematically dense and very hard to use to characterize the fringe localization in holographic interferometry, and accordingly neither has been used for that.

Johnston provides a quotation from Hildebrand regarding the communication of his ideas on holographic interferometry to Emmett Leith in the spring of 1965 as recounted to patent attorneys in 1970 which confirms our priority. Percy didn't realize that I had recorded similar ideas in my notebook two weeks earlier and that Leith "did not mention Karl's entry to me when he signed mine because he didn't want a conflict to arise. However, he foresaw the present problem, which of course doesn't help me now." That problem was the division of the initial U of M patent on holographic interferometry.

Initially there were two patent applications filed by the University of Michigan in which Powell and I were involved; one titled "wavefront reconstruction" serial no. 503,993 filed on October 23, 1965, with six names: Emmett N. Leith, Juris Upatnieks, Bernard P. Hildebrand, Kenneth A. Haines, Karl A. Stetson, and Robert L. Powell – and one titled "method and apparatus for analyzing structures using wavefront reconstruction," serial no. 514,482 filed on December 17, 1965, with four names – Bernard P. Hildebrand, Kenneth A. Haines, Karl A. Stetson, and Robert L. Powell. In 1970, when I was at the National Physical Laboratory in Teddington, UK, I received a communication from an English solicitor representing the United States lawyers who were handling this patent application. I was told that the initial patent application was being broken up into three applications, each based upon the work initially done by each pair of inventors, Leith & Upatnieks, Stetson & Powell, and Haines & Hildebrand. In particular, the invention of holographic vibration analysis was being filed by Leith & Upatnieks, based on the disclosure they filed in April of 1964. I do not have the papers connected with this, but I am quite sure that Powell and I were to be credited with the discovery of real-time interferometry between an object and its hologram reconstruction, and Haines and Hildebrand were to be credited with holographic contouring by varying laser wavelengths. Leith, Upatnieks, Haines, and Hildebrand had all signed the necessary documents, but Robert Powell was raising objections.

To justify crediting holographic vibration analysis to Leith & Upatnieks, they provided me with a copy of their initial disclosure. After carefully reading this document, I pointed out that it contained no reference to any interference within the reconstruction of a hologram, nor it did not mention any fringes within the hologram reconstruction – in particular the zero-order Bessel function fringes that Robert Powell and I were the first to observe and identify. Essentially, their disclosure described a hologram reconstruction that would be analogous to a Chladni pattern, where small particles on a vibrating surface migrate away from vibration antinodes and collect at vibration nodes. It would be bright at the vibration nodes and dark in the antinodes. This was obvious from what we all knew about recording holograms, i.e., that if the object moved during the hologram recording, its reflected light lost coherence and the reconstructed object would appear dark.

Before I would sign their documents, I required that all reference to fringes and all mention of how to measure vibration amplitude within antinode of a vibrating object via its holographic reconstruction be removed from the claims of the patent. This effectively neutered it. The reader may obtain a copy of the final patent, number 3548643, and verify that this was done. There is one mention of fringes in the discussion section, and there are drawings sketching some of the results that Powell and I obtained, but none of this is claimed. In retrospect, I should have insisted on expunging all mention of fringes and any of our work from the entire document, but since the legal teeth of a patent are its claims, I decided to let it go. Powell accepted the modifications I had obtained, and signed the documents also. So far as I know, the application for Powell and me was never awarded a patent, nor do I know it was ever filed; however, a patent for holographic contouring was awarded to Haines and Hildebrand, No. 3,552,858. Whether our patent was abandoned because of the restrictions we placed on the Leith & Upatnieks patent, there is no way to know.

Anyone who has worked with lawyers will attest that had there been any way in which the Leith & Upatnieks disclosure or anything in their notebooks could have been used to argue that they understood that the concepts of holographic interferometry, I could never have imposed the restrictions I did. For this reason, I feel justified in saying that Robert Powell and I discovered holographic interferometry. Juris Upatnieks confirmed this for me in an email communication: “We observed the effect and noted it, but did not get into any detail. Our main interest was image quality (of holograms).”

One possible contender for discovery prior to ours was Melvin H. Horman. [13] He presented a paper at the same OSA meeting in Dallas where we presented our vibration work, and published in Applied Optics in March of 1965, with a received date of October 1964. His paper was essentially theoretical with no experimental work presented, and it proposed that holograms could be used to replace an object in an otherwise conventional Mach-Zehnder interferometer. To perceive that theoretically was impressive, but there is no mention in the paper of the ability of a hologram to generate interference between components in its reconstructed field, nor was a hologram proposed as a beamsplitter between a reference beam and the object beam used in its recording.

An important contribution to the theory of holographic interferometry was made by Adam Kozma not long after our vibration experiments. The question occurred to him – what would a hologram reconstruction look like if the object moved randomly during the hologram exposure? Assuming the motion to be stationary and ergodic, a probability density function could be associated with it. This allows the time integral definition of the fringe function to be replaced with an integral over all the values assumed by the object displacement, and the fringe function becomes the characteristic function of the probability density function. This is why the fringe function in holographic interferometry is usually called a characteristic fringe function. Since the characteristic function is the Fourier transform of the density distribution, it becomes very easy to describe the various fringe functions associated with various object motions. It also emphasizes the view point that the photographic plate records elementary holograms for each object position and then reconstructs them simultaneously.

The year 1965 saw a number of independent discoveries of holographic interferometry. One of the first was by Collier, Doherty, and Pennington, [14] who described interference effects within a hologram reconstruction as a moiré effect. Very shortly afterward, pulsed laser holographic interference was reported by Brooks, Heflinger, and Wuerker. [15] Their discovery was a fortunate accident that occurred while they were recording pulsed laser holograms of a bullet in flight. The laser fired two pulses during one of these recordings, one of which occurred with the bullet in the field of view and one without it. The shock waves in the air were clearly visible as an interference pattern. In England, J. M. Burch published a paper in which he described experiments done at the National Physical Laboratory in Teddington that showed both double-exposure and real-time holographic interferometry. [16] In September of that year, Haines and Hildebrand published a paper proposing that if the wavelength of a laser could be varied, holograms could be used to generate contours of an object surface via interference effects. [17] No experimental demonstration was done because at that time laser wavelengths could not be varied. In the Soviet Union, holographic interferometry was discovered about this same time by Yu I. Ostrovsky. It is clear from these and other publications that year that holographic interferometry was a phenomenon waiting to be discovered by nearly anyone experimenting with recording holograms.

An interesting point concerns the name of this phenomenon. At the time of our discovery, George Stroke was making claims regarding holography and even went so far as to claim authorship of the name holography itself. So far as I know, Gabor only used the word hologram. For that reason Powell and I decided to call our discovery hologram interferometry rather than holographic interferometry, and I used this term for many years. Time has worked in favor of the term holographic interferometry, so that is the term I am using in this work.

Chapter 3. The Search for Applications

After the discovery

The main problem Robert Powell and I faced regarding what to do with holographic interferometry was that neither of us was a mechanical engineer. For example, two of the vibration patterns we recorded of our film can bottom showed a five lobed pattern that was an impossible mode for a circular vibrating structure. In retrospect, it was a combination of a five diameter mode and a two ring circumferential mode and not a mode by itself, but we had no idea what it was at the time. I communicated information about our discovery quite freely, and one of the people I told of our work was Ralph Grant, a professor of acoustics at the University. I also came in contact with Donald Gillespie, who was very interested in finding a way to exploit holograms financially. Don worked in a laser laboratory and was able to make his own lasers. I helped him set up a holography laboratory in his basement in which he made holograms for demonstration purposes. Eventually he and his brother John Gillespie formed the first company to make equipment specifically designed for holography laboratories, Jodon Engineering.

Arthur Funkhouser had left the Radar Laboratories in the fall of 1963 when George Stroke came to U of Mich. I met George Stroke, and obtained a copy of his analysis of diffraction gratings, his doctoral thesis. Sometime, early in 1965, Denis Gabor visited our laboratory at Willow Run, and I remember showing him our work with holographic interferometry. I also remember Emmett stating to us after he had left that he felt Gabor was truly overwhelmed, and well he should have been. Late spring or early summer of that year, our laboratory moved to the North Campus of the University. I remember that I found that the parking arrangement there was particularly annoying. We had to walk a long distance in the summer heat to get from the lot to our laboratory. There were parking spaces in front of the building, but they were metered and reserved for visitors.

Early in 1965, Bob Powell retreated to his office to write out equations describing holography and holographic interferometry. This he did on desk blotters in order to have enough room, and the process was essentially endless. He did point out to me that object displacement and the illumination and observation propagation were all vectors, which simplified the description of how this generated phase change. He also brought to my attention a paper by Gabor that described optical propagation in terms of plane-wave decomposition, that is, a Fourier transform. Both of these ideas became kingpins in my own theoretical work. Powell's attitude toward publishing our work was frustrating. Our original submission to JOSA was returned with numerous requests for alterations, and Bob objected to the simplified theoretical discussion I had written. I eventually had to rewrite it in terms of optical processing language to which he could not object because he didn't understand it sufficiently. I have always felt that had it been left to him, we would still not have the paper published. That said, there is no question in my mind that without Powell's participation, I would never have done the experiments necessary to make our discovery. His perception of the physics of what we were doing was crucial.

By the fall of 1965, Elaine and I were ready to move back east, and I flew to Massachusetts to interview companies, notably GCA Corporation in Bedford, MA, and Block Engineering in Cambridge. This was November 1965, and while I was east, the great northeast blackout occurred. My father was a ham radio enthusiast and had a generator which he fired up to keep the house running. He also got on his ham radio and managed to get a message back to Elaine that I was okay and would be coming home the following day. I remember my interview at Block Engineering, with its founder Myron Block. He was something of a maverick, and Block Engineering had become rather notorious by publishing technical material in JOSA as paid advertisements, thus circumventing the reviewing process. I remember meeting an engineer there who had been a few years ahead of me in Concord High School, Tuckerman (Tuck) Moss. He had played the Sousaphone in the high school band, and took the instrument home with him to practice. Since he lived biking distance from the school, he carried it home wrapped around his shoulders in playing position, much to people's amusement. I got an offer from Block as well as GCA, and I remember Myron cautioning me not to become fixated on the one discovery I had made, but to branch out into other areas. In a way, I both did and didn't. Holography and optics led me into physics, and the applications of holographic interferometry led me into mechanics, but holographic interferometry has always been the center of my technical world. Anyway, I declined the offer from Block in part because of the prospect of commuting into crowded Cambridge, MA, each day.

GCA Corporation

I left U of Michigan at the end of 1965 and worked at GCA Corp. in Bedford, MA, for a little over a year. GCA was not a company in need of the technology that holographic interferometry could provide, and our primary concentration was on finding applications for holography in general. I did interferometry work there, and we sold a system for holographic vibration analysis to Juergen Tonndorf at Columbia University for holographic analysis of ear drum vibrations. I also did interferometric contouring experiments by placing the object in a tank of water and adding salt to increase the index of reflection between two exposures. I also published a work describing how it was possible to use holographic recording to eliminate the effects of fog with a holographic recording. [18]

We also did research into holographic contouring for General Motors using the idea of limited depth of focus in a hologram reconstruction. [19] The idea was to paint the surface of a model with a pattern that could be observed in the real image of a hologram reconstruction. This work never panned out for us, but I did learn that the optical quality of the substrate on which the hologram was recorded was critical to obtaining a high resolution real image from it. The hologram recording is an interference pattern between the object and reference beam and is recorded exactly as these beams enter the photographic emulsion. To reconstruct a real image, however, a beam of light has to propagate through the hologram substrate in the opposite direction to the original reference beam before it gets to the emulsion. If the substrate is not optically perfect, the beam acquires aberrations that will affect the reconstruction resolution.

The primary invention I made at GCA was the total internal reflection (TIR) hologram, which was a way to circumvent the problem of obtaining a high resolution real image from a hologram. [20] Here the reference field entered the back of the plate by means of a prism and was totally reflected from the outer surface of the photographic emulsion.

This allowed the object to be placed very close to the hologram so that only a small region of the hologram was used to reconstruct each object point over which the optical quality of the substrate was essentially perfect. When a TIR hologram was illuminated so as to reconstruct a real image, a large image could be obtained with high resolution over the entire surface of the hologram. Developed for the GCA subsidiary, D. W. Mann Corp., this was intended for integrated circuit mask printing. A master circuit mask would be made by a step and repeat camera, copied into the hologram, and then reconstructed for printing onto substrates without physical contact. This would eliminate degradation of circuit masks due to contact printing. The results I obtained with photographic film were too noisy to use for circuit printing, and the process had to wait about thirty years until a Swiss company, Holtronic Technologies, SA, made it practical by using a photo polymer from Dupont.

While I worked for GCA Corporation, I met Stephen Benton, who was then working for Polaroid Corporation. He went on to become quite well known in the field of display holography and invented, among other things, the “rainbow hologram.” I also arranged to teach a course back at my old college, LTI, on holography and modern optics during that period of time. Another memory I have from that time is of being approached by a fellow who was bent on commercializing holography. He had managed to record some simple holograms on low resolution film that were visible as a diffraction halo when illuminated with a pen light. I recall his visiting me at my house in Acton, MA, to describe his ideas. Unfortunately, nothing he talked about fit together at all. He could not seem to grasp how totally impractical his concepts were, and I finally had to tell him to just go away. I don’t remember his name at all.

GC Optronics

By 1967, I was interested in leaving industry and obtaining a position at a university. I applied to, among other colleges, the University of Massachusetts in Amherst, MA, but as soon as they found out that I did not have a doctorate, the interview was canceled. At this point, I decided to bite the bullet and get the Ph. D. Ralph Grant, back in Ann Arbor, had spoken highly of his experiences getting a doctorate at the University of Delft in Holland. By then, Adam Kozma had gone to England to get a doctorate, as Bud van der Lugt also did a little later. I knew Raoul van Ligten who was at American Optical and had also graduated from Delft, and he recommended to me that I write to Professor Erik Ingelstam at the Royal Institute of Technology in Stockholm (known as KTH for Kunglika Tekniska Högskolan). I did, and was pleased to receive an offer of a position and a salary with which to support myself while working on my doctorate. At this point, however, my position at GCA was terminated. They had become subject to stock speculation because of some of their space surveillance products, and had to trim their budget in order to support the increased price of their stock. My position in Stockholm would begin that fall, and I needed an interim income.

By then, Ralph Grant had teamed up with a lawyer, Joseph Crampton to form Grant-Crafton Optronics, aka GCO. They were fortunate in finding that this new interferometry could make bonding flaws visible on laminated structures such as pneumatic tires when subjected to changes in pressure, and this became a major business for this company. Around 1973 or so, GCO ended and was replaced by Industrial Holographics, Inc., which, in turn, was replaced by Grant Engineering, Inc. around 1986. Approximately 1997, this business was obtained by another company who sold systems under the trade name, L-Ray. Many other groups in the US and around the world started to investigate how this new form of interferometry could be put to practical use. The main areas of application were recording vibration modes, mapping static deformations, and visualizing bonding flaws.

Over the summer of 1967, I worked for GCO, spending alternate weeks in Ann Arbor, and every other trip, Elaine came with me. Bob Powell had left the University of Michigan laboratory and was working at GCO. Another person I remember there was Jim Seydel. Many of the techniques they had developed for holography setups were new and innovative, and we investigated quite a number of measurement problems. One I recall used a sequence of holograms to measure a large scale deformation of a domed structure. I also was asked about using holography for measurement of corrosion or etching. I speculated that if the object were observed at a specular reflection angle, so that only the low spatial frequency components of the surface contributed to the light observed, it might work. I demonstrated that this worked, and was able to continue this work during the next year in Stockholm. We also duplicated the contouring via limited depth of focus work I had done at GCA and improved on the results. Still, this was not adequate for the automotive contouring GM had in mind.

Chapter 4. Europe

Culture Shock

In September of 1967, Elaine and I flew to Stockholm. We had made arrangements with a neighbor of my parents to store our furniture and belongings in a room in his vacant barn. We got some self-study recordings from the library on Swedish, but didn't make much progress with this before we left. The flight was overnight, as always, and we had dinner with some friends before we left, not realizing that there would be dinner served on the flight. We had about six bags with us, and we had to pay a fee to check them at the airport. We landed in England and made the connection for the flight to Stockholm. We were met at the Arlanda airport by Klaus and Edelgard Biedermann who had driven there in their Volkswagen beetle. Somehow, we managed to get all of us and our luggage in their car and drove to the Wenner-Gren Center in Stockholm. This was an apartment complex built by the head of Electrolux for use of visiting researchers, etc. and their families. It was a semicircular structure of apartments in three floors with a tall building called the "pylon" in the center. There was still some landscaping to be done, so the appearance was strange. At the apartment we were met by Gunnel Nordenfelt, the secretary of the Institute for Optical Research where I would be working, and she had the key to our apartment. She had booked us into a small efficiency apartment on the ground floor. We eventually moved to a larger flat on the top floor. We had packed the bulk of our clothes and such items as we felt we needed for the next two years and had them shipped by boat to Stockholm. They arrived in a month or so.

We arrived on Sunday midday, slept till evening, and then went out to get something to eat. Inexperienced as we were, we had not changed money into Swedish kroner, so we had to rely on the restaurant to accept our traveler's check and give us change. The next day we set up a bank account, changed money, and went shopping for groceries, etc. Most Swedes used the post office in place of a bank account, i.e., post giro, but we were unfamiliar with that, and we had seen an ad that promised a free gift to people setting up accounts at the Svenska Handelsbank (the Swedish Merchant's bank). I remember the teller seemed a little puzzled by our request for the free gift, but then gave us a letter opener with SHB on one side of its leather case. I still have that letter opener. We also managed to register with some office to certify that we were eligible to continue paying US taxes for a period of two years in lieu of Swedish taxes. All this took two days, and I remember Prof. Ingelstam being somewhat perturbed that I had not shown up for work yet when I got there on Wednesday; however, my explanation of our need to set up these details concerning our sojourn seemed to be okay.

Aside from trips to Canada, which hardly count, this was our first experience outside of the US. I had studied Spanish for two years in high school and German for a year in college, but had not learned to speak either. Elaine, by contrast had studied Spanish and French in high school and had more Spanish in college, so she was more poly-lingual than I. We signed up with kursverksamhetten, a program set up to teach foreigners Swedish. Our first classes were frustrating, but then we managed to get into a somewhat special class with a family from Poland, and another young Polish woman and which was taught by a Swedish woman named Margarita Wibring. She was quite fluent in English, which our classmates spoke quite well, and was vivacious and enthusiastic. Under her training, we learned to speak Swedish to a degree, but still had trouble understanding the answers we got. It seemed that, if you wanted to know where something was in Stockholm, you could ask a young person, who would almost surely speak English but would probably not know where it was, or ask an older person who would know where it was but probably wouldn't speak English. We gradually learned our way around Stockholm and where the most useful shops were. There was a fish market on the way back to the apartment from the Institute where I would often buy fish for supper. I remember learning from Fru Wibring the expected inquiry from a shop keeper who intended to wait on you, "Vad för det lov at vara?" which is literally, "what would it please to be?" When I commented that that wasn't what they said at the fish market I went to, she asked what they did say. I replied "Vad ska det vara?" literally, "what'll it be?" She seemed to think that was a little rough.

I also remember a story in one of our workbooks, written in what I would expect is the Swedish equivalent of Reader's Digest style called "Hur Vill Tantorna Har Det?" or in translation "What do the Aunts Want?" We had had it explained to us that the formal word for you, ni, had the feeling of a superior addressing an inferior. Thus, at the Institute I was told right off that we all used du, the familiar form of you. For children, how to address an adult stranger was something of a problem. Accordingly, the narrator of the article described his daughter coming home from the park one day in tears. She had asked a woman for the time, and had been scolded that she was being brought up badly. Upon inquiry, the narrator learned that it had been a young woman, and that his daughter had called her tant, which means aunt. So, he explained that she could use ni, which would apply equally well to anyone. The next day, as expected, she again came home in tears with the same complaint. At this point, her older brother took her in hand to return to the park to, as he put it, "wreak bloody vengeance." Together, they accosted a woman to which he said "Höra du tant, kan ni sajer vad tiden är?" literally, "Listen you, (familiar) aunt, can you (formal) say what the time is?" This time, however, it was a mild mannered elderly lady who simply looked at her watch and told them the time. The narrator went on to describe how he realized he was getting older when one day, at an event, a young lady dropped him a curtsey and asked "Can uncle give me the time?" I thought little more about this until one day much later, when I was at the fish market and standing next to a young woman with a little boy, who, as children will, was fidgeting. Suddenly she said sternly to him, "Rör inte farbror!" literally "Don't disturb your father's brother!" I almost laughed out loud.

We got to Sweden near the autumnal equinox, so the days and nights were pretty much the same as back in Massachusetts, but at that latitude the days change much more quickly. Soon it was growing cold and dark, and at each solstice, there is a six week period where the days and nights change very little. In winter, the sun appears about nine in the morning, rolls along the southern horizon, and sinks below it about three in the afternoon. It provides very little warmth, and I remember day after day where the temperature was the same day and night, about zero degrees F. We got a lot of snow that first winter, and it lasted into May. When we left New England, it was promising to be a lovely fall, but now here we were in Sweden. I had about a half hour walk to the University and the Institute, and I would often have to pull the frost off my beard when I got there. There was a park next to the Wenner-Gren Center called Haga Park, and we often took walks there. As the next spring turned into summer, we had the opposite effect. It would get down to a dark purple in the northern sky about quarter to 12, and then grow light again. We had a joke about it – it's time to go to bed, it's getting light out. That first summer we picked quite a few mushrooms in Haga Park as well as both red and yellow raspberries. The second summer we had a severe drought. It rained in May and then didn't rain again until September, and we saw numerous small trees die for lack of water. The northern latitude of Stockholm also has a dramatic effect on sunsets. Because the sun approaches the horizon at a shallow angle, a beautiful sunset can last over an hour.

Institutet för Optisk Forskning

At the Institute, I had arranged with GCO to get some funding to carry on my exploration of corrosion measurement by holography. I had hoped to have that money to help with my expenses, but Professor Ingelstam declared that it had to be paid to the Institute if I was to use their facilities. I did pursue this work, and showed quite promising results with etching of aluminum via sodium hydroxide. Since GCO had filed a patent disclosure on it, I was unable to publish it, although I think no patent was ever awarded on the technique. I had set up a holography table using a marble slab from a gravestone maker and an innerspring mattress for vibration isolation. There were pillars in the basement laboratories that were disconnected from the building, so the isolation was very good. I had a number of components made up and was soon making good quality holograms. Klaus Biedermann, who had been there about six months before me, introduced me to the new photographic emulsions made by AGFA Gevaert, 8E70, 8E75, 10E70, and 10E75. These were much faster than the old Kodak 649F, and the 8E series had about the same resolution. The E70 series had a notch in its spectral sensitivity so that it could be used under a blue-green safe light, and that was very helpful. I used that material to continue my work with the TIR holograms, and with it I managed to achieve much better results, [21] albeit still too noisy for industrial application. I went on to do a detailed analysis of the process and the various parameters associated with it. [22] One of the other things I did, of course, was to proof read the papers written in English by various staff members.

I shared an office with Stefan Johansson, and Klaus Biedermann's office was two doors down. I think Sten Walles had the office between ours, and I remember some of the other researchers, Sven Ragnarsson, Roland Jakobsson, Gunnar Skilberg, Leif Stensland, and a technician/machinist whose name was Jan-Erik Falk. The second year I was there we were joined by Philip Baumeister from the Institute of Optics in Rochester, NY, as a visiting researcher. Everybody spoke English to me, for the most part, especially as my Swedish was practically nonexistent in the beginning. I remember that the professor's office was at the end of the hall, and larger than the rest, and next to it was the secretary's office. Across from that was the library and coffee room. It was the custom to have a coffee break in the morning and again in the afternoon. I didn't drink coffee after the morning, so eventually I procured a teapot and would make tea in the afternoon. There were usually small rusks there, called skorpor, the Swedish equivalent of zweibak or biscotti. At the end of the break, we would all line up and wash out our cups at the small sink, and put them in a rack. If there were anything special, there might be a cake or torta. I remember the middle of the first October, I had been procuring some float glass for mirrors and beamsplitters, and came back midafternoon to find a cake and the crew ready to sing Happy Birthday to me. I was thirty years old that day.

Before I had come to Stockholm, I had been formulating an analytic approach to holographic interferometry and fringe localization. It turned out that Sten Walles was already working on that issue, and we agreed to divide the topic – he would consider double exposure holography and I would consider vibration. This worked out well. I spent the first year there working on the etching technique, total internal reflection holography, and developing my theoretical work on fringe localization, etc. Because my Bachelor's and Master's degrees were in engineering, I was required to read and take exams on two books, one on solid state physics and the other Classical Electricity and Magnetism by W. Panofsky and M. Phillips. I had a course with this latter book in my Master's program, but welcomed the chance to go back to it more thoroughly. The vector calculus in that book was exactly what I needed to have refreshed for the theoretical work I wanted to do for holographic interferometry. At one point during that first winter, Ralph Grant back at GCO asked me to join one of his people, Ed Champagne, in Delft for a technical meeting. Elaine and I flew down there for a few days and got a break from the Swedish winter. I don't remember how we got to the airport and back. I took my oral exams on the two books, and was then assigned all of Principles of Optics by Born and Wolf to learn in lieu of class work. That was a daunting project.

One of the people I met, I think in 1968, was Nils Abramson, who was in the mechanical engineering department of KTH. They were also doing work in holography, and were more involved in its applications. Back in the IST in Michigan, Powell and I had noticed that loci of equal path length between an illumination source and the hologram plate were ellipses, and I had often used a string to find out where in space the object should be located for recording the best hologram. Nils Abramson had also noticed that, but carried it much further, making what he called a holo-diagram – a set of ellipses with the center of the hologram and the illumination source as foci. He had worked out some elaborate procedures for analyzing the fringes based on this construction. I remember Nils inviting me, at one point, to join him in his hobby – sky diving! I declined as politely as I could. When I was in England, I was invited to be the faculty opponent for his doctoral disputation. The second opponent was Dieter Röss from Siemens in Germany. At the same time, I was the second opponent for Klaus Biedermann's disputation.

I found Abramson something of a paradox. His holo-diagram did offer some insight, but it was a two-dimensional view of what was really a three-dimensional situation. His papers on the holo-diagram stressed practicality, but in the late 70s he became enamored with what he called light-in-flight. He recognized that a hologram, recorded with a short coherence laser, would only reconstruct light where the path length of the object beam matched that of the reference beam, and thus it would show contours of equal path length. With the correct geometries, these could depict optical wave fronts. He used this as a way to get into the theory of relativity, etc. Little was gained by this, but from the perspective of the 21st century, this was one of the first observations of a phenomenon that gave rise to what is now called Optical Coherence Tomography. It is too bad that neither he nor any of us saw that at the time.

At some point in early 1968, two holography conferences were organized, the Engineering Uses of Holography in Glasgow, Scotland, and the week after, the Symposium on Applications of Coherent Light in Florence, Italy, 17-20 and 23-26 September respectively. I was invited to a submit paper to the Glasgow conference. Although Prof. Ingelstam was aware of my publications, I don't think he quite appreciated their implications, and the fact that I was an invited speaker at a conference impressed him. It was even more impressive at the conferences when he saw how many researchers were working with what he called "Powell-Stetson fringes." Elaine and I planned a major train trip for these conferences.

We took the train to Amsterdam, stopping overnight in Copenhagen. My sister had been traveling in England and met us for a week in Amsterdam. We had subscribed to the International Herald Tribune in Stockholm to keep abreast of world events, and we had noticed that there was going to be a British comedy group performing in Amsterdam while we were there, so we had written for reservations. They referred to themselves as exporting "British Ham." There were five actors in the troupe, three men and two women, one of whom was a very comely young blond. Their show was a series of skits, and at one point they addressed the steps that were being taken in England to teach foreign languages. One I remember skit in particular started with the ingénue on stage, at which point the younger of the men rushed in saying "je suis!" She said, passionately, "tu es!" He then said, guardedly, "il est?" to which she nodded yes and they embraced exclaiming in unison, "nous sommes!" At this point, another man burst onto the stage saying "vous étes!" at which he drew out a stage pistol and shot them. As they fell to the floor, he said with satisfaction "ils sont!" As they got up and exited the stage, the third man, acting as an announcer said "Tune in tomorrow when we'll bring you avoir and tenir, to have and to hold."

At the end of the week we crossed the channel and took the train to Glasgow. We had booked a room near the University of Strathclyde, and Elaine saw the city while I was at the conference. Robert Powell had left GCO and was working at American Optical, and he had also received invitation to these two conferences and presented a complementary paper to mine. My paper in Glasgow was the first draft at my theory of holographic fringe localization. [23] On Saturday morning after the conference, we got on the train for Florence. I remember we shared a compartment with a Scots woman with a small child bound for London, and she commented toward the end of the trip how much she enjoyed our American accents. This tickled us in view of the fact that we could barely understand her strong Scots speech.

We got across London and caught the train to the ferry across the channel. This was many years before the "Chunnel." We got to Paris the early next morning, took a cab across the city, and got the train for Florence. I remember sharing a compartment with a group of students from Italy who had been on vacation in France. Elaine was able to speak to them to some extent in French, and they complained that "one could not eat well in France." When she asked where one could eat well, they said Parma, which was where they were from. I remember we had to change trains at one point, and that we finally got into Florence very late at night and thoroughly exhausted. We had booked a room at the Pensione Merlini, which was near the Palazzo dei Congressi where conference was held, and I made it to the late morning sessions on Monday. Powell was there with his wife, Betty, with whom we had socialized in Glasgow. In Florence, we were joined by Mike and Josie Wall. Mike worked at Aldermaston Weapons Research Establishment, and who had been very active at the Glasgow conference. We all "ate well" in Florence. I met a couple of researchers at the conference who were working at an RCA research laboratory in Zurich, and they invited me to visit them on our way back north.

We had planned to stay on in Florence for a week after the conference, but Elaine got sick, and we stayed a little longer as a result. I remember getting to Zurich and having to buy something, I don't remember what, the next morning. The woman in the shop spoke no English – French, German, Italian – yes, but not English and certainly not Swedish. We'd picked up a little Italian, but not enough. It was frustrating to hear her switch smoothly from one language to another, and still not be able to communicate with her. What Swedish I knew seemed to push what little I might know of any other language out of my head. The visit to RCA was very interesting. They were developing a holographic system for recording TV programs for commercial sale. Video tape recording, which eventually put this out of business, was a number of years off. We got back on the train and travelled back to Sweden, taking the Puttgarden Ferry at lunch time and enjoying their smörgåsbord.

Back in Stockholm, I remember a conference organized there that fall, I think on high speed photography, and one speaker talked about holography and described its possibilities for vibration analysis. In the question and answer session, someone asked if it were possible to use holography to measure vibrations on string instruments. I noted the man, and approached him after the session to introduce myself. He was Carl-Hugo Ågren, and he made and played viols as a hobby. He worked in photography, I think. Anyway, I proposed that he make up a treble viol top plate and a mount to support it, and bring it to our laboratory. I didn't think much about for the next month or so, until one day he telephoned me that he had the plate ready to bring over. He brought it the next morning and we tried to make holograms of it with no success. It turned out that it was readjusting from the cold, because we went to lunch and returned after that to find we could now make holograms of it with no trouble. I had made up a double plate holder, invented I think back at GCO, which held two plates, one in front of the other. We would record a hologram for real-time holography in the slot away from the object, and this would allow us to observe vibration patterns and record them by placing a plate in the front slot without disturbing the real-time hologram.

Professor Ingelstam was in France for a six month sabbatical when we got the viol plate, and thus we were able to get impressive looking results so that by the time he got back, we had been invited to present a paper at the Acoustical Society of America. We were also visited by John Huber from Martin Guitars, and he encouraged us to do some work on guitars. Somehow Nils got in contact with a guitar maker in Stockholm, named Bolin, and he provided us with a guitar top plate mounted on a stand. We recorded modes of the top plate and also of a back plate. With the need to record large numbers of holograms for vibration modes, we started recording them on 35mm film strips. This required forming an image of the object near the hologram so that deformation of the film would not aberrate the image.

I took my first examination on Born and Wolf, and did not pass it. In August 1968, I was joined by Nils-Erik Molin, who was assigned to work with me in holography. I had him make some display holograms to get the hang of it, and these became very useful for visitors. His prowess in the laboratory soon became very evident, and I set him to work on a set of experiments to verify aspects of the theoretical work I was developing. One of the first was to show the effects of how vibration modes combined in vibration fringe patterns. He fabricated a clamped circular disk composed of a thin plate of glass cemented to a hollow metal cylinder, and he balanced it with bits of wax until the two first diametral modes had the same frequency. With two drivers from a stereo amplifier, he was able to excite one mode, the other mode, or both, and vary the phase of the drive signals so that the excitations were in-phase, out-of-phase, or in quadrature. This showed that modes could combine to create patterns that might look like modes, but were not. It also showed that when modes combined in quadrature, they would create traveling waves that would generate nodal spots, and all of this was predicted by the theory I was developing. My work also predicted that when two vibrations occurred that were at irrational frequencies, the resulting fringe pattern would be the product of the two J_0 fringe patterns associated with each of the vibrations. He recorded a beautiful hologram of a two diameter mode and a mode with one circumferential ring. I eventually had an enlarged print made and framed it for my office wall. We published these results as our first collaborative effort. [24]

After this, I had him fabricate some set ups for studying fringe localization. One of these was a hollow cylinder mounted by springs to a solid cylinder. This allowed us vibrate this surface albeit at a low frequency, somewhere around 60 Hertz or so. Looking at this surface straight on, we could observe the effects of in-plane vibration or static deformation. As predicted, we got fringes that localized along a line inclined to the surface, both for static or vibratory movement and we published the results. [25] Eventually, he mounted a fixture on the surface that held a plate on a torsional spring attached to its center. With this, it was possible to generate fringes with different localization planes, one due to the rotation of the base mount and one due to rotation of the plate itself. Much of this work was done after I left Stockholm, and published while I was in England. [26]

One other publication I made was with Klaus Biedermann on the influence of development time on hologram characteristics. [27] Klaus, Nils-Erik, and I formed a close friendship and usually ate lunch together. We felt that we made a very effective team.

Klaus's main project was the development of a setup for evaluating holographic emulsions used for holography. This involved constructing an interferometer for generating high frequency interference patterns to expose the film and then to illuminate the film after it had been exposed and developed. He was very successful in this and discovered some important results. He and Nils also did a study and published a paper on in-situ development of photographic plates for real time holographic interferometry.

After the conferences, I set about to complete my work on fringe theory in holographic interferometry, and published a paper on what I referred to as a rigorous theory. [28] This set up the conditions for fringe localization and fringe observation in terms of derivatives of the characteristic fringe function and its argument. I felt this was sufficient for a paper and left the solution of the conditions imposed on the argument of the fringe function for a second publication. I remember one weekend deciding to work out the equations for that and getting hopelessly mired down. Nothing seemed to be working out right. Finally, on Sunday evening, I went back over the equations and found that I had assumed a plus sign where it should have been minus, and, once I changed that, everything worked out beautifully, and I published these results when I was in England. [29]

As 1969 progressed, Elaine expressed a strong desire to take a diploma course on art history in England that was being held at the Victoria and Albert Museum. I applied for a visiting research position at the National Physical Laboratory, which was located in Teddington. We could live between Teddington and London and commute in opposite directions. We eventually settled on Richmond, which was on the train line from Teddington to London. I interviewed at NPL and was offered a research associate position that provided a reasonable salary. We also located and arranged for an apartment which would be available just before we got there.

I remember that summer Klaus and Edelgard inviting Elaine and me out to their stuga (cabin) on the island where they were taking a vacation. They wanted to do an American style cookout and asked what was required. I said mainly what they needed was some hard wood, and to confirm what they had he brought in a piece, which I found on my desk. It was a small piece of white birch with the bark still on it. I took a marking pen and wrote on the bark, "How much wood could a woodchuck chuck if a woodchuck could chuck wood?" This, of course, precipitated a long conversation about what a woodchuck was, and my Webster's dictionary described it as a type of marmot. They kept that piece of wood for years after that, and it was very funny to hear them chant that question in unison.

Our dictionary was a very useful source of information. For example, not long after we got there, we wanted to get some buckwheat for pancakes. We had a little hand cranked coffee mill for grinding the groats, but didn't know what they were called in Swedish. Our Swedish dictionary was no help, and none of my colleagues knew what buckwheat was, so they couldn't tell me what to call it in Swedish. At one point, I looked buckwheat up in Webster's and found from the etymology that it was from the Dutch, boekweit, literally book wheat or beech wheat, based on the resemblance of the buckwheat seed to a beechnut. I knew the Swedish word for wheat was vete, and that the word for book was bok, which was the same as the word for the beech tree. Based on that, I walked into a health food store and asked for bokvete. The clerk asked me how much I wanted? I then told this to my office mate, and he finally knew what I was talking about, and pointed out that it was bovete.

As the summer drew on, I was preparing for my disputation and for our move to England. Elaine's course started about two weeks before my disputation was scheduled, so she left before the event. Traditionally, a doctoral disputation was done in formal attire, and I had rented a tuxedo for the occasion, which required a deposit of one third the cost up front. After they got it altered to fit me properly, Professor Ingelstam suggested that we do it in informal dress. I complained to Klaus that my deposit was now wasted, and he pointed out that the amount I was spending had been reduced by two thirds. My faculty opponent was Roland Jakobsson, and one of Nils Abramson's colleagues agreed to be my second opponent. It was also expected that the doctoral candidate would host a dinner for those involved. After discussing this pro and con with Klaus and Nils-Erik, I decided to have a luncheon at the University cafeteria and invite the entire department. This seemed to work out well. The disputation went okay, and I was now Dr. Stetson. I'm pretty sure that Klaus and Edelgard took me to the airport a few days later, and I flew to England.

We had gotten a large walk-in crate which we were able to store in a garage at ground level below our apartment, and I was able to get boxes and pack everything we had brought to Sweden and acquired there in it. I remember an event that happened shortly before I left. I had arranged with a shipping company to pick up our crate, and I had told them that they were going to need two men to do this properly. One morning, after Elaine had left, I was trying to get some extra sleep when I heard noises outside that I identified as the shipping company coming to pick up the crate. I rolled over to try to drift off but eventually heard a crash. I got up, got dressed, and came down to see a single man with a truck equipped with a crane. He had tried to pull the crate out of the garage, but it had tilted when it got to the short ramp to the road and the back of it had broken the overhead door. I asked him in Swedish, "What happened?" and he replied "Allt gick til helveta!" Everything went to hell! I helped him by guiding the lift on the front end and the pull to slide it out. Once it was out, he had no problem getting it on the truck. I then went to the manager of the center and arranged to pay for the repair of the broken door. I remember her saying, "I'm glad you didn't just leave without paying for the damage."

England

Our apartment in Richmond, Surrey, in England was two flights up a rather curved stairway. We had bought a queen-size bed in Sweden to use in place of the twin beds that came with the apartment, and the box spring had a wooden frame that wouldn't allow it to go up this stairway. Fortunately, the window to our bedroom was quite tall, and after I took it apart, it was possible to hoist the box spring up and get it through the window. It went out the same way two years later when we left. The apartment was three main rooms, a living room with the bathroom off of it, a bedroom, and a kitchen. Heating consisted of electric heaters in the bedroom, living room, and bath. The stove had a gas range and the hot water was heated on demand by a gas burner. When we moved there, the currency was still on the pound/shilling/pence system, but after about a year they switched to the pound and new pence. Richmond was (and I presume still is) on the approach route to Heathrow Airport, so we had the periodic drone of airplanes, especially in the morning as the trans-Atlantic flights arrived. There were a nice range of shops, including some very good bakeries. In Sweden, we bought rye breads almost exclusively, as we didn't care for their wheat breads. In England, we almost always bought wheat breads, and the rye breads were unappealing. In particular, we liked the granary loaf with malted grain in it. There was also a very active theater in Richmond, and we bought season tickets. Kew Gardens were a short distance, required a penny admission fee, and was a lovely place to walk. There were a good collection of pubs, and a nice walk along the Thames River. Elaine had been taking the underground to her course at the V&A for a couple of weeks, and I started taking the train out to Teddington to the NPL.

Shortly after I arrived in England, I was invited to visit the research center of Brown-Boveri in Baden, Switzerland, by F. von Willisen. I accepted and flew over, arriving in the evening. I remember having a really tasty white wine in the hotel restaurant, so good that I ordered a second half carafe. Having a liter of wine was not a good idea, and I slept very badly that night. The next morning, at the laboratories, I learned that the real reason for inviting me there was to solicit me for a position in the new holography laboratory they were forming. This was impossible, because I had accepted a two year fellowship to work at NPL, but they wanted to show me around anyway. About mid-morning, the two researchers from the RCA laboratories in Zurich joined us, and they flashed a hologram recorded in dichromated gelatin. This completely absorbed my interest, and we spent quite a bit of time examining it and its characteristics, much to the dismay of my host. I met there François Mottier, whom I had met at that high speed photography conference in Stockholm, but had forgotten. Toward the end of my stay in England, Elaine and I visited Baden together to explore the possibility of moving there, but the salary offered seemed very low. I remember having dinner together with François Mottier and his wife Ann.

At NPL, I shared an office on the second floor with Tony Ennos, and Jim Burch, who was head of the group, had an office at the end of the hall. I remember that John Gates had an office on the same floor, I think next to Jim's. The secretary was downstairs next to the common room where there was a coffee/tea break in the mornings and afternoons. Eddie Archbold was a senior technician, and there was a young woman named Patricia Taylor who was also a technician, and eventually became my assistant. There were several others in the group, including Ian Ross, but I can't recall the names of the rest. I recall there was a very nice enclosed garden next to the director's house called the "director's garden," and we were allowed to walk through it during lunch breaks. I recall an area at one end where all the leaves raked from under the trees and shrubs were piled. This formed a natural compost area where a lot of edible mushrooms grew. During the first year, I would often walk down to the market area in Teddington to buy groceries. It was there I first encountered romano beans, broad, long pole beans that looked for all in the world that they had become far too mature to eat. "Only the English!" I thought to myself, until, one day where there was really nothing else, I decided that maybe they were something I really didn't know about. They were excellent! I also recall what were called "baby marrows," a type of zucchini squash that were nearly two feet long. Despite their size, these too were very edible.

When I was interviewing at NPL, I also took in part of a conference in which I remember John Leendertz from Loughborough University presenting a paper on the use of speckles for displacement measurement, work which was published the following year. [30] I had also seen the publication by Archbold, Burch, Ennos, and Taylor [31][30] in *Nature* on an instrument they had conceived for real-time vibration observation. The idea was to observe the object through an optical system with an aperture small enough that you would observe its surface speckles. A smooth reference beam was combined with that image so that you observed the interference of the speckled object and the smooth reference beam, which was also a speckle pattern. When the object vibrated, the speckles would become blurred in the antinode regions and remain along the vibration nodes. It was usually referred to as a speckle interferometer.

When I saw this publication, I was impressed with the potential for this instrument. It would allow real-time vibration pattern observation without the need for recording a hologram of the stationary object. Real-time holograms were a problem because if the object moved even a small amount, the fringe pattern was lost and a new hologram was required. With a speckle interferometer, this would not be a problem, because only the speckle pattern would be changed by an object movement, and the pattern of speckle visibility would not change. This instrument was the first item on which I worked at NPL. The optical configuration that they had built up didn't work very well, mainly because of too many artifacts caused by the optical elements in the reference beam. I took an "Emmett Leith" approach to redesigning this instrument and eliminated any optical surface in the reference beam that was not necessary. The result was a much more practical instrument, [32] that was about as fatiguing to use as a microscope.

I was in contact with Carlene Hutchins with the Catgut Acoustical Society, a group devoted to technical research on string instruments, and she made available to me a set of violin plates for holographic analysis. She was interested in the modes of these plates when they were freely suspended, analogous to an instrument maker's "tap tone" test. A plate would be held, typically, between the thumb and forefinger about two inches or so from an upper corner and tapped in the center. The plate would be thinned accordingly to generate a recognizable tap tone. We set up a vertical steel plate with sponge rubber holders on magnets that could support the violin plates at their edges. Our supports could be moved to coincide with the observed vibration nodes so that their influence would be minimized. This arrangement would have been impossible for real-time holography, but it worked very well with the speckle interferometer. Once vibration modes were identified, they were recorded as time-average holograms. We set up, therefore, a dual system combining a speckle interferometer with a time-average hologram recording system on 35 mm film using a material made by Ilford. This work resulted in two presentations at meetings of the Acoustical Society of America [33, 34] and one invited paper. [35] In particular, we determined that the characteristic "tap tone" was actually a mode with a node line that was a distorted ellipse. Other experimental work that I remember includes a study of a cylindrical acoustical transducer, and a study of a hi-fi tweeter.

The speckle interferometer became the instrument that John Butters and John Leendertz at Loughborough University developed into the first real-time holography system. The idea was to detect the image with a TV camera and use a high-pass filter on the electronic signal to display only those parts of the image that had spatial modulation. This idea was also picked up by Ole Løkberg in Trondhiem, Norway. The initial results were not spectacular, but easier to look at than the optical instrument. One problem was reflections from the glass surface of the image orthicon or vidicon camera tube, and the standard solution was to cement a wedge to it with a high quality antireflection coating. As time progressed, solid-state cameras and frame grabbers became available, and the results of these systems improved. While I was at NPL, we used only the visual speckle interferometer that I had built.

During the two years I spent at NPL, I continued my analysis of holographic interferometry. I wrote a sequence of five papers all concerned mainly with vibration fringes, the effects of beam modulation, for both fringe loci and their localization, [36] the analysis of the effects of nonsinusoidal vibrations, [37] the effects of simple nonlinear vibrations, [38] the effects of pulse width in stroboscopic holography [39] and the use of the method of stationary phase [40] Nils-Erik Molin continued our work on experimental demonstrations of fringe formation and localization, first with a demonstration of involving pivot motion, in-plane rotation and in-plane translation [41] and then for independent and dependent rotations around orthogonal, non-intersecting axes. [42] For this latter paper, he traveled to Teddington with the holograms he had made so that we could reconstruct them and record the localization data together. On one day, we took a trip out to Reading to visit Harold H. Hopkins, a well-known professor in optics, and we also visited Mike and Josie Wall who lived in that city. I remember introducing Nils to English pubs, which he enjoyed. In particular, I recall having dinner at a pub and being served large baked russet potatoes. We never had much success baking potatoes in Sweden because they were more suited to boiling. When he saw the size of these potatoes, he remarked "In Sweden, we would consider these as having gone by." I helped proof read his papers and papers that Klaus wrote while I was in England.

In 1970, I was invited to be the faculty opponent at the doctorate examination of Nils Abramson, and this was coordinated with the doctoral examination of Klaus Biedermann for whom I was the second opponent. Dieter Röss, from Siemens, was Klaus's faculty opponent and the second opponent for Nils Abramson. I remember being invited to a restaurant before the disputations as guest of Nils and sitting next to a dentist for whom he had done some measurements on a unique dental bridge design he had developed. One thing that I remember strongly was this fellow going on at great length about the dropping of the atomic bombs on Japan by the US. How he dragged that into the conversation I can't remember, but anyway he was adamant about the fact that Japan was making efforts at issuing a surrender before these incidents. I didn't have much to say to him about this, but it stuck in my mind. Years later, I learned more about these events. Yes, in 1945, there was no question that Japan had lost the war, but the issue was the terms of surrender. The allies, and the US in particular, insisted on unconditional surrender, and that was not acceptable to the Japanese military leaders. As I understand it, they were using the specter of a land invasion as leverage to impose conditions on their surrender. That was what required something catastrophic like the atomic bomb to change their perception. I was quite familiar with dental bridges having had a molar removed when I was about 20 and having it replaced with a bridge. That bridge eventually failed and was replaced with a second one. Eventually that one failed when I was in my 60s and it was replaced by two dental implants. Bridges, at that point, had become obsolete.

Both disputations went off successfully. Klaus surprised me, before his, by asking me to tell him what questions I would be asking so that he could prepare his replies. I had not considered doing that when I did my disputation as it had never occurred to me. I told him my questions, but I carefully withheld one so that he would have to respond extemporaneously at least once. As I remember, there was a dinner afterward at Stallmestergården. They had also arranged for me to teach a short course on holographic interferometry while I was there.

I was also involved with a project brought to us by Norman MacLeod at the University of Edinburgh. He was doing work in the field of heat transfer by means of what is called mass transfer analogy. The equations describing the sublimation of a solid into air are essentially the same as those for heat flow, so it is possible to model heat flow from a structure by making a model of it in a volatile material such as naphtha and measure its shape change. He had developed a version of this where he coated the structure with a layer of a polymer which could absorb a volatile liquid. The layer would swell with absorption and shrink as the liquid evaporated out. To get a fine measurement of this, he wanted to use holographic interferometry, and he sent his student down to NPL with some samples for us to test. Having done work measuring corrosion holographically, I felt we could possibly do this if we arranged to observe the opaque layer at a specular angle. We worked on this and got some results, although I don't remember exactly how useful they were.

Professor MacLeod invited me up to Edinburgh to give a lecture, and we were at his apartment for dinner talking about various things, and I happened to mention regarding the mass transfer work, "It's too bad you can't make the layer transparent." He said something like "Would that help?" I explained that the surface we would be looking at would be that of the metal under the transparent layer and the shrinking of the polymer layer would just change the optical path length of the light reflected rather than change its speckle pattern. His response was "We can easily make the layer transparent." I got a letter from him some time later describing the excellent results they had obtained with this technique.

There were standard morning and afternoon breaks for coffee and tea. The coffee they made was, by my standards, extremely weak and undrinkable; however, I didn't drink coffee after breakfast anyway. The tea, on the other hand, was stronger than espresso, so far as I was concerned, and they drank both with copious amounts of milk and sugar. I would drink the tea in the afternoon, diluted about half and half with hot water. There was no air conditioning, so in the summer it occasionally got rather warm, and the prospect of hot tea did not appeal to me. Since it was so strong, it was a simple matter for me to put some ice cubes in a glass and pour the strong tea over them to make iced tea. With a little sugar to sweeten it slightly, it was quite refreshing. Needless to say, this was viewed as extremely strange by everyone there. One of the technicians, who had a definitely cockney accent, said one day, "I tried that iced tea you make last night. It was the worst thing I ever tasted." Everyone laughed, and I told him I was sorry it worked out so badly. Still, I was impressed that he had the initiative to try something so strange. I could have described the pitfall he might have run into in that when you ice tea that is too strong, it turns cloudy and tastes terrible. I also could have mentioned that, having grown up on tea flavored milk, he might be totally unaware of what tea actually tastes like.

I remember getting into arguments with my British colleagues over the terminology used in holographic interferometry. What we called real-time interferometry they called live interferometry, and what we called double-exposure holograms, they called frozen fringe holograms. I spent some time with my trusty Webster's and came up with the term concomitant to replace the terms real-time and live fringes. I used it for quite a few years, but I can't say that it ever caught on. It was one of those arguments where their attitude was, "you may have invented the technique, but we invented the language!" I remember one day during afternoon break, one of the technicians leafing through the copy of Fowler's English Usage, and coming to the section on American/British usage variations, and he started asking me "what do you say for ____?" By then I was quite familiar with British terms, and I was able to supply each of the American terms on cue. Since I never used these terms when speaking with them, he was quite entertained. The second year I was there, Jim Burch spent a year at the Institute of Optics in Rochester. When he returned, he had lost a lot of weight, and I remember him greeting me with "Hi," I word I never heard in England. We were also joined that year by Michael Herscher from Rochester.

I know that while I was in England, I made two trips to the US, at least one with Elaine. I remember giving a paper at an ASA meeting, I think at Atlantic City, NJ. I also interviewed at Worcester Polytechnic Institute's Mechanical Engineering Department with Prof. Zweep, on one of these trips, and I remember giving a number of lectures for honoraria to defray the costs. The problem was that this was 1971, a period during which many colleges were having trouble with student unrest. I had gotten my "union card," i.e., my Ph.D., but the shop was closed. At one point during my second year at NPL, I received an inquiry from a Dr. Robert Gardon at Ford Motor Company Scientific Research Laboratory, in Dearborn, MI. A position was becoming available in their glass research department for the head of an optics group, replacing Max Irland, and I also interviewed there on one of these trips. Elaine was negative about going back to Michigan as was I, but as things worked out, the position at Ford was by far the best paying offer I had, almost by a factor of two. Her course work progressed well during our first year there, and as she prepared to take her final exams at the end of it in May 1970, she got a phone call from her mother in New Jersey that her father was in the hospital with a severe heart attack. She could not just drop everything and fly back, as she had invested an entire year in this and had to take the exams to get any benefit from it. She took her exams and passed, but her father died without seeing her. The result of this was that her sister inherited her father's business, a restaurant & bar in New Jersey. As I recall, at this point her mother and father had divorced, so everything went to her sister. These events pushed us toward accepting the best paying offer I had for employment. Ironically, the inheritance ended up being more of a burden to her sister and brother-in-law than a benefit, but Elaine always felt that she had been left out by what happened.

While we were in England, Elaine often expressed a desire to continue living there. Most of her encounters there were to her liking, and she seemed to feel comfortable with life there. My point of view differed, however, as I did not feel well accepted by my colleagues. Yes, I had my talents, and I had a lot to offer, but I was a "Yank" and not "one of them." They were generally polite, but reserved. I did discuss the possibility of possibly joining the staff at the University of Loughborough where John Butters was located. At one point, we made a trip up there, and I gave a lecture. I remember, after my talk, a young woman, faculty or student I don't remember, showing me an Appellation dulcimer she had made. It was a beautifully crafted piece of work and played well. My sister had made one years before, so I was somewhat familiar with it. I know that I met the department head and talked about possibly joining them, but I never got much of a response from him. I called him after we had made our decision to return to the US to let him know that I was no longer interested in a position there. I remember the cordiality of his response to wish me well - they were the first cordial words I ever received from him. Later, when I saw John Butters and told him of our plans, he expressed disappointment and asked why I hadn't called him about it. I told him of my interactions with his department head, which clearly indicated he was not interested in having me there.

At one point, we took a vacation trip to the west of England and to Wales, and this was my first experience driving on the left side of the road. We had visited Mike and Josie Wall, and I picked up the rental car in the middle of Reading, and from there I drove out to the university to visit Prof. Hopkins and Bud vander Lugt, who was doing his doctoral work there. It took me about a half hour to drive what should have taken about ten minutes. After we got out of the city, it became easier. Being very interested in English antiques from her studies, Elaine bought a fair bit of furniture which we eventually brought to the US. Somehow, we managed to store it in our small apartment. Ford paid for our move to Dearborn, MI, and we hired a moving company to pack up our belongings and ship them.

I was visited at one point by Carl-Hugo Ågren with whom I traveled down to Hazelmere in Surrey to visit the Dolmetch factory, which made replicas of early instruments such as recorders and viols. At that point it was run by Carl Dolmetch, the son of Arnold Dolmetch, who had been the major figure in the renaissance of early music in England. They made plastic recorders, which were quite inexpensive, and Carl-Hugo picked one up and played it quite nicely. I decided to buy one and learn how to play it, which was not hard for me as I had played clarinet for many years. I bought some music and practiced it during my lunch hours at NPL. This was overheard be some others at the laboratory, and by the time I left we had a trio which played during lunch break. I made a number of other trips to the Dolmetch factory, once accompanied by Carleen Hutchins, who was bringing me some violin plates for analysis.

My last week at NPL, I somehow caught my foot on one of the steps coming down the stairs from the cross-over at the railroad station. I lost my balance, fell, and hit my head, but managed to get to a bench. I think I lost consciousness for a short while, and when I came to, a woman clerk said "You got your color back." They took me to a hospital, where I was examined and diagnosed as having had a mild concussion. I gave them Jim Burch's number to call, and he picked me up and dropped me off at my apartment.

They had planned to have some sherry that afternoon to celebrate my last week, and offered me the bottle to take with me, but I declined owing to the problems it would cause coming into the US. I don't remember where we stayed our last night in England, because everything in our apartment had been shipped, but the next day we were on our way home.

Chapter 5. Back in the USA

Ford Scientific Research Laboratory

In September of 1971, we flew to Boston and stayed with my parents in Concord, MA, while we arranged for movers to pick up our stored items to ship to Michigan. From there, we flew to Michigan and stayed at the Dearborn Inn in Dearborn while we looked for an apartment. I had made arrangement with Ford to lease a car, a Ford Pinto, which we used to search for a place to live. We found a house for rent in Dearborn and moved in as soon as our goods arrived from Massachusetts. We discovered that the kitchen had no outlet for an electric stove, and the technician in my group was kind enough to come over and do the necessary wiring. We quickly discovered that Dearborn was not a good location to have chosen to live, because when the wind blew from the south, the pollution from the Rouge River Plant drifted north with obnoxious fumes. We bought an electrostatic precipitator with an activated charcoal filter, which helped a little. Elaine made some contacts at the Henry Ford Museum, which later proved useful. Eventually, our goods arrived from England, and we discovered that somehow things had gotten damp everything was covered with mold. We filed a claim with the moving company and were compensated. There were quite a few prints we had acquired and these all needed conservation work.

The optics group I headed was composed initially of C. Lee Giles, and a technician, Richard Guarino. While I was there, we hired two engineers, Joel Levitt and Jon Sollid from American Optical and General Dynamics respectively. We managed to set up two holography tables and do some meaningful work. One project I remember involved vibration analysis of a turbine disk for an experimental auto engine. Lee Giles spent most of the time I was there on a project to measure the smoothness of solidified paint surfaces. A technique had been developed to apply paint to a surface much in the same way toner is used in a copying machine and solidify it by heat. The smoothness measurements were to be done by optical Fourier transform processing. There was also a project to consider holographic lenses for tail lights. One of the problems with tail lights was called "propellers." These were streaks of light that radiated from the center of the Fresnel reflectors used to direct the light from the bulb. I determined that the reflection came from the rounded edges of the reflector elements, but I don't think that was ever pursued. Another issue I recall addressing was how to count sheets of glass stacked on carts at the glass factory. We made some display holograms for general public relations work. The only two papers published while I was at Ford were on work done previously – a paper with

Carl-Hugo Ågren on our holographic work with the treble viol, [43] and a paper with Pauline Taylor on using vibration mode data to predict static deflections of an object. [44] Joel Levitt and I did a project illustrating a method for measuring the phase of vibrations with time average holograms, [45] which was published in 1976.

I found the manager of the glass research department in which we worked, Robert Gardon, a difficult person. His thinking was very concerned with company politics, and he expected me to fit into this. He suggested that I should make it a point to report to him on a daily basis, a suggestion I felt was insulting and which I ignored. I felt I was hired there to do a job, i.e., set up a holography laboratory, and the most important thing was that I did my job, not whether I did it in the way he thought was most politically correct. Needless to say, this did not sit well with him, and in a few months it became clear that I needed to leave. I was able to attend to the fall 1972 meeting of Optical Society and heard a paper presented by a researcher at the United Aircraft Research Laboratories on a way to obtain strains from holographic data. I noticed an error in that presentation and corresponded with the author. This led to an interview and a job offer in East Hartford, CT. In February 1973, I left Ford and moved to Connecticut. Jon Sollid took over the group for several years, and then left for Los Alamos National Laboratory, after which the group was headed by Gordon Brown, formerly of GC Optronics.

As I look back upon my time at Ford, one of the things I feel best about was helping Lee Giles. Gardon's micromanagement style had taken a heavy toll on him, and when I took over the group, he was very depressed and his sense of self-worth was extremely low. Basically, I befriended him, and held something of an umbrella over him to shield him from Gardon so that he could actually do some meaningful work. He remained there for a period of time after I left, but eventually left himself. I remember meeting him at a college in upstate New York, where he was teaching. I later ran into him at a convention in Hawaii. His positions and accomplishments continued to grow, and now he is a professor at Pennsylvania State University.

United Aircraft/United Technologies

The move to United Aircraft was difficult in a way. At Ford, I had been head of a group whereas at UA I was merely a member of one, and I didn't even have my own laboratory or any equipment with which to work. Robert Erf, my supervisor, asked me to write a chapter on holographic vibration analysis [46] for a book he was editing on holographic nondestructive testing. This took up a fair amount of my time in the first month or two on the job. One of the engineers did some experiments for me on using speckle photography for in-plane deformation measurement [47] and I published this as well as an analysis of fringe interpretation for homogeneous deformations [48] in holographic interferometry. I used the book chapter to summarize the work I had published in Optik on the analysis of fringes and their localization, and went on to describe the methodology of using holographic techniques to identify vibration modes. Eventually, support was obtained from Pratt & Whitney for me to do an analysis of a compressor fan, and this allowed me to acquire a holography table and laser, which I set up in one of the laboratory rooms. One of the holograms I obtained was used on the dust jacket for Erf's book.

Our move to Connecticut also presented a difficulty. We had wanted to rent a house while we got to know the area and decided where we wanted to live, but there was very little in the way of such properties to choose from. We actually put down a deposit on a rental in Newington, but while visiting a realtor in East Hartford, Elaine became interested in an old house in Coventry. She was strongly interested in antiques as a result of her diploma course in England, and her contacts with the Henry Ford Museum in Dearborn, so we went to look at the place. She decided she wanted to make a bid to buy the property, and the agent negotiated a slight discount from the owners. They had had a minor antique business, but I think the birth of a child had prompted them to move to something less troublesome. So it was that we bought an old house, circa 1732.

Musical Instruments and Vibration Design

I had set my own personal holography system, together with a speckle interferometer, while I was living in Dearborn, and I brought it to Connecticut. This allowed me to do research on string instruments, violins, viols, and guitars. I had been in contact with Martin Guitars while in Dearborn, and I had analyzed modes of some experimental guitars for them. One of the things I discovered was the extent to which the modes of the back of the guitar coupled with modes of the front. I made a simple structure, a box about 10 inches square with thin perforated sides about 1 inch high. This allowed me to examine mode combinations of two plates without the complication of the air cavity of the box. I set it between two mirrors so that I could view both sides simultaneously. This showed that essentially each plate mode resulted in two modes of the combined structure, one where the plates moved in opposite directions, and one where they moved in the same direction with the sides moving in opposition. I had seen this same behavior in guitars, and this clarified it. I eventually published a theoretical description of this phenomenon [49] together with Carl-Hugo Ågren, and published the experimental work [50] in 1981. For years I imagined that it might be possible to make improvements to string instruments using holographic interferometry that could prove economically profitable, but this never materialized.

One issue that came out of my work on musical instruments was how to make use of the information that holographic interferometry provided. Once the modes of a good instrument are known, and the modes of a new instrument are known, the question is: what should be done to the plates of a new instrument to duplicate the modes of good one? The obvious approach was to use perturbation theory, and I published a paper on this [51] showing how to express the changes in mode shapes and frequencies of a shell-like structure due to small changes in thickness. The new modes could be expressed as admixtures of the old ones. Each new mode is expressed as the corresponding old mode plus varying amounts of all the other old modes. The admixture coefficients decrease in proportion to one over the difference of the mode frequencies squared, so it is the closest frequency modes that admix most readily.

Interesting as my perturbation analysis was, it still didn't answer the question of how to change the structure to get specific modal responses. For that, we needed a method of inverting the perturbation analysis, [52] and I was fortunate to be joined by Gary Palma who formulated this from the basic theory I had developed. It turned out that calculus of variations could be used to obtain the minimum possible changes necessary to specify a set of admixture coefficients and frequency changes and that this solution was unique. Clearly, this had application to jet engine blades where vibration modes are stimulated by pressure fluctuations in the air flow caused by primarily by stators and which lead to what is called high cycle fatigue. We hired a young woman engineer/programmer, Irene Harrison, and she implemented this as a design program. [53] We initially conceived this as based upon holographic data, but on the suggestion of Hans Stargardter at Pratt & Whitney, we reformulated it to work with NASTRAN analyses [54] and in this formulation it was much more practical.

Although Pratt & Whitney found this interesting, it never did catch on there. One problem was that their structures were rotating, and our analysis was for stationary structures. Another was that we were an optics group, and this was something that should have been developed in the computer department of our laboratories. They had much more access to divisional problems to which it could have been applied, and had they developed it, they would have had a vested interest in promoting it.

Fringe analysis

I continued my development of a comprehensive analysis of the fringes of holographic interferometry, and this was influenced the work of Walter Schumann. I was scheduled in May of 1973 to go to the meeting in Los Angeles of the Society for Experimental Stress Analysis (renamed the Society for Experimental Mechanics in 1984). Unfortunately, the weekend before that trip, Elaine and I put in a stone path to our front door, and the lifting caused some injury to her back so that she was in severe pain. Because of this, I couldn't make this trip and stayed home to help her get around. Walter Schumann, whom I did not yet know, presented a paper there which was subsequently published [55] in Experimental Mechanics. When I first saw this paper, I didn't understand much of his mathematics because he used the notation **ab** instead of **a**⊗**b** where **a** and **b** are two vectors. This operation is what is called in some texts a "matric" product which creates a matrix from the products of the components of the vectors **a** and **b.** Schumann eventually published an extensive treatment of this subject in two books. [56, 57] This topic was pursued by many authors between 1965 and 1980. The following sections will describe the major aspects of the work I did is this field.

Fringes and localization

The general description of fringes and their localization, which I developed during my time at the Institute of Optical Research at the Royal Institute of Technology, can be summarized in Eq. (1). To first order approximation the field reconstructed by a hologram with any kind of time average interferometry can be described in the region where the fringes are observable as

$$S_{rec} = M(\Omega)S(\mathbf{R}) + \nabla_k M(\Omega)\bullet\nabla_R S(\mathbf{R}), \quad (1)$$

where S_{rec} is the reconstructed object field, $M(\Omega)$ is the characteristic fringe function, Ω is the fringe locus function, $S(\mathbf{R})$ is the reconstructed object field in the absence of interference effects, ∇_k is a two-dimensional gradient operator in the propagation vector variables of the field propagation, ∇_R is the three-dimensional gradient operator in the space variables, and $\mathbf{R}$ is the vector defining points in space.

The first term in the right hand side of Eq. (4) describes the observable fringes, and its form depends on the object motion that occurred during the recording of the hologram and any beam modulation introduced. Its argument, $\Omega(\mathbf{R})$, is the function that describes the contours of the fringes in space in that constant values of Ω define the contours of the fringes. The second term in that equation acts as a noise function, and fringes are observable when this term is zero. If the reconstructed field is specular, that is not a diffuse field, then $\nabla_R S(\mathbf{R})$ is zero or very small and the fringes are not localized. If the field is diffuse, the fringes will be localized to regions in space where $\nabla_k M(\Omega)$ is zero. One of the most significant results of my work was the discovery that, in general, fringes localize along a line in three dimensional space when the reconstructed field is formed by an optical system that has a large, two-dimensional aperture. I also developed the concept of fringe vectors for describing the way fringes will appear on a three-dimensional object, and this led to a connection between them and strains and rotations of an object.

Mode combinations and independent motions

The work I did together with N.-E. Molin on the effect of combinations of vibration modes in holographic vibration analysis showed theoretically and experimentally that vibration patterns combine within the argument of the zero-order Bessel function as phase vectors. For two phase vectors, $\boldsymbol{\Omega}_1$ and $\boldsymbol{\Omega}_2$ for example, the fringe function will be

$$M = J_0(|\boldsymbol{\Omega}_1 + \boldsymbol{\Omega}_2|), \quad (2)$$

where $|\boldsymbol{\Omega}_1 + \boldsymbol{\Omega}_2| = [\Omega_1^2 + \Omega_2^2 + 2\Omega_1 \Omega_2 \cos(\theta)]^{1/2}$, and (3)

θ is the phase angle between the two vibrations. We also showed that if vibrations were irrational, i.e. independent, the resulting characteristic function is the product of the two separate fringe functions. We also showed that for these motions, their fringe localization conditions were also independent, that is, each motion generates fringes that localize independently of those of the other motion.

Fringe vectors

One useful concept I developed for describing fringes in holographic interferometry is that of fringe vectors, which can be used to describe how fringes will appear on a three-dimensional object due to a homogeneous deformation or rotation. [58] The object displacement, **L**, due to such a movement can be described by the equation

$$\mathbf{L} = [f]\, \mathbf{R_o}, \tag{4}$$

where $\mathbf{R_o}$ is the vector defining points on the object surface, and [f] is a matrix defining the homogeneous deformation and rotation of the object. If this is substituted into Eq. (2), we have

$$\Omega = \mathbf{K} \bullet \mathbf{L} = \mathbf{K}\, [f]\, \mathbf{R_o} = \mathbf{K_f} \bullet \mathbf{R_o}, \tag{5}$$

from which we may define the fringe vector as

$$\mathbf{K_f} = \mathbf{K}\, [f]. \tag{6}$$

The fringe vector is normal to a set of fringe planes that intersect the object surface, and its magnitude is inversely proportional to the spacing between the fringe planes. One may go on to define an observed fringe vector, $\mathbf{K_{fob}}$, normal to the direction of observation of the object. [59] It is related to the fringe vector by

$$\mathbf{K_{fob}} = \mathbf{K_f}\, [P_{kn}], \tag{7}$$

where $[P_{kn}]$ is a matrix that projects the vector $\mathbf{K_f}$ onto the surface defined by the normal vector, **n**, from the direction of $\mathbf{K_2}$.

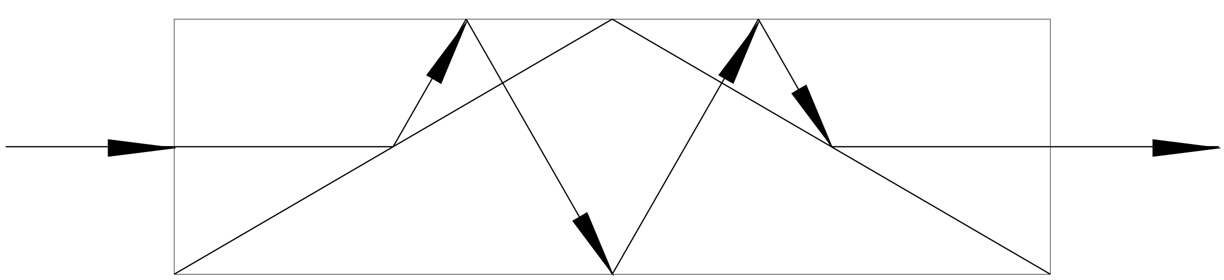
In April 1975, I attended a second conference in Glasgow, Scotland, *The Engineering Uses of Coherent Light*, where I presented a paper. [60] From there, I traveled to the continent where I used a Eurail pass to travel to Switzerland to visit the holography laboratory at Brown Boveri in Baden, headed by René Dändliker. From there I traveled to Belgium where I had been invited to present a lecture. From there I went on to Stockholm and taught a one week course on the theory of holographic interferometry. While in Scotland, the name of our company was changed from United Aircraft Corporation to United Technologies Corporation, and we became the United Technologies Research Center (UTRC).

In the fall of 1975, I arranged to teach an evening course in holographic interferometry at the University of Connecticut in Storrs, CT, which was not far from my home in Coventry. One of those attending was a graduate student, Richard Pryputniewicz, who was involved in a project to measure displacements of teeth due to orthodontic appliances. He was very enthusiastic about holographic interferometry and the theoretical material I was presenting, and we collaborated on a considerable amount of work. [61, 62, 63]

The Derotator Project

Interest in using holographic interferometry for the study of rotating objects began in 1970 with two papers [64, 65] delivered at the conference on Applications of Holography at Besançon, France by Waddell, Kennedy, and Waddell, and by Tsuruta and Itoh. By 1973, Waddell and Smart had published images of a rotating object seen through an optical system rotating at half the speed of the object, which removed the rotating component of the object's motion. [66] This was an improvement upon their earlier work, which stopped the rotary motion with a stroboscope. By 1975, interest in this developed at UTRC, and we managed to obtain a contract from Wright-Patterson Air Force Base (WPAFB) to develop a system of this sort for applications to jet engines. Our WPAFB contract officer on this project was Dr. James MacBain.

We invited Peter Waddell, the lead researcher in this area to spend the summer of 1975 at UTRC to work on this with us. The system he had built involved reflecting the object via a rotating corner prism and observing its reflection via a beamsplitter. We duplicated this system at UTRC and demonstrated it, but I felt that this design had a number of drawbacks. First, three quarters of the light available from the object is lost by transmission and reflection at the beamsplitter. Second, the image quality could be compromised by imaging through the beamsplitter and the corner prism. After some consideration, I opted for what was called a folded Abbé prism, a 30-120-30 degree prism with two 30-60-90 degree prisms as entrance and exit elements. The layout is shown in the following figure with a central ray trace.



A folded Abbe image derotator prism assembly

The entrance and exit prisms are separated from the central prism by a narrow air gap so that the entering and exiting rays totally reflect from the angled surfaces. The upper surfaces of the entrance and exit prisms and the long side of the central prism are coated with aluminum so that light reflects from them. Any optical system with an odd number of reflections will rotate an image seen through it. The simplest of these is called a Dove prism, which has one reflection and angled entrance and exit faces. The Porro prism has three reflections, normal entrance and exit faces, but, like the Dove prism, it is not symmetrical about the axis of image rotation. The folded Abbe prism has five reflections, normal entrance and exit faces, and is symmetrical about the image rotation axis. This design is compact and fits easily into a hollow shafted motor. For this system, we had made an air bearing motor assembly powered by what is called a DC torque motor. The assembly had an encoder wheel mounted on it to provide speed and phase-lock signals which could be used by a specially designed electronics system to lock the speed to one half that of the object and lock its phase to an exact position of the object. An encoder wheel was placed on the object to provide a signal for rotation speed and object position. One of the UTRC engineers, Jack Elkins, did the design of the system and it was built mainly by the technician Larry Gates.

The prism assembly had to be adjusted carefully so that its optical axis coincided with the rotation axis of the motor assembly, and various adjustment screws were provided for this purpose. Furthermore, both the illumination and the axis of the derotator unit had to coincide exactly with the center of rotation of the object. To accomplish that, the illumination to the object passed through a pair of mirrors with translation and tilt adjustments so that it could be aligned with the object rotation axis. The illumination passed through a beamsplitter so that the returned reflection to the derotator would also be aligned with the rotation axis. The reflection from the beamsplitter provided the reference beam for hologram recordings after sufficient propagation to match path length. In order to obtain sufficient light reflected from the object, it was generally coated with retroreflective paint. We demonstrated the operation of this system in the laboratory and recorded vibration modes on rotating objects. This development work was documented in a paper published in 1978. [67]

Recording holograms through an image derotator required the use of a pulsed laser because the general vibration level was much too great to allow real-time observation. Also, the rotating prism assembly approximated a half-wave plate and it introduced a Doppler shift to the beam passing through it as it rotated. Fortunately, our group had acquired an up-to-date ruby laser not long before this project, and I was able to use it for our derotator project. A continuous wave laser, which was aligned to be on axis with the pulsed laser, was used to provide alignment of the derotator system, after which its beam was moved out of the path of the pulsed laser.

In November of 1976, we moved our equipment to a Pratt & Whitney test stand near the Bradley Airport in Windsor Locks, CT, to attempt recording holograms of a compressor fan in flutter. This project was hampered by the winter weather and it stretched on until February 1977 when some results were finally obtained. Unfortunately, the results were not very definitive. The fringes obtained looked quite random and were probably the result of air flow around the blades rather than their motion per se. The project was continued at the P&W facility in West Palm Beach, FL, by a group headed by Joseph Bearden, where there was a spin pit that could be used for this work. A spin pit is evacuated to eliminate the air drag so that disk assemblies can be rotated as high speeds. We shipped our system down there, and we eventually designed and delivered a system to the Florida division for continuation of this work, under the direction of Jeff Clarady. Eventually, the group in Florida, did obtain vibration data on a rotating disk of blades under a separate Air Force Contract in 1980. We also provided a derotator system to WPAFB for their research use.

I had been in contact, before 1976, with Paul Ziekman at SKF research laboratories in Nieuwegein (Jutphaas), the Netherlands, and I sent him our reports from this project. In 1980, SKF issued a purchase order to UTRC for one of our derotator systems to be used to study ball bearings under rotating conditions. In the summer of that year, we delivered the system and I went over to supervise its installation and operation with a technician, Larry Gates, who was accompanied by his wife. I recall that we booked two weeks for this. The technical people there had tried to operate the system before we got there, and this created problems which Larry had to overcome. We eventually got the system operating and providing a derotated image of a bearing. I recall that we stayed at the Holiday Inn in Utrecht, which was the nearest large city. On the weekend, I traveled over to Germany to visit Klaus and Edelgard Biedermann, who were staying with her parents. Their son Niklas was about 1 year old then, and the major source of attention. I remember staying at a local inn, and on Monday morning Klaus was picked me up and we drove over to the Volkswagen plant to visit a researcher there. From there it was the train back to Utrecht to finish up the work at SKF. I remember that when we got to the airport for the trip back, there was a delay on the flight due to mechanical problems. We were not overly concerned, because we had a six hour layover at New York for our flight up to Hartford. The delay was so long, however, that we missed that connecting flight. As I recall, I never did hear from Paul Ziekman again, and I don't know to what extent they ever used the system. That was the only commercial derotator system we sold.

Speckle

I had become interested in speckle techniques in England at NPL, and I continued working in that area at UTRC. Several techniques had been developed which may be loosely described as speckle interferometry, speckle correlation, and speckle photography. Speckle interferometry has been generally used to describe the speckle interferometer invented by Burch, et al., where a speckled image interferes with a smooth reference beam. Speckle correlation generally refers to the interference of two individual speckled fields. In this case, changing the phase between the two fields will cause the resulting speckles to become alternately uncorrelated and correlated. A double exposure recording of such a speckle pattern, with a linear phase introduced across the field of view, gives a resulting pattern in which the speckle modulation varies with the phase and allows the phase to be detected. The third method, speckle photography, makes use of the fact that, when a diffusely reflecting object is photographed in focus, the recorded speckles move as if attach to the object. If an object translates between two photographic exposures, the resulting speckle pattern shift will match that of the object. This speckle shift can be measured by taking the Fourier transform of small sections of the photograph. This can be done easily done by illuminating small regions of the photograph with a narrow converging laser beam and examining the halo of light scattered around the undiffracted beam where it comes to focus. The doubling of the speckles results in a set of cosine fringes in the transform plane whose spacing is inversely proportional to the speckle displacement. If the object vibrates during the photographic recording, the resulting fringes follow a J_0 function.

In 1976, I published an analysis of the effects of defocusing in speckle photography, [68] and in 1977 I published a paper on the effects of lens aberrations. [69] My supervisor, R. K. Erf, decided by then to edit another book aimed at the field of speckle techniques, which he called Speckle Metrology. I wrote a chapter for this book as well. [70] My analysis of the motions of defocused speckles led to a concept for using speckle motions in three-dimensional space to measure strains with a technique I called tandem speckle photography. [71, 72] Although this was conceptually interesting, the implementing it was problematic. We required speckle recordings of an object, with two photographic plates at two separate focal planes, and these had to be recorded with three or more illuminations of the object. It was only practical to do this with a vibrating object where the motion could be kept constant for the different illuminations. These photographic plates had to be developed and illuminated by a converging laser beam at the same location for all of the photographs, and this point had to be located on the object. The halo (transform plane) fringe patterns all had to be evaluated on a digitizing tablet to determine the fringe spacings. Whereas this was reasonably accurate for measuring rotations of the object, the strain values were not accurate enough for engineering use.

Heterodyne Techniques

Heterodyne readout of interference fringes in holographic interferometry was introduced by Dändliker, et al., in 1973, [73] and this created a new level of precision and flexibility. By recording the two exposures of a double-exposure hologram with separate reference beams, it became possible to introduce a frequency shift between the two reconstructing beams and convert the fringes into sinusoidal irradiance fluctuations. These could be converted to electrical signals by a photodetector, and these could be analyzed by an electronic phase meter with a phase resolution of 0.1 degree. The fringes could be evaluated anywhere within the interferogram and to a much higher level of accuracy than by locating fringe centers.

In 1980, together with G. B. Smith of Pratt & Whitney, I applied heterodyne fringe readout to the analysis of speckle photographs, [74] (which are also known as specklegrams). Two specklegrams were recorded of an object, before and after a deformation, and were placed in a modified Mach-Zehnder interferometer. Light entered the interferometer as a narrow converging laser beam and was divided to illuminate the two specklegrams. The halos of light they scattered were combined by a beamsplitter, and their Fourier transforms were displayed where the beams came to focus. If the two specklegrams were aligned so that the same object regions were illuminated, the transforms would match and create an interference pattern. The illuminating beam could be translated horizontally or vertically to shift simultaneously the regions being illuminated, and if the displacements varied, this would change the phase of the interference pattern of the transform plane fringes. Introduction of an optical frequency shift between the two beams illuminating the specklegrams made it possible to detect the output fringes with photodetectors and measure phase changes with a phase meter. The changes in phase that were measured could be related to strain on the object. This was used to measure high temperature strains on a rotating turbine disk in a spin pit. [75]

I also used heterodyne interferometry to create an optical strain sensor that would measure strain between two points on an object in real-time. [76] In this case, the laser beam was split into two beams that illuminated two spots on the object, approximately 1 mm apart, with each spot covering about 0.1 mm diameter. A photographic recording was made in the back focal plane of a lens that collimated the light from the spots. The recording would have a random speckle pattern and within each speckle would be a set of fine fringes. The original light beams were used to illuminate the same two spots, and two photodetectors were placed in the transform plane, as far apart as practical. Strain on the object surface caused a difference in the phase of the signals detected by the two photodetectors. The phase meter we used allowed us to introduce a calibration constant so that it could display the strain directly. We eventually designed this instrument to have two identical, parallel lens systems, one for each angular view of the object surface.

Phase Stepping and Digital Electronic Holography

In the early 1980s, I was visited by Kathy Creath and by P. Hariharan, both of whom were working with phase stepping interferometry. With the development of solid-state TV cameras, it became possible to obtain images whose picture elements, or pixels, were accurate measurements of the light irradiating the sensor elements. If the irradiating field were an interference pattern, and known phase changes could be introduced between the two interfering light fields, the measured irradiance values could be used to calculate the phase of the interference for each sensor element. With heterodyne interferometry, the interference pattern had to be scanned by a photodetector whereas with phase-step interferometry, an entire field of values could be obtained at once. Concurrent with this development, was the rise of personal computers, which put remarkable computing power in the hands of the average engineer and scientist. Coupling these two resulted in a major development of TV image processing technology.

Sometime in the mid to late 1970s, the Pratt & Whitney group in West Palm Beach, Florida, developed a method using holography to detect bonding flaws of abradable seals in jet engines. It is a standard procedure to put a layer of material on the engine casing that is soft enough to be scraped away if a rotating blade touches it during operation of the engine. This assures the minimum possible gap between the engine blade tips and the casing and gives optimum engine performance. For Pratt & Whitney, these were typically made of felt metal that was brazed to the casing, although eventually ceramic materials were used. If the bonding was not solid, contact with the engine blades could rip out chunks of the seal material, which would pass through the engine. Although the engine would not be harmed by this, it decreased the engine efficiency, so it was important to test the bonding of these seals. The procedure developed by Joseph Bearden and Jeff Clarady involved recording a hologram of the part under test while it was being excited by random ultrasonic vibrations. Random excitation assured that the unbonded areas appeared dark in the hologram reconstruction and were not marked by any fringe patterns associated with sinusoidal resonance.

A company called Laser Technology, Inc., under the direction of John Newman, developed an automated holography system using photographic film and a monobath developer for this process, and had sold a number of systems to P&W for seal inspection. Although these systems were practical, the chemical processing of the film was a continuing problem, and became more so with increasing concerns regarding industrial pollution. As a result of this, I had a strong incentive to develop a completely electronic approach to holography based upon phase stepping, solid-state TV, and personal computers. The existing technology was called ESPI, for Electronic Speckle Pattern Interferometry. These systems utilized a standard speckle interferometer with a television camera read out. The usual way they worked was to record a reference frame of the random interference of the speckle field and the smooth reference field. This was subtracted from each subsequent TV image and the result displayed in real-time. With no change in the image, the resulting field would be completely black, but if the object were deformed, the change in the interference would cause areas where the phase change was pi to appear bright. In this way, fringes of deformation could be displayed.

The approach we took, however, used phase stepping. We had the controlling computer change the phase of the reference beam by $\pi/4$ (modulo 2π) after each TV frame and record a sequence of four images. A running set of four images was processed by an image processing board to yield the following image:

$$I_{im} = [(a\text{-}c)^2 + (b\text{-}d)^2]^{1/2}, \tag{8}$$

where a, b, c, and d are four sequential images from the TV camera. Mathematical analysis shows that this computation provides an image that is equivalent to a true hologram reconstruction. It is bright when no change to the object occurs and is modified by the zero-order Bessel function of the first kind when the object vibrates. Furthermore, a reference set of four images can be stored from the initial condition of the object, and these can be subtracted from the incoming images to yield double-exposure fringes. Multiple recordings of an object can also be recorded and made to interfere within the program to give time lapse interferograms. This was, in fact, the first example of a digital holography system. Because we employed a lens system to focus an image of the object on the TV camera, no thought was given to the possibility changing the focus of the image, although the data recorded always allowed for that possibility. I eventually demonstrated this is 2009. [77] Digital holography, as it is now understood, originated with the work of Schnars and Jüptner [78] in 1993. The digital holography system that we developed in the late 1980s eventually became the product sold by my own company, Karl Stetson Associates, LLC.

The image processing hardware and software for our digital holography system was developed by a company in Massachusetts called Recognition Technology, Inc. (RTI). The initial system they developed was made from image processing boards they had developed for other applications, and consisted of about 18 plug-in boards in a unit that weighed eighty pounds. The first system was developed for the Pratt & Whitney plant in North Berwick, Maine, and for the Pratt & Whitney plant in West Palm Beach, Florida. I was visited by my old friend and colleague, Nils-Erik Molin, and we received an order from his university Tekniska Högskolan i Luleå. That system was built and delivered, and I scheduled a week visit to Luleå to do the installation and training. After we got the system unpacked and put together, I tried to run the program and got nothing but noise. I called to RTI, and went through a long sequence of attempts to fix the problem to no avail. I remember being asked about the address assignment of a critical part of the program and being told that it was set properly. The main problem was that the engineer with whom I was talking assumed that it must have something to do with the difference in line current between Europe and the US – 220 volts @ 50 Hz versus 110 volts @ 60 Hz. They expedited delivery of replacement boards, none of which made any difference. I went with Nils to a conference he was scheduled to attend during that week while we waited for more parts from America. There, I ran into all the old colleagues from the Institute in Stockholm, and the group of us posed for a photograph together. When we got back nothing was improved by the new parts from RTI, and I left with nothing accomplished. The engineer from RTI eventually went to Luleå, armed, as I recall, with a complete new set of boards, and finally found that the software address was wrong, which was the entire problem.

The system for North Berwick was, essentially, the prototype for my optical design of this system. The idea was to have the optics as a self-contained unit on a small optics breadboard, 18 inches square. Around this time, the diode-pumped frequency-doubled lasers became available, and we were able to get 100 mw of laser output from a small package. This made it possible to have the laser on the same optical breadboard as the rest of the optics. I remember when we had installed our first digital holography system at the plant in West Palm Beach. When we finally got it operating with an image of a vibration mode on the TV monitor, one of the engineers asked, "How can we get a printout of the image? Another engineer recalled that they had a video printer and hooked that up to the line from the holographic processor. A single press of a button generated a printout, and video printers were used for many years with these systems.

The UTRC digital holography system was in large part due to my having hired a technician named William Brohinsky, whom I had met through my musical activities. My previous technician, John Palacki, had left, I needed someone to work in my laboratory, and Brohinsky was available. I worked out the idea of the phase-stepped digital holography system with him in discussions over lunch in the cafeteria. We initially conceived it as a binary system, but eventually realized the advantages of doing arithmetical computation of the image pixels, and we published our concepts and results. [79, 80, 81]

We also published a technique I had conceived for converting digital holographic data from vibration interferograms into numerical data. [82] This involved introducing a vibratory phase modulation into the reference beam at the same frequency and phase as the object vibration. I knew from my theoretical studies years before that this would shift the Jo fringes in a manner analogous to phase stepping in a two-beam interferometer. The idea was this, i.e., to record images with phase steps corresponding to four 90^0 phase shifts for a two-beam interferometer and use the recordings to calculate a wrapped phase map. When this is unwrapped, it differs from the true fringe locus function of the J_0 because the Bessel function differs from a cosine function. This difference can be calculated, however, and a lookup table constructed to convert the data to the correct values.

Travel and Conferences

During my twenty-one years at UTRC, I attended a fair number of conferences, and some stand out in my memory. I recall in particular, a conference I attended in Strasbourg in the autumn of 1979. My wife and I had purchased a Eurail pass and flew to Europe via Icelandic Air, with its obligatory stop in Reykjavik, and landed in Luxemburg. We took the train into France, and I remember staying in Dijon. We had a room in a pension and one morning the light failed in the bathroom. So I asked Elaine, "How do you say there is no light is the bathroom in French?" As one person we said "Jeanne D'Arc!" I also remember that the wall was quite thin between our room and the next, and one night while we were going to sleep, we heard a couple come in and become intimate. We continued down the Loire to Lyons and to Avignon which we hit in the middle of a mistral.

I also remember that I developed a cold. We continued on to Arles and Monti Carlo where the weather was better. The food was, of course, wonderful, and we both gained weight during this trip. I remember in particular a Mediterranean fish called dore. Finally, we took the train back north to Strasbourg for the conference.

At the conference, I was told I was to chair the sessions on holographic metrology. This would have been okay had the conference been organized well, but it wasn't. I had not met the speakers, so I didn't know who was there and who wasn't. Also, many papers weren't being presented and others were being presented in their place. The first morning session was quite chaotic. Lunch was, of course, very delightful. The afternoon session seem to see things settle down. To understand what happened next, you have to understand the venue of the conference. We were in a hall usually used by NATO or the EEC, I think, for international meetings. It was set up for simultaneous translation of the speakers, which you could obtain via head phones. The attendants sat in semicircular tiers very much like those in the United Nations building in New York. We chairmen were seated at a large long desk structure at the center of all this. So, let us say, it was an imposing venue.

I think it was the second paper in the afternoon session. It was being presented by a fairly young woman who started well, but then stopped and started again with a little more stress in her voice. She stopped again, and started once more with a note of severe stress. At this point, she collapsed into an epileptic seizure. Her colleague was attending her as best he could. I really didn't know what to do, I didn't speak French, I didn't know how to summon help, and the worst of it was that I felt that somehow it reflected on me that I didn't. Eventually I called for someone who could to summon help. Some people came with a stretcher and took her out of the hall and the conference continued. As my experiences with conferences continued, I found that this "trial by fire" actually prepared me to handle the unexpected.

I also remember a conference in Munich in 1978 sponsored by the Society for Experimental Stress Analysis, which was renamed the Society for Experimental Mechanics in 1984. Klaus Biedermann, who was originally from Munich, helped us find an inexpensive place to stay in the city near where the conference was held.

I approached the Optical Society of America in 1979 about the idea of a topical meeting devoted to holographic interferometry and speckle metrology. They suggested the Sea Crest Motel in North Falmouth, Cape Cod. I had experienced venues for other conferences where there was really no place for conferees to sit together to talk outside of a noisy hotel bar, so I drove over to the motel to check it out and found that it seemed quite suitable. Accordingly, the meeting was held there on the 2^{nd} to 4^{th} of June, 1980. Because we were not located in a city of any significance, I was concerned about attendees having something to do in the evenings. As a consequence, I organized informal presentations to be held in the evenings, plus beer. This was generally a successful meeting. We had a delegation from China, who had also made a visit to me at UTRC the week before. I recall a number of minor disasters. Charles Vest, who would later become Provost at the University of Michigan and President of the Massachusetts Institute of Technology, was there with his family. They were staying in a room under where one of the attendees from China was booked, and this person neglected to put the shower curtain inside the tub when using the shower. The resulting water dripping through the floor into the Vest's room required them to be moved. Also, on the last night, some vandal stole the overhead projector and was chased across the motel roof, finally dropping it over the edge into the sand. It was still operational the last day, but the lens/mirror assembly had to be held up in order for the images to be seen properly. I remember spending the last sessions doing that. I also remember that one attendee from the Netherlands developed a collapsed lung while standing in the lobby. He was taken to a hospital, and I arranged for another attendee from Holland to look after him and help.

Another memorable trip was to a conference in 1982 in what was then Yugoslavia. Although the meeting is described in the proceedings as being held in Dubrovnik at the Croatia Hotel de Luxe, it was actually held in a city further down the coast called Cavtat. We did drive up to Dubrovnik for events associated with the conference, however. Dubrovnik was amazing. It was originally an island, but the gap to the shore had been filled in long ago. The city was essentially all built of marble. It had been rebuilt after an earthquake in the 1600s and was essentially intact from that time. Elaine and I had flown to Zagreb where we rented a car and toured around the country for a week before the conference. We rented a small Renault 4, which seemed to lose power when the altitude was too high, but which never actually failed us. I remember noticing rather odd looking grey rock cliffs everywhere which I later found out was marble. In large measure, the entire country was marble. I remember visiting the cities of Split, Mostar, and Jajce. This last city was quite a disappointment. When we got there, we found that it was located, as most of the inland cities, in a valley between two rather high mountain ridges. There was a very large aluminum refining plant there that generated a remarkable level of air pollution. We stayed in a hotel outside the city, but did very little sightseeing within the city itself. After the conference, we drove to Belgrade and flew home from there.

In July of 1984, I participated in a NATO Advanced Study Institute on Optical Metrology in Viana do Castelo, Portugal. Again, we arrived a week early and rented a car to drive leisurely from Lisbon north to where the conference was being held near the border with Spain. The course spanned two weeks from the 16th through the 27th. The lecturers pretty much reads as a "who's who" of holography, René Dändliker, Rich Pryputniewicz, Charles Vest, John Caulfield, Ole Løkberg, Nils Abramson, Gert von Bally, etc. I had, not long before that, obtained a computer for word processing, an old Radio Shack TRS-80. I remember that I was still learning how to format text on it, so that my equation numbers were placed on the left to simplify things. I remember that at the end, we had turned the car in to the rental company and took the train back to Lisbon. We met some attendees from the course on the train and met them for dinner that evening.

In the spring of 1986, I was invited to present a paper at a topical meeting of the Optical Society of America in Honolulu, Hawaii on holography. I remember arriving there on Saturday evening, March 30th and waking up early (owing to the time change) on Sunday morning, which was Easter that year. I had the whole day to walk around the city and get oriented. I remember that there was a small park by the hotel which early Sunday afternoon was entirely filled with people picnicking. That evening, however, it was not only empty, but there wasn't even a scrap of paper to show that anybody had been there. I made it a point to spend time on the beach only early in the morning and late in the afternoon, and thus I avoided sunburn. Richard Pryputniewicz, also came to this meeting and was there with his brother, and they both got seriously sunburned. I remember getting some really wonderful sushi. Rich, his brother, and I took a car trip around the island, and we visited a waterfall on the other side. I also remember that it started to rain on the day I left.

In May 1988, I presented an invited paper at a conference in Tianjin, China. As part of this, visits were arranged for me to laboratories in Shanghai, Hangzhou, Nanjing, and Peking. We flew to Hong Kong, and the plane had to make a stop in Taipei owing to head winds. We spent a week in Hong Kong, then flew north to Shanghai. From there it was train travel to Hangzhou, Nanjing, Tianjin, and Peking. As I recall, one leg of the trip involved an overnight on the train. In Peking, we had booked a tour that was offered as part of the conference amenities. We got to see the Forbidden City, and the Great Wall among other things. Elaine became ill during the trip with a mild virus, and I developed it after we got home. The whole trip took about a month. I remember that on the way back we stopped on route in Tokyo and stayed overnight stay in Honolulu. When we were in Nanjing, a young woman acted as a translator for me, named Yuzhuo Chen. We remained in touch, and she eventually came to the US to study in Massachusetts. I met her at Kennedy airport and she stayed with us a couple of days, after which we took her up to the University of Lowell. We became friends, and she visited us in Connecticut quite often. One of the first things she asked us to help her with was to translate her name into something more English. She explained that in Chinese it meant essentially, carved jade, so we suggested she take the name of Jade in English. She eventually brought her husband and son over, and they still live here.

In April of 1989, I gave the Honorary Lecture for the first of a series of conferences in Germany called simply FRINGE, which have been held every four years since then. This conference was held in what was then East Berlin. Elaine and I flew to Berlin and spent some time with a couple, the husband of whom we had met at the NATO Advanced Study Institute in Portugal. They had visited us in Connecticut and offered to have us visit them in return. I remember that they had tickets for us to a concert by the Berlin Philharmonic with Seiji Osawa as guest conductor. It seemed a little odd to travel all the way to Berlin to hear the conductor of the Boston Symphony Orchestra. The husband was a very accomplished flautist. The meeting was Tuesday through Friday, and on Sunday Werner Jüptner and his colleagues met us at our host's apartment and we spent the night at a hotel in West Berlin before crossing the border the next day. The conference was quite successful. After it, Werner drove us down to what was then Karl Marx Stadt, since reverted to its original name of Chemniz. There we met Roland Höfling and Wolfgang Osten and visited their laboratories. I remember visiting a church there as well as an old castle. The weather was cold and there was still some snow on the ground. From there we went back to Bremen where Werner was one of the co-directors of BIAS, the Bremer Institute för angewandte Strahltechnik. I remember that we visited with Thomas Kreis and went to a local museum with him and his child. I also remember snapping some pictures of Werner's daughter, who was something of a tomboy, climbing a tree. As it turned out, I did not attend another FRINGE conference until 2005.

As the 80s drew to a close, Rich Pryputniewicz and I decided it would be a good idea to plan another major conference along the lines of the Topical Meeting held in 1980 with the target date of 1990. We approached the Society for Experimental Mechanics, and visited their headquarters in Bethel, CT. They normally have one major conference a year, usually in June, and had been supplementing that with a fall meeting, which often was more topical. They offered the fall 1990 meeting which was planned to be held in Baltimore, MD, and we accepted this. This was a larger meeting than the one in 1980, and it attracted some attendees from Russia. The Berlin wall had recently fallen, and détente was in the air. Thus, we were honored by the attendance of Yu. I. Ostrovsky, the Russian discoverer of holographic interferometry. My brother-in-law is originally from Baltimore, and had recommended some restaurants to us. I remember going to one called "Bertha's Mussels" with a group including Prof. Ostrovsky. I also remember going to another seafood restaurant where the recommended specialty was steamed crab. Our group was seated at a long table on which the waitress dumped a basket full of steamed crabs. Our foreign guests looked rather shocked, at which point the waitress demonstrated how we were to attack them with the mallets and forks we had been given. Obligingly, we all set about cracking the shells and digging out the crab meat, in an industrious manner. This was, as the 1980 meeting had been, a single session conference lasting four days. Rich and I didn't want to have people choosing which papers to attend and which to miss.

Rich and I had agreed that no papers would be presented at the meeting unless their manuscripts were published in the conference proceedings. This was to prevent the kind of problem I had encountered in the Strasbourg conference. I remember that Thomas Kreis from BIAS had not picked up on this deadline and was under the impression that he could bring his manuscript to the conference as was common with some SPIE conferences. When it was brought to my attention that we had not received his manuscript, it was just one day before the deadline. I called him and he apologized for missing this deadline, but said that it was too late for him to get a manuscript typed because it was a holiday celebrating the reunification of Germany. So I suggested that he fax me what material he could and I would get it typed at UTRC which was still possible because of the six hour time difference. This worked, and we got his paper in the proceedings.

I think it was in 1991 that we were approached by an Italian company, Alenia, located in the city of Foggia, who made aircraft body components for Boeing among other customers. They wished to set up a joint research project on the use of holography for non-destructive testing of these components. These were composite structures composed of graphite-epoxy skins bonded to a honeycomb core made of a material called Nomex. They were interested in detecting cracks in the skins and disbonds between the skins and core. This resulted in my traveling to Italy a couple of times a year interlaced by reciprocal visits to the US by my counterpart at their company, Pietro Ferraro. Shortly before this got underway, Elaine and I enrolled in an adult education course in Italian, and we took a trip to Italy in conjunction with this visiting Venice and cities in Tuscany. When we were living in England, we had also taken a course in Italian and taken a trip to Italy, so I had some small grounding in the language from that. I remember my the first day of my first visit to Foggia that Pietro had to go to Napoli and I was left to work with his technician, Felice, who spoke essentially no English. It was quite an experience, but I managed. Pietro and I agreed that at lunch we would speak only Italian, and I remember that eventually I began using things like the future tense and the subjunctive. By the time the project ended in 1993, I was able to go into a hotel in Italy and book my room, order my dinner, etc., all in Italian. The hotel staff knew I was American, of course, but didn't feel the need to speak English to me.

Another thing with which I became involved in the early 90s involved my old invention of the total internal reflection hologram. I had visited Rene Dändliker, who was then at the University of Neufchatel, in Switzerland, with Elaine. We were introduced to William Hugle and shown a promotional video on their proposal to develop a new technology for microcircuit printing using TIR holography. They had identified a photopolymer made by DuPont that had the low scattering required to obtain clean images. Sometime later, I was contacted by their major financer, Ali Resa Nobari, and I drove to New Haven, CT, to meet with him. He had taken over the company because of disagreements with Bill Hugle, and he showed me some examples of what they were able to accomplish, and I was very impressed. He asked me if I could consult with them to help deal with the various technical problems they were encountering. I said I would take that up with my supervisors at UTRC and get back to him. I presented to Fred Leonberger what I had learned from Nobari, and described this as a technology which the corporation should keep abreast of, but that the company, Holtronics, SA, could not afford to pay UTRCs rate for my consulting. As a result, he agreed to let me consult with them privately. I negotiated terms with Ali which included my travel expenses plus a fee and stock in the company. I made several trips to Neufchatel, and continued this for a short while after I left UTRC.

The photopolymer they used was spun onto the surface of the substrate and dried. It was exposed to the laser pattern, in the ultraviolet (361 nm if I recall correctly) and then exposed to uniform UV to develop the pattern. One of the problems they had was that the photopolymer shrank as it was processed. If the hologram were reconstructed at exactly the same angle at which it was recorded, the reconstruction efficiency was severely reduced. When I experimented with this in Stockholm, I was able to compensate for this by changing the angle of the reconstructing beam without much loss of resolution by keeping the distance between the hologram and the mask very small. The combination of shorter wavelength and the fact that they had settled on a fairly large distance to the mask, 0.1 mm, required that they reconstruct at exactly the same angle as the recording and suffer the loss in diffraction efficiency. They developed a system for reconstructing the hologram with a narrow beam that was scanned across the hologram while the distance to the substrate onto which it was printing was varied to compensate for lack of substrate flatness. In my memory, I believe they sold several of these systems, but in the end the company did not survive and the stock I hold is worthless. I still feel this is a potentially practical approach to microcircuit printing, and the first step in making in work would be to develop a photopolymer that doesn't shrink during development.

Leaving UTRC

When I began working at UTRC (initially UARL) under Robert Erf, it was quite exciting, and I feel that I accomplished significant work there. Things gradually changed over the two decades, however. In 1977, Eli Snitzer came to work there under Anthony J. DeMaria. In the early 80s, I felt unhappy with my position under Erf and I sought to transfer to Eli's group under Tony DeMaria. Eli was agreeable, and this took place. I don't remember exactly what year that was, but from what research I've done, Eli had left UTRC by August of 1984, so it must have been before that, probably around 1981. Eli was replaced by Fred Leonberger, who worked under Tony DeMaria until 1991, when he left to head United Technologies Photonics. Under Leonberger, I worked under several people, the last of whom was Bill Nighan. A number of different management concepts were being introduced, such as "Matrix Management," where you were supposed to be working for several different organizations within the company depending on what you were doing, etc.

Although by the early 1990s we had developed an important product, our electronic holography system, which was being extensively used by Pratt & Whitney, UTRC had no interest in promoting it as a product. Pratt had dabbled in this, but the overhead involved made the cost of such systems beyond most customers' budgets. The company who had developed the computer hardware and software for our system, Recognition Technology, Inc., was interested in pursuing it as a product and needed someone with optics expertise to provide the optics hardware. I had been essentially asked to divest our group of any production of these systems. I contacted someone my brother-in-law had recommended and referred him to RTI. Rather than simply use the design I had developed, he insisted on making "improvements." As a result, RTI did not want to use him in producing these systems.

The main system was our digital holography system; however, I had actually developed additional systems at UTRC, all of which used the same computer program. The one most closely related to holography is what we called our speckle correlation system. Here, the laser beam was divided into two beams and illuminated an object from two directions to either side of the observation direction of the TV camera. One of the beams was phase stepped so that the images recorded by the camera could be processed in the same way as with the holography systems. This configuration allowed measurement of displacement in a direction normal to the camera observation, and it was excellent for measuring things like thermal expansion and one dimensional strain.

The next system we sold was what is called a shearography system. Here, the object was observed through a system that split the image into two laterally displaced images. In our system, we used a Michelson interferometer to create the two images, and used a piezo-electric mirror in one arm to obtain phase stepped images. This system gives fringes that are proportional to the slope of the object displacement. The configuration tends to be less sensitive to environmental vibration than holography and is often used for nondestructive testing.

The fourth system we sold was a system for projected fringe moiré. We used the interferometer from the shearography system to generate a set of fringes in a beam that illuminated the object. These fringes had to be broad enough to be resolved by the TV camera, of course, and they were phase stepped. The fringes illuminated the object surface from an angle and the object was viewed along its surface normal. This system allowed measurement of large scale displacements and vibrations of an object. It was also possible to compare the shapes of different objects, because the images did not depend upon any speckle pattern in the object. In fact, this system worked much better if the speckles were too fine to be observed.

By 1993, it was clear that I should consider leaving. For that reason, I did not go to the FRINGE '93 conference, nor did I attend a conference in India to which I had been invited even though they sent me a check (which I returned) to cover my travel expenses. Two years earlier, the company had offered a very nice termination package for retirees, but at that point I was not vested in my retirement and would have faced a couple of years of no income. In 1993, we were offered a single year continuation of our health plan, and that was it. RTI had obtained a contract from Sandia Laboratories for one of our systems, so there was a system to sell. As the year rolled to a close, I looked at what I could expect for a pension, what I would get as a severance package, and what Elaine was earning teaching at a local college. The numbers added up. I could expect to live on my pension, if all else failed, and I had enough of a bank roll to start manufacturing optical systems. With RTI's contract from Sandia in place, I "hit the ground running."

Karl Stetson Associates

I toyed with a number of names for my own company, but the marketing person at RTI, Sheldon Isaacs, suggested that I should use my name in the company in order to capitalize on what reputation I had. I should not have used my first name. The spelling of Karl with a K has always been a problem. The only place where it was assumed to be spelt with a K was in Sweden, where "en Karl" is equivalent to what we call "a guy." Be that as it may, I was in business for myself as of January 4, 1994. I bought a laptop computer, a printer, a fax machine, a desk, and eventually a file cabinet. I set up in a spare room in my house that overlooked our back yard. Initially, I used a machinist that RTI had used, but eventually I settled on a local machinist.

I decided rather early that year that I would need to use an accountant for my taxes. Initially I tried to work with a fellow I had met a couple of years earlier, but I found him rather confusing. One thing he did do for me was to look at my paper records of expenses, etc., and point out that no accountant would want to work with that. He suggested that I obtain the program Quicken. In retrospect, I should have gotten QuickBooks, but the Quicken program worked and now provides me with a document of my activity over the years.

The system for Sandia Laboratories went pretty much according to plan. One problem I had was that the delivery I could expect from Newport Corporation on an 18 inch square optics breadboard was not soon enough. I had worked with graphite/epoxy sheets on the project with Alenia, so I was interested in whether we could use them to make an effective optics breadboard. I conceived the idea of using threaded standoffs to separate a pair of sheets with holes drilled in them, and filling the spaces in between with foam to deaden vibrations. I noticed a company in the yellow pages in the neighboring city of Manchester, CT, called Composites, Inc. I talked with the owner about my idea, and he agreed to fabricate a unit based on that design. I was able to provide him with an old Newport honeycomb breadboard which he could use as an assembly plate. This worked out quite well, and over the years he produced a considerable number of these items. Initially we used stainless steel standoffs, and the units weighed about ten pounds, but eventually he had aluminum ones made and that cut the weight down to about six and a half pounds. Most of the weight came from the standoffs which were on one inch centers in an eighteen by eighteen array. At one point I put a new product announcement in the journal Physics Today. I got one inquiry, but the cost was beyond what the customer could afford.

Another factor occurred during the Sandia job, and that was my reaction to ordering components with which to build the system. I had ordered equipment and components all my career, but they were always bought by the company or organization for which I worked. With a slightly eerie feeling, I realized that I was buying these things with my own money. If something happened and I didn't sell the unit, I was going to lose that money. I just had to have faith in the system. It worked, I got paid in May of 1994, and I now had more money that I had before. I'd made a profit. In October of 1994, I also received payment for a system RTI had sold to Pratt & Whitney for seal inspection, and my business was looking quite good.

Only once in the last twenty years did I incur a serious payment problem. A company in Israel had ordered a system and sent me a purchase order. This occurred after they had sent us some samples to test to verify the effectiveness of our equipment to solve their measurement problems, so it seemed that they were committed to purchasing one of our systems. The purchase order had a clause in it stating that they had the right to "change the delivery date within 45 days from the date of the purchase order." At the time, I didn't think much about it because a change in the delivery date wouldn't be a real problem, although it did give me a little pause. Almost exactly 45 days after the date of the purchase order, I got an email from the purchasing agent that they were canceling the order. I took this to a lawyer, and he pointed out that their clause did not give them the right to cancel the order but only the delivery date. I had obtained from them an irrevocable letter of credit for the payment, so they were really committed to this purchase. I communicated this information to the buyer, and his reaction was to "change the delivery date to several years in the future. I pointed out that by now the 45 days had passed and he could no longer do that. After considerable back and forth, they settled the case by paying for the cost of the components I had purchased, which amounted to essentially my costs for a complete system. I think this settlement was mainly dictated by their US parent company. Eventually, all of the items were resold in subsequent systems, so everything worked out okay.

In the spring of 1995 I got a telephone call one evening just before supper from Dale Flanders at a company called Lasertron, in Burlington, MA. He had been given my home number after calling UTRC – Elaine answered and called me to the phone. I spent a long time talking to him about the problem they were having developing laser diode systems for communications applications, and Elaine was getting impatient about having dinner. I was able to arrange to rent them a system to deal with their immediate problems while they placed an order and we produced a permanent system for them which was delivered in the autumn. I remember the woman engineer in charge of the project was Livia Racz. The main feature they used was our time lapse recording and replay routine. Our program had been designed to record holograms of an object at selected times and store the results. With this procedure, you could record holograms of an object at a number of times during some process and retrieve them after the fact to create interferograms between the various states. Lasertron used this to record an initial hologram of an object which was located in a mount from which it could be removed and replaced exactly. This allowed them to see what happened to the device during thermal processing, and this allowed them to solve some otherwise intractable problems. I got paid in October of 1995, and this was the first system I sold as the primary company dealing with the customer.

In December of 1995, I received an order from the Northern Illinois University in DeKalb, IL, for a system for Prof. Thomas Rossing. I had met him at a meeting of the Acoustical Society of America in Cambridge where I had been invited to present a paper. Prof. Rossing was a well-known researcher in musical acoustics, and the university was expanding his laboratory. I sent the system there via UPS and there was considerable damage. I was providing an interim laser, and eventually replaced both the laser and the breadboard. I remember having dinner at a Thai restaurant in DeKalb on my first visit and nearly having my mouth burned out by the hot peppers. Tom Rossing also arranged a bike trip and got me a loaner bike to ride.

Early in 1996, I got in touch with Ralph Windeler who had a business servicing the tire testing machines that had been sold by GCO, Industrial Holographics, and Grant Engineering. I had met him when he and Ralph Grant visited me at UTRC around 1990. He had left Grant Engineering and now had his own business, and he was interested in the possibility of selling our holography and shearography systems for tire testing. I had visited him with Elaine earlier when we had traveled to Ann Arbor to visit the eye doctor who had taken over the patients from her original doctor. I made a trip to his house in Bloomfield Hills, MI, that spring, and we visited a couple of companies in northern Ohio. My demonstrations there did not raise much interest. Next, we made a trip together to a tire company in Kingman, AZ. There, we did manage to do a demonstration that resulted in a request for a quotation for a replacement system. These companies were using photographic holography or shearography for their testing, and the prospect of going to direct digital systems was attractive. When Ralph and I got to the restaurant that evening for diner, I started to map out how we would submit the proposal. Windeler Engineering was to me the key company, because he had the experience and familiarity with this technology and could provide the vacuum chamber and apparatus for handling the tires, etc. I would provide the optics for holography or shearography and integrate it into the tire testing machinery. Jawed at RTI would provide the computer hardware and software, as always. To my disappointment, Ralph balked at being the primary company in this effort. As a consequence, I prepared the quotation and to proposed to act as the integrating company. I have always felt that this doomed the proposal and was the reason we never managed to sell any systems in this market.

In June of 1996, Jawed Wahid from RTI and I exhibited at a meeting of the SEM in Nashville, TN. This was the first time we exhibited our system together, and we had a demonstration system operating for people to observe. We were using the repaired optics breadboard that had been replaced in the system at Northern Ill. University and had purchased our own HeNe laser. RTI had a booth from RTI's conference exhibitions which we set up in the exhibit hall. We got quite a good response from attendees, and eventually sold several systems as a result of this show. One of the major sales we obtained from the SEM show was to the Rockwell Science Center in Thousand Oaks, CA., and this involved several of our systems. I also obtained an order from a researcher named Dan

Borza in the University of Rouen, France. We also sold a systems to Jet Avion, to Professor Ume at Georgia Tech, in Atlanta, and Motorola in Phoenix, AZ, so as 1996 turned into 1997, business was quite brisk. We exhibited at a fair number of shows in the years that followed, but none resulted in further sales. The rationale was that a single sale would more than pay for the cost of the exhibition, but that sale never materialized.

We made three technical advances between 1994 and 1997. With the introduction by Microsoft of Windows 95, Jawed undertook to have his original DOS program converted over to a proper Windows program, which was eventually called PCHolo32. He hired a free-lance programmer in New Hampshire named Paul Gauthier with a company named Image Softworks to do this. It took about two years to accomplish, so that it ended up running in Windows 97. Before this, our program ran in a DOS window in the Windows program.

The second advance was to develop a real-time phase image program. This was for the static deformation routine, which displayed cosine fringes of the static deformation in real time. The basic idea was to calculate the phase of the hologram image for the reference hologram, store that, and subtract it from the phase of the incoming image in real time. The phase would be calculated as an image with eight bit pixels. Subtracting two such images would create an image with nine bit pixels, and would contain a phenomenon that can be called random wrapping. This was first shown, I believe, by K. Creath. Let us assign the variable $\Phi(x,y)$ as the phase of the stored image. Because of the random nature of the speckled image of the diffusely reflecting object, $\Phi(x,y)$ varies randomly from $-\pi$ to $+\pi$. If $\Phi(x,y)$ is near $-\pi$ or $+\pi$ in value, and the change in phase due to deformation of the object, $\Delta(x,y)$, is large enough, subtracting it from $\Phi(x,y)$ can cause the recorded phase of the image of the deformed object to wrap, either from $-\pi$ to $+\pi$ or from $+\pi$ to $-\pi$. This occurs randomly, and the random wraps will persist when the phase images are subtracted. In the mid '90s, I had realized that wrapping the nine bit image into eight bits would remove the random wrapping effect and give a simple image of the wrapped phase of the cosine function describing the interference of the two hologram recordings. This could be done in real time and was very helpful for nondestructive testing. Jawed designed the program so that when such an image was saved, it also saved all the necessary files needed for phase unwrapping, which I will discuss next. We published this in the SEM journal Experimental Techniques. [83] We had also implemented the technique I had published for vibration. [82]

The third advance we made was to generate a robust phase unwrapping program to use as part of our program. Phase unwrapping was a hot topic at this point, and many people had published routines to do it. The basic issue was that with phase stepping interferometry any phase was obtained by an inverse tangent calculation. This meant that a continuously increasing or decreasing phase function would wrap back to the same range of $-\pi$ to π over and over again instead of continuously increasing or decreasing. To obtain the actual phase function, this wrapped function had to be unwrapped. In the absence of errors, this was a simple problem, but holographic data often contained inconsistences that resulted in errors for simple methods. As a result, numerous complex methods were developed, many employing what are called "branch cuts" which prohibit unwrapping across certain specified boundaries. With our phase stepping routine, which employed four phase steps, it occurred to me that it was really arbitrary which of the four steps was taken as zero. As a result, from our data it was possible to construct four different wrapped phase maps, and let us refer to them as 0, 90, 180, and 270. If we take two of these corresponding to 0 and 180 (or alternatively 90 and 270), it was possible to subtract them and divide by 2 and get values of either $-\pi/2$ or $+\pi/2$. Alternatively, we may add them and divide by 2 to get a map where the phase ranges from $-\pi/2$ to $+\pi/2$ and in which there are twice as many wraps as before. In this situation, however, the first map defines regions where the wrapping must be the same for all pixels in that region. So the idea was to use the calculated wrap regions as a guide to unwrapping the doubled wrapped map. I also realized that the process was not dependent on the number of phase steps used to generate the initial wrapped phase map. We could create unwrap regions by simply assigning 0 to all pixel values from 0 to 63 and from 192 to 255 and 1 to all pixel values from 64 to 191. Similarly, we could create the double wrapped phase map by adding 128 to all pixels from 0 to 63 and subtracting 128 from all pixels from 192 to 255. Paul Gauthier implemented this program, and it has been an extremely successful part of our product. The method fails when the fringes are so closely spaced that the calculated phase regions become connected. We published the method [84, 85] but it has not been picked up by any other researchers, nor was it mentioned in the book by Ghiglia and Pritt. [86] That is a pity because it is one of the few truly innovative approaches to this problem.

During this time period, we also developed a system for Nils-Erik in Luleå for pulsed hologram recording using what was called spatial phase stepping. This involved having the reference beam of the speckle interferometer off axis so that there were visible fringes in the recording, and a Fourier transform was made of the recorded image. If the aperture for the imaging optics was small enough, the Fourier transform would have three distinct parts, a halo around the zero-order beam and two side bands. One of these was selected by an aperture, translated to the zero-order position, and inverse transformed. The result is a function which can be evaluated for its phase. For us, the issue was modifying the program so that it could record a pulsed laser hologram. We were successful, and Nils his colleague Henrik Saldner, and I published a paper on this. [87]

Death

The autumn of 1996 brought a major change to my life. In September, Elaine felt she was having trouble pronouncing certain words, like "Connecticut". I hadn't noticed any problem, and she often tended to be overly concerned with her health, so I really didn't take it too seriously. She went to our local General Practitioner and he observed that when she smiled, the right side of her mouth didn't turn up symmetrically with the left.

He recommended she visit a neurologist. The first one he recommended was not in our medical system's network. We had replaced the UTRC health insurance with a major medical plan from the company called Golden Rule. They recommended an alternative, and she went to see him. He did some tests, including electromyography and one called a tensillon test where she was injected with a substance known to reverse temporarily the effects of myasthenia gravis, an autoimmune disease with symptoms very like what she was experiencing. Nothing seemed to show up. We hoped that she was experiencing Bell's palsy which often goes away by itself. The symptoms progressed, however, and Elaine decided to go to the neurologist originally recommended by our GP. At this point she went to the GP with a written note, because she felt unable to express herself verbally to argue for what she wanted. This neurologist took her symptoms very seriously, and he felt it was more likely to be MG than Bell's palsy, because Bell's palsy usually happens and gradually goes away. By contrast, her symptoms began slowly and were gradually getting worse.

The second neurologist ordered a second EMG at the Health Center at Farmington, specifically requesting a single filament EMG, which would definitely rule out MG if it were negative. The second EMG was initially delayed a week due to bad scheduling and was done the first week in December. In the meantime, the second neurologist performed a second tensillon test at which I was present. Based on what we had learned from the first one, we felt it was important to do something to try to quantify what was happening when the drug was administered. The neurologist agreed and Elaine read aloud from a magazine for the whole time. I noticed that after about 90 seconds, when the first injection seemed to have no effect, he did a second, exactly according to what we had read about this procedure. At one point her eyelids started to flutter, which was a clear result of the drug, so we knew it was active in her system. The results of this test were also negative, but we felt that it was more conclusive than the first one. When the second EMG was done, I had the opportunity to sit and watch the entire physical examination and EMG procedure. This doctor, our third neurologist, stopped short of doing the single filament EMG, because he said that what he found indicated that it wasn't necessary, and he didn't want to put her through it. He was not at liberty to tell us what he had found, but would communicate it immediately to our second neurologist who would tell us.

When we got home, we called the second neurologist. He had to call back, being busy, and I wanted to take a walk for some exercise, so Elaine handled the return call. The doctor wanted to see us that Thursday, the current day being Tuesday. That was impossible because I was to leave on a business trip to Rockwell in California that day. We spent Wednesday trying to reach the neurologist to find out what the diagnosis was and to schedule a visit. That morning, my brother-in-law called from Massachusetts that my father was in the hospital with a critical infection. He and my sister were there with him and wanted to let us know so we could come up and have the chance to see him in case he died. We had seen him the week before with my sister, her family, and my brother at our house for Thanksgiving dinner. He was eighty-nine years old, very feeble, and we knew that his death could come at any moment. Yet, we had a very strong premonition from the second EMG that something was seriously wrong with Elaine, so we stayed home and tried to contact the neurologist. Around midday, my brother-in-law called again to emphasize that this really looked like the end for my father. I explained that we couldn't come because we absolutely had to find out what was wrong with Elaine.

That evening the doctor called and we scheduled an appointment with him on the following Monday, but he said he didn't want to tell us the diagnosis over the telephone. I argued that I was going to be going away on a business trip, leaving Elaine home alone to worry about this, and that what he was doing was worse than telling us. Finally he relented and said that they thought it was motor neuron disease. I thanked him for telling me, and agreed to make it to his office on Monday for a full briefing. We immediately looked up what motor neuron disease was and found that it is the general term for the group of diseases that include Lou Gehrig's disease, more clinically referred to as amyothrophic lateral sclerosis, ALS. Shortly after that we got a call from my sister that my father had died. When I told her that Elaine had Lou Gehrig's disease, it was like trading blows. Her reaction was "I'm so sorry!"

We were rather in shock. I pulled some information about it off of the Internet, as I had done for MG when we thought that was her disease. But the next morning, as I was packing to leave, I found myself crying spontaneously. At this point I knew that ALS was an incurable disease, which it would progress inexorably, weakening Elaine, until it finally killed her. I had estimates of how much longer I could expect her to live: two to five years on average. The airplane trip was terrible. Everything I saw, from older couples traveling together to young couples reminded me of what I was about to lose or what I had had. Fortunately there was nobody sitting beside me on either leg of the flight. As I entered the first plane, I found myself in tears and asked the stewardess for a napkin. She looked at me and gave me a stack. I mumbled something about personal problems and thanked her. She said that if there was anything she could to do to just ask. I thought to myself, "I wish there were."

Elaine and I were in touch by telephone and by Email. I completed my business and returned on Sunday as scheduled. As I rode to the parking lot in the courtesy van, I asked the driver how much snow they had had and he said about a foot. I was stunned. My car was waiting for me and I drove home, due to arrive there about 9:30 PM. As I got to Bolton, I noticed a stop light was not functioning and the houses were dark, and it struck me that the power lines had been broken by the storm. As I continued, I came to a region where power was restored, but as I turned off onto the road to my house, the houses were dark again. I imagined that our driveway would be filled with snow unless Elaine had contracted someone to plow it out. To my delight, it was shoveled enough for me to get the car into the garage. I could see a flashlight in the kitchen. As I entered the house, I could smell wood smoke, and I knew that the fireplace was being used. Elaine told me in that she had been without power for about 24 hours and had called my brother. He had come over and made a fire for her so that she could get warm, shoveled the driveway so that I could get my car in, and had stayed with her, helping her cook supper over the fire, until they confirmed that my flight had landed.

I spent that night catnapping by the fire, keeping it supplied with wood, because I knew the house would cool down to a dangerous level if I didn't. That morning we had breakfast, boiling water over the fire for coffee, and I washed the dishes from the day before with more water boiled the same way. In the morning, while I was shoveling out the rest of the driveway, we got our power restored, but then the furnace wouldn't start. We had to wait most of the day for the furnace repair people to get there, but with luck we were able to make a 4:00 appointment with the neurologist.

The doctor was very good, giving us nearly an hour of his time and patiently answering our questions. I thanked him for telling us in advance because, even though it caused us grief, at least we had had time to prepare our questions. Had we learned the diagnosis in his office, we would have left with dozens of questions unasked because we would have been too shocked to think of them. We arranged to get a second diagnosis with another specialist in Boston. He also gave us a prescription for Riluzole, the only drug available at that time for treating ALS. We had learned about it from the Internet, and knew that it offered a scant three months extension of life expectancy. My reaction was that Faust had gotten a better deal than that. All the drug seemed to promise was three more months of suffering.

Intellectually, it was interesting how this disease is diagnosed - it is sort of like "none of the above." There are many conditions that can give symptoms like those that define ALS, and, in large part, when everything else is eliminated, it must be ALS. And that isn't truly saying very much because the mechanism for ALS is not at all understood. Elaine had some ALS symptoms, mainly characteristic of upper motor neuron disease, but what the second EMG had picked up was an indication of lower motor neuron disease. The various indications, plus the progressive nature of the disease - it was clearly getting worse - seemed to indicate that it is ALS.

The diagnosis was on December 6, 1996. Needless to say, the next year was very trying. Her muscles continued to weaken. In her case, the disease started at the top of the spinal cord and affected muscles in her mouth and throat first, making speech difficult and affecting her ability to swallow, then worked its way down to the rest of her body. She would often choke on food and suffer a laryngeal spasm. I ended up putting all her food through a blender to render it into a thick soup so she could swallow it. None the less, she lost considerable weight due to poor nutrition and muscle atrophy.

In May of 1997, Jawed and I made a trip to the University of Akron to do a demonstration of pulsed laser holography for a group exploring holography for breast cancer detection. They had done quite a bit of work with pulsed laser holography using photographic film and had some promising results; however, the time delay associated with developing photographic holograms was a severe practical drawback. The key technique was to place the test subject so that she lay over an opening in a chamber where the pressure could be pulsed. By timing the laser pulses to catch the breast tissue before and during the pressure pulse, solidification within the breast tissue could be observed. Basically, we set up the kind of system we had sent to Nils-Erik in Sweden; however, we had never had the opportunity to test it out with an actual pulsed laser. We had great difficulty interfacing it with their laser, and their setup was very hard to work with. We had a lot of patterns forming in the reference beam, and as a result we didn't get any meaningful results. At this point, Elaine could still walk, but talked only with the use of a speech synthesizer. I had told her to try calling me at the motel, but the people at the desk didn't understand the synthesizer well enough to route the call to my room.

In June 1997, during Elaine's illness, Jawed and I exhibited at the SEM conference in Bellingham, WA, across the bay from Seattle. We had hoped that, after the success at Nashville, we would get some sales from this show as well. Elaine could still walk at that point, and we decided to go to the conference together. It was rather heart breaking to see her frail and in a wheel chair rolling through the airports where previously we would have walked briskly. We spent a Sunday afternoon shopping at Nordstrom's, but otherwise she was stuck at the motel where we were staying. During the conference, I was told by a colleague that I had been chosen to receive their Murray Medal and give the corresponding lecture in 1999. That really broke my heart because I knew that by then Elaine would be dead, and she would not see me receive this acknowledgement of my accomplishments, which were in many ways directly connected to my having met and married her.

She had been teaching remedial English at a local college, and in the summer of 1997 she taught this using a speech synthesizer and a program I had written for it, with the help of a colleague, to read and speak text from her computer. In early August, she had a feeding tube put in her abdomen, and at this point she seemed to lose the ability to walk on her own. I moved her down to what had been our dining room and rented a medical bed along with other equipment. Caring for her full time put such a strain on me that I hired live-in care givers. She was visited by her sister and eventually by her mother and cousin. Other friends visited to pay their respects and wish her well.

One major problem was excessive salivation, a common occurrence with ALS. I obtained a set of clips from our dentist to allow us to put napkins under her chin to blot the liquid, but her real problem occurred when she tried to sleep. She would continually cough up liquid from her throat which she could not swallow. We experimented with several drugs to try to suppress this, but didn't have much success. One, which seemed to work was called Elavil, but it seemed to knock her out, so we only tried it once. In the middle of November, 1997, Elaine suggested one night that we try Elavil again so that she could for once get a good night's sleep. I gave her some just after 9:30 PM, and about 10:15 I was putting her to bed when she started to have what turned out to be a seizure, and couldn't breathe. I got her back on the couch and managed to get her to respond to the question of whether she wanted me to call 911, the emergency number. She tapped yes, and I called them. They came very quickly, put her on oxygen, and put her in the ambulance to take her to the emergency room at the hospital in Willimantic. I followed with my car, and by the time I got to the emergency room, she was essentially catatonic, staring forward with unseeing eyes, lungs heaving to try to move air through her constricted throat. They found that her carbon dioxide level was about twice normal due to the fact that she was not moving enough air out of her lungs. Gradually, she came out of it and eventually was even able to write a little. They wanted to do a CAT scan on her, but she couldn't endure the position of being flat on her back because she couldn't breathe in that position. They gave that up. Before she had regained consciousness they had suctioned her lungs with a thin tube inserted through her nose. They removed quite a good deal of white liquid. X-rays of her lungs showed that she had the beginnings of pneumonia, and she was admitted to the hospital.

This led to a tracheostomy. It seemed that after each invasive operation, she lost more capability, and at this point she was totally bed ridden and couldn't manage to write with a pen. She eventually communicated by pointing to letters on an alphabet board. We had obtained a computer program to help her communicate, but she never was able to use it effectively. She spent a considerable amount of time after the operation in a nursing home, which I visited each evening. She eventually came home, but couldn't breathe well enough to continue there. She went back to the hospital, and eventually to another nursing home on a hospice program. On March 27th, 1998, she died.

During this period of time, we sold about 5 systems, including the one to France. I was unable to do any of the installation visits, so they were handled by Jawed. RTI had had a considerable number of employees when I first started to work with them while at UTRC, but now it had dwindled to the point where it was only Jawed operating out of a laboratory over his garage. I ran into some trouble with the sale to the university in Rouen, France, in that they could not do a funds transfer to my account. I had started out using the credit union that had an office at UTRC as the bank for my company. While this was convenient for me, they were not a commercial bank and the University of Rouen could not transfer funds to them. I found this out from Prof. Borza, after which I started an account with the same bank we had used for our checking account. When I gave this account number and routing information to them, the university paid my invoice much to my relief. Also during this time period was held the third FRINGE conference. Involved as I was as Elaine's caregiver, I could not even think of attending.

Life goes on

During the period when Elaine was ill, we sold a system to Wright Patterson Air Force Base in Dayton, OH. There were two people with whom I was well acquainted there, Gene Maddux and William Stange, and I know we sold systems to both of their laboratories. I don't remember the order, but I think the one to Bill Stange was first. In September of 1996, as Elaine's illness was starting, I had made a demonstration visit to both the University of Michigan and WPAFB, which resulted in two sales in 1998. I also remember at U of M that Emmett Leith attended our demonstration and commented very favorably about what we had accomplished. I remember that the shipping of the demo system was quite tricky, since I had to make sure it was received in time for me have it available for each demonstration. Anyway, by the end of 1998, I had payments for both of these sales, and Jawed and I had also sold a system to the University of Texas at El Paso. I was very much back in business.

I met my present wife, Arlene Hudson Nordstrom, around the time Elaine died. We became close, and married on January 15th 1999. One of the things than attracted me to her was that she was in business for herself and had had a career in marketing before that. I felt I could learn from her, because marketing was my major problem, and she helped in a number of ways. She insisted that I get a separate telephone line for my fax machine to give the impression that I was more than merely a one man band. She also helped me generate a more professional looking product brochure. My product, however, was aimed at what she described as a "niche market," and it was impossible for her to take any direct role in product sales. She helped in a number of other ways, though. She gave me some of the devices left over from her business, and she also gave me the display booth she had used for her exhibitions. We had new signs made for it, and it provided a much more professional looking exhibition booth than what RTI had had. She insisted on my setting up my business as a Limited Liability Corporation, and switched me over to the CPA she had used for her business. She also took over preparation of the tax documents for him, which essentially relieved me of the tax preparation each year.

During the year of 1999, only one major system was sold, and that was to NASA Lewis in Cleveland, a sale that was handled by Jawed. Dan Borza, in Rouen, also bought a shearography head from me during that year. In June of that year, Arlene and I went to the SEM meeting in Cincinnati, OH, to exhibit and for me to give the Murray medal lecture. The lecture was well received, and eventually published in their journal. [88] Our exhibition, however, resulted in no requests for systems. It was there that I met Andreas Ettemeyer from Germany, who had developed a new very compact unit that in some ways competed with our systems. I recall bidding unsuccessfully against his company for a government contract. His company opened an office in Connecticut, and I had lunch with the young woman who was running it. Eventually, they were absorbed by a company called Dantec. Toward the end of that 1999, I received orders from the GE research center near Schenectady, NY, for the laboratory headed by Kevin Harding, whom I had known when he was at WPAFB. We ultimately sold them three systems, which were delivered the end of that year and early in 2000. I remember being confronted by their safety officer, and being made aware of all the regulations I had not observed in the design of our systems. To comply with this, I designed a shutter that was triggered to block the laser beam from entering the optics head and sound a buzzer when the lid was opened. There was a reset switch to turn the buzzer off and unblock the laser when the lid was closed. The same reset switch would unblock the beam when the lid was open so that optical adjustments could be made.

In July of 1999, I did a demonstration of our holography system at Allied Signal in Phoenix, AZ, and this gave me a chance to visit our customer at Motorola in Tempe and service the system we had sold them. The demonstration did not result in a sale, however. In November 1999, Arlene and I took a trip to Rouen, France, to visit Dan Borza so that I could do a service visit on his unit as well. We rented a car and drove from the airport at Orly, and we stopped on the way for lunch. I remember that Arlene order the mussels in wine, and they were the best we have ever tasted. We did a little sightseeing while we were there, including a drive down the Seine. As is nearly always the case, the food in France was wonderful.

I think that it was in 1999 that Pratt & Whitney moved their holography laboratory from West Palm Beach to East Hartford. They had essentially five systems for holographic vibration analysis, and eventually all of these were upgraded to use our new program by RTI and eventually I replaced all of the optics hardware with our new designs. They ended up with three of our "point and shoot" systems and two free-form systems that used my components mounted on posts. I remember taking Jeff Clarady and his wife to dinner shortly after they had relocated. The safety circuits I had designed proved necessary for their safety regulations as well. During 2001, we sold a system to the Air Force Institute of Technology at WPAFB in Dayton. I shipped this system via UPS as per directions in the AFIT purchase order, and it arrived with damage to two of the plug-in boards in Jawed's computer. He sent replacements via overnight, and I was able to get the system operational, but I discovered that their air suspension table was in contact with one of its supports so that floor vibrations were being transmitted to the table. I left after giving them instructions to get this corrected.

In September 2001, I was scheduled to attend the FRINGE '01 conference in Germany, I had my tickets, I had prepared my paper, [89] and sent the manuscript, so all that was left to do was get on the plane. About a week before the conference, the terrorist attack on the World Trade Center in New York and on the Pentagon occurred on September 11th. This grounded all flights for a period of time that included the conference, so again I could not attend. I did attend the FRINGE conference in 2005, and presented a paper challenging the role of what are called residues in phase unwrapping. [90] In November, 2003, Stephen Benton died and they had a memorial meeting at MIT for him, which I attended. At this point, Charles Vest was president of MIT, and I remember talking with him at this event. This meeting had been planned before Stephen died with the hope that he would be alive to attend it. Many well-known figures from holography attended: Emmett Leith, Yuri Denisuk, Joseph Goodman, and Nicholas Philips, among others.

Another well-known figure in holography, John Caulfield, was editing a book in 2004 dedicated to Emmett Leith and Yuri Denisyuk. He asked me to contribute a chapter to it, which I did describing the current state of electronic holographic interferometry and describing its photographic origin. [91] In December, 2005, Emmett Leith died of a stroke, and some events were planned as a tribute to him. The Journal of Holography and Speckle that December planned a special issue with James Trolinger as guest editor, and the following autumn the Optical Society of America held a symposium at their meeting in Rochester, NY, in honor of him. I was invited to contribute to the JHS special issue, and I and a number of other figures in the history of holography were invited to present papers at the symposium. These included Joseph Goodman, Kenneth Haines, A. Asher Friesem, and Nicholas George. Adolph Lohman was scheduled to attend, but couldn't due to the recent death of his wife, Karla; however, his paper was presented by a colleague. My paper provided a brief history of holographic interferometry, and it was published along with other papers in the Journal of Holography and Speckle that December. [92] In 2006, I did a study on the effects of phase mismatch in our pseudo phase step method for extracting data from time average holograms of vibration modes. [93]

Problems with PCHolo32

Over the period from 2000 to 2004, we sold six systems: to Florida Turbine Technology, in Jupiter, FL, the Air Force Institute of Technology, at WPAFB, Goshen College in Goshen, Indiana, P&W at North Berwick, Maine, the University of Maine in Orono, Maine, and to my old colleague, Pietro Ferraro at INO near Naples, where he now worked. Over this span of time, a number of issues surfaced with the hardware RTI was selling. What had been state of the art ten to fifteen years earlier was now woefully out of date. The plug-in boards were designed for the ISA slots common to computers in the '80s early '90s, but by 2000 these were almost completely replaced by PCI slots. Jawed now had to procure special computer motherboards with these slots. He had never upgraded to PCI slots because these require program development. The TV monitors we used to display the hologram images were increasingly hard to find and cost over four times as much as the new flat-panel computer monitors. Even the original TV cameras we used, made by NEC, were discontinued and we had to replace them with Sony cameras. The RTI system digitized an analog TV signal, but by then TV cameras that did their own digitizing were common. These problems became more and more troublesome over the next two years, during which I sold a system to Belac, a company near Tampa, FL, and to a university in Lebanon via an international agency in New Jersey.

Other problems derived from the holography program itself. When it was converted from DOS to Windows, many of the limitations of the old DOS program were carried over because no real thought was given to what the customer would want to do with the results. For one thing, the image was digitized into 512 by 480 pixels, because the FIFO chips Jawed used had 512^2 elements. The aspect ratio of the TV image was the standard 4x3 ratio, however. Images were stored in a format with the extension *.RTI that was specific to this program. Even by the mid-90s, many image processing programs were available that used TIFF formats, or JPG formats, etc. The PCHolo32 program could store images in TIFF format, but they had to be retrieved from the RTI format and stored again. Furthermore, when viewed by any image processing program, they had to be corrected for aspect ratio. The PCHolo32 program was able to read a lane of data from an image, but you could not save the result. It could perform what was called a "box dump" which read the values inside a box positioned on the image, but the box size could not be varied and you could not save the data read. Also, the program could only run in Windows 97 or Windows ME, and not in Windows NT or Windows XP.

During this time period, Pratt & Whitney in East Hartford wanted to upgrade all their computers to Windows XP. To do this would involve a major program development to rewrite PCHolo32 it so that it would work in the XP environment. To circumvent this, Jawed found a bridge program, obtainable from a company in Germany that would allow programs written for Windows 97 and ME to run in XP. In conjunction with this, I obtained a contract from P&W in September 2004 to pay for an upgrade of the PCHolo32 program to correct as many of its limitations as possible. I used the same programmer who had originally developed the Windows program, Paul Gauthier.

By mid-decade, computers had also made major improvements in speed, and multiple core computers were also common. Jawed proposed that he and a colleague develop a computer program to replace the image processing hardware with software. He was working as a consultant to a company, and one of his colleagues was to do the programming. He initially started on this, but very little progress was made because his colleague got tied up with other work, and I decided I needed to take the matter into my own hands. I had enough money available at this point to pay for this development, so I contracted Paul Gauthier to create the program. He purchased a new computer for me for this project with four internal cores, and I provided him with a Sony digital TV camera. It took us some time, working together, to get this program running, and one of the problems we discovered was that the Sony camera had an internal, patterned, noise function. I checked the digitizing routine in Jawed's original hardware, and I found that it also had this problem. We found a camera from a company in British Columbia, Prosilica, that provided a much better digitized output. We eliminated the old RTI file format in favor of a standard TIFF format. We allowed two other formats, a raw data format with no header or footer information, and a hologram format for time-lapse or static interferometry applications. We also made the program read extensions and store and read images appropriately. I decided to call this program HoloFringe300K. The 300K came from the fact that our images were 640x480 square pixels, which generates 300x1024, or 300K pixels. With square pixels, the images automatically had the correct aspect ratio.

This work started in April 2006 and it took nearly a year to complete. After the basic program was developed, there were numerous additional upgrades to satisfy customer requirements. We eventually developed, under a contract from Jet Propulsion Laboratories (JPL), a high definition version of the program called HoloFringe1200K, which gave images of 1280x960 pixels. Also, in the following years Windows XP was replaced by Windows 7, and the firewire camera that we originally used was replaced with a GigE camera. Also, Prosilica became part of the company Allied Vision Technologies.

HoloFringe300K

The first company to purchase one of my systems with the new HoloFringe program was Innovative Test Solutions, Inc. in Schenectady, NY (ITS). I had demonstrated our system to them a couple of years earlier, and in the autumn of 2007 they ordered one. The major issue for them was vibration testing, and in particular, testing of replacement blades for jet engines which they did for a number of customers. The FAA had issued an advisory circular that specified requirements for certifying engine replacement blades, and these included matching not just vibration modes for the blades but also matching vibration mode shapes. This could be done with a laser vibrometer, but it was actually much cheaper and faster to do it with our holography system. I responded to this requirement by contracting Paul Gauthier to write a program to compute what are called Modal Assurance Criteria. These are essentially normalized correlations of mode shapes. I called this program ModeMatch, and it was set up to do batch calculations to compare a set of modes for an Original Equipment Manufacturer blade to the corresponding modes of a replacement blade. I published a paper describing this in 2008. [94] In the fall of 2007, I sold a system to Jet Propulsion Laboratories in Pasadena, CA. Their system was ultimately used to examine space antennas subject to thermal irradiation.

ITS eventually bought a second system for this work, and we made numerous upgrades to the HoloFringe program to make testing easier for them. Florida Turbine Technology (FTT) upgraded their holography system and also bought the ModeMatch program. Belac also upgraded their system and acquired ModeMatch, and in addition they bought a second system. All of this took place in 2008. In early 2009, GE also upgraded their systems and P&W started to upgrade all of their systems as well. In the autumn of that year, Heico, in Hollywood, FL, upgraded a holography system they had bought years earlier as Jet Avion. In 2010, Chromalloy, who had been a customer of ITS, placed an order for two systems. These were installed in their facility in Newnan, Georgia, and this entailed several trips down there. Eventually both units were installed and operating. In addition to this, I received a request from Georgia Tech. in Atlanta to upgrade their system for projected fringe moiré.

In June, 2007, the Society for Experimental Mechanics held their annual meeting at the Sheraton Hotel in Springfield, MA. Because this was easy driving distance from my house in Coventry, I contracted to exhibit there; however, I booked a room at the hotel rather than commute from home so that I could be more a part of the event. This was my first exhibition of the HoloFringe300K product, and the society invited exhibitors to do presentations in a special session regarding their products. I prepared a short study of deformations due to bolt tightening as studied with our digital holography system. Although many people visited our exhibit, no sales resulted. At this point, my old colleague, Richard Pryputniewicz, organized a conference, the International Symposium to Commemorate the 60th Anniversary of the Invention of Holography, held in Springfield, Massachusetts USA, October 27-29, 2008. It attracted numerous researchers from all over the world; however, there was no exhibition at this conference.

With the Fringe '09 conference coming up, I considered what I might do to generate a paper worth presenting there. I hit upon the idea of upgrading to digital photography the technique I had developed for heterodyne speckle photogrammetry. I had initially been motivated to consider this by a request from P&W back in 2003 to come up with a method for optical strain analysis. My idea was to record speckle photographs digitally rather than photographically. With the photographs in digital form, it would be possible to calculate Fourier transforms of them and subtract the phase functions of these transforms and wrap the results into 8 bits as we did with our static phase interferograms. The slope of this phase function was a direct measurement of the speckle displacement between the two images. By dividing the images into sectors, and doing this calculation for neighboring sectors, it was possible to get strain values. This required observation of the surface at nearly normal incidence.

I had engaged my brother-in-law, Mark Rosenthal, to do computer modeling of this process, assuming 12 bit digitization of the images, and the results indicated very good strain resolution. I purchased a digital camera for this project, and contracted Mark to develop a prototype program for this. I brought my laser and the necessary equipment up to his house in Arlington, MA, to do experimental tests, but, in the end, these tests did not pan out. What I had neglected to consider was the limitations of the solid-state detectors due to shot noise. This had sat in limbo since then, and in early 2009 I decided to explore it experimentally myself. One of the key features was that the subtracted phase functions would give wrapped phase functions which had to be unwrapped in order to calculate the slope of the phase function. Doing this myself, I could use the phase unwrapping program that was part of our HoloFringe program. I obtained a program called DADiSP which allowed me to do Fourier transforms, and with this I was able to do an extensive study of this process and determine its practical limits.

I submitted the paper to Fringe '09, and it was accepted as a poster session paper. Considering that it was, I felt, a more original piece of work that much of which was usually presented at Fringe conferences, I felt this was a rather lukewarm reception. I struggled to fit the paper into the page limit for the proceedings, but when the materials for the poster came, I saw that I could not present even less of the paper there than I could in the proceedings. I reluctantly decided to forego attending that conference, and actually ended up at Heico during the conference upgrading their holography system. The paper was published it is abbreviated form in their proceedings. [95] I submitted the paper to Applied Optics and got a rejection. One reviewer claimed that the process was really no different that digital image correlation (DIC), which was widely used for optical strain measurement, an assertion that was patently false. Both reviewers claimed that the paper would have no impact on the research of others in the field, a comment that should have been rejected by the topical editor, but which was not. I revised the paper, added a comparison to a DIC calculation using the same data as one of my examples. This was also rejected, so I left the matter alone until the SEM conference at the Mohegan Sun in 2011. I submitted the paper to this conference and presented it in one of their sessions. I submitted it to their journal, Experimental Mechanics, and it was finally published. [96]

Early in 2010, I received a request from Greg Weaver at Vibrant Corporation in Albequerque, NM, to analyze ten turbine blades for their vibration modes. The analysis had to be done, however, with what they called a free mounting fixture. This consisted of three points on which the blade sat, with a removable fixture with three rods against which the blade could be positioned to assure its proper location. One of the three points on which the blade sat was a piezo-electric driver. I had a fixture for this made up and tested it, the results of which indicated that we needed to cement the blade to the driver in order to avoid movement of the blade due to vibration and nonlinear response do to loss of contact during part of the vibration cycle. Our setup was quite successful, and we published this work in the SEM journal Experimental Techniques. [97]

Toward the end of 2010, I received a request for a quotation from the Western Carolina University in Cullowhee, North Carolina. This was initiated by Ken Nelson from Florida Turbine Technology, who was in close contact with people at the university. They requested that I do a demonstration there in conjunction with an event they were having to mark the opening of a laboratory for turbine engine research in cooperation with the Brazilian company Vale. I shipped our demonstration unit down there and made the trip, flying into Asheville, NC. They supplied a computer for operating our program, and I managed to get our system set up and operating in plenty of time for their demonstration. What I didn't quite realize was that they were having an open house for the laboratory, and I was expected to explain our system to visitors and upper echelon from the university. This required "thinking on my feet." I remember explaining that a colleague and I had discovered that holograms could be used to perform interferometry on three-dimensional objects back in 1964 when holograms were made on photographic film. Now the same technology that had put a TV camera in every cell phone made it possible to record holograms electronically, and that was what I was demonstrating. The demonstration went well, and I received an order from the US division of TAO Sustainable Power Solutions. I had occasion to telephone the contact I had from them in Texas, and was surprised to get what sounded like a personal answering machine. This rather upset me, and I contacted Ken Nelson and told him that I would require advanced payment of half the purchase price in order to accept the contract. We eventually settled on prepayment for the laser and a final payment upon receipt of the system. For that reason, I shipped the final unit to the FedEx office in Asheville and picked it up to transport it personally to WCU in the spring of 2011. The installation went okay, and I received payment that day. A couple of years later, however, I got a contact from someone connected with that company informing me that the cooperative program with the University had ceased and they were wondering if I would be interested in buying the system back. At that time, unfortunately, I had no orders and could not afford to do that.

During the period from 2007 to 2011, the holography units in the engineering department of Pratt and Whitney were upgraded to the HoloFringe program. In the spring of 2011, the unit P&W used for abradable seal testing was upgraded, and this involved refurbishing the optical head as well as providing a new computer and program. Because they were accustomed to printing their images and fastening them to the part tested, we eventually provided a printer to be connected to the computer and modified the HF300K program to allow image printing. Eventually, we had to replace their old Coherent laser with a newer one made by Crystalaser. This laser eventually showed problems, and had to be replaced with one from a company in Sweden called Cobolt. The Crystal unit was repaired and provides a spare laser for their unit.

In 2011, I was invited to present a paper at the meeting of the Acoustical Society of America in Seattle, WA, in May. I had done some studies of the vibration modes of clarinet reeds (my first musical instrument) so I expanded this work to a more detailed study. This involved considerable research for prior work in this area, and I located a paper by researchers in France, whose results I felt my studies contradicted. My presentation was well received, and the next morning as I left the hotel, I stumbled at the edge of the sidewalk and fell on my face. This badly cut my lower lip and actually broke the bones comprising my left eye socket. The hotel staff persuaded me to go to the local hospital where they did an MRI and sewed up my cut lip. To top this off, I discovered that I had made a mistake regarding the flight time of my return flight. I was scheduled to return that day at 12:30 AM not PM. The airline was able to reschedule my flight to that evening, and I returned early the next morning with a severe black eye. This necessitated a considerable round of doctor's visits, where it was finally determined that the best thing to do regarding my eye socket was nothing. Operating on it to repair things would risk damage to the optic nerve and could lose me my sight in that eye. Fortunately, everything healed okay, and my sight has remained unchanged.

I had been scheduled to exhibit at the 2011 SEM meeting which was being held at the Mohegan Sun Casino in Uncasville, CT, at their June meeting. My old colleague, Rich Pryputniewicz, was now serving as president of the SEM. I was required to do no heavy lifting because of my injury, so I had to schedule help loading and unloading my equipment. An old colleague, Cesar Sciammarella, was teaching at the University of Bari, Italy, as a guest lecturer, and he had seen our unit at the SEM show in 2007 at Springfield, and had recommended it to his colleagues in Bari, Italy. The people from Bari were there at the SEM meeting and had the opportunity to view our equipment in operation. This resulted in an order for two systems in 2012, a holography system and a shearography system. This was rather a complicated transaction, however. They were given the go ahead for the purchase in September 2011. We had some initial back and forth regarding payment, as I was requesting an initial payment of 50% with the placement of the purchase order. This could not be accepted under their government regulations, and we agree to terms amounting to 50% at delivery, 40% upon installation, and 10% upon first successful demonstration of operation. The next issue was the requirement of a guarantee deposit which we were supposed to make, which would be refunded upon the end of the contract. I objected to this in that I was taking the risk by spending the money to purchase components with which to construct these systems. They finally waived this in lieu of a 1% discount on the price, which I was able to realize by re-quoting the systems with an increased price. Next, they said that they needed something tangible delivered in lieu of the complete order so that they would not lose the money that had been set aside for this purchase. I complied with this by shipping them the frequency generator, an item worth $1000. They, in turn, paid me the total amount of the system.

At this point, I continued with production and shipped them the rest of the equipment. This caused a major problem, because I had declared the value of the equipment shipped at replacement cost to me. Their customs people required a VAT payment from the university, which they had already paid. Their solution to this was to have the entire shipment returned to me. I eventually had to reship the equipment without a proper value declaration. To cover myself, I took out an insurance policy to cover the potential loss I would suffer should the equipment get lost in transit. Anyway, in the fall of 2012, I made a trip to Bari to do the installation and training. I got rather poor directions from the rental car place at the Bari airport, and ended up going through the city itself rather than going around it as I should have. I eventually found my way to my hotel, settled in, and spent several days at the university. From there, I drove across Italy to Napoli and spent a day upgrading the system at Pietro's laboratory. From there I drove to Rome to catch my plane back to the US. I had managed to get roaming authorized for my cell phone, so I could stay in contact with Arlene back it CT.

At the end of 2012, I was contracted by P&W to upgrade a holography system in the laboratory of Wesley Larkin. Their system had been installed nearly 20 years earlier, so it was quite out of date. To do this, I simply delivered my demonstration system, with a new computer and a some optics from their old system for illumination beam shaping. Early in 2013, I was requested to quote for an upgrade the system we had sold to Goshen College. For this, Prof. John Buschert requested that we provide an external D/A converter unit rather than the plug-in unit that we normally used. Fortunately, our supplier had such a unit available that could be adapted to our application. The trip to Goshen went well, except that he had insisted that they provide the computer, and this resulted in a very serious installation problem that would have been totally eliminated had I had the computer in my laboratory before shipment.

Prof. Buschert had done work analyzing the vibrations of bells, and this gave me an idea for an extension of our system capabilities. Vibration modes of cylindrical shells are two-dimensional, and interest had been expressed at P&W in the past about having the ability to record two-dimensional vibrations of turbine blades. Vibrational displacement along the length of a turbine blade can mostly be ignored, but their twist and camber make one-dimensional analysis rather incomplete. I worked out an arrangement of mirrors that allowed me to direct my object illumination so as to illuminate an object either from the right or from the left at an angle of 38 degrees to the direction of observation. With our routine for recording vibration mode data, it was possible to record the vibration data first with one illumination and then by the other, all the while maintaining the same vibration level on the object. The data from both recordings could then be processed and combined to give both the displacement in the direction of observation or at right angles to that direction. The object used for this experiment was a small wine glass mounted on the same fixture that I had used for the pseudo free mounted blade analysis. Lane data read near the rim of the glass was resolved into vibration magnitude and angle of orientation. This was published in the SEM journal Experimental Techniques. [98]

I was looking now at the possibility of attending the Fringe '13 conference in Stuttgart, and I had to do something to prepare a paper for it. It occurred to me that I had never really demonstrated the capabilities of our projected fringe technology, especially regarding vibration analysis. Our program had been provided with a calibration routine so that we could set the spatial frequency of the projected fringes. I had generated a mathematical pattern consisting of 128 cycles of sinusoidal irradiance across the field of view. This could be loaded into the projected fringe moiré routine of our program and used as a reference against which to adjust the frequency of the projected fringes. Once this was done, and the angle of projection measured, the object displacements could be measured from the data recorded. To test this for vibration, I made what was essentially a wide tuning fork from a sheet of tempered steel. I was able to excite this into its fundamental vibration at 155 Hz at an amplitude adequate for our holographic processing. In addition, I wedged a spacer between the two tines to create a static displacement. My paper demonstrated both vibratory and static deformation measurement with a range in the neighborhood of 1mm. This was presented at Fringe '13 as a poster paper, and this time the poster format allowed me to present the entire paper.

The flight to Stuttgart, where Wolfgang Osten had been a professor for over a decade, was uneventful. Wolfgang met me and another attendee at the airport and drove us to the hotel in the nearby city of Nürtingen. Our rooms were not available that early, and so the two of us strolled around the town and got some breakfast while Wolfgang left us to attend to conference business. I spent a fair bit of time with Jim Trolinger at the conference, and I sat beside him at the dinner on the first night at an old castle where we were bussed. There was an award ceremony where a number of us were designated as members of "the Fringe Community," and I was distinguished with the title of "first member of the Fringe Community." This included a framed document to that effect, which hangs in my guest room. The conference was interesting. Wolfgang had requested that I send by poster as a PowerPoint format that they could print the poster there to save me the trouble of packing it or shipping it. One of the events of the conference was a visit to a castle in a city about a half hour drive from Nürtingen. We had a tour of the castle and then met at a restaurant for the conference banquet. There was the traditional ceremony of crowning a new "Holoknight," who then had to make a speech. All in all, we didn't get dinner until about 10 PM, and I didn't get to bed until about 1:00 in the morning. While I was at the conference, I learned that both my old friend, Nils-Erik Molin, and my old colleague, Rich Pryputniewicz, had both had heart trouble. I felt quite fortunate that I had managed to avoid that complication of aging so far. While I was away, some pipes started leaking back at my house in Coventry, and Arlene had to deal with that on her own. When I got back, we had the old pipes replaced with plastic ones that would never be leached out by the slightly acid water of our shallow well.

The Present

At this point business is in a lull, as it has been well over a year since I have received an order for a system. I have had requests for a quotations from the Breedlove Guitar Company in Oregon, and from Pontifícia Universidade Católica do Rio de Janeiro, but I am not sure if either will result in an order. This year I have sold two piezo-electric mirror assemblies. I was invited, while at the Fringe conference in Nürtingen, to be the keynote speaker at a conference in the Czech Republic in the city of Liberec this last October, 2014. This gave me the chance to see the city of Prague, which I have never visited, and Arlene accompanied me on this trip. We had several days sightseeing in Prague and strolled around Liberec as well. Unfortunately she became ill while in Liberec, and this required some serious treatment after we got back. The main company sponsoring this was Toptec, located in Turnov, and while visiting them the day after the conference, I learned that they have a strong need for vibration measurement. My contact had read my paper in JOSA:A[81] from 1988, and I explained that we had made great strides in that area since then. I was able to give him the documentation file for the HoloFringe300K program so he could understand what our system could do. It may be that they will order a system from us in the coming year.

At present, the major activity in holography is in digital holography. Fifty years ago all you needed to get into holography was a laser and some 649F photographic plates, now all you need is a laser, a digital camera, and a computer. A search of Amazon's website shows twenty books on the subject of digital holography:

1. Digital Holography Hardcover, March 5, 2012, by Pascal Picart, Jun-chang Li, (Wiley)

2. Introduction to Modern Digital Holography with Matlab, March 24, 2014, Ting-Chung Poon and Yung-Ping Liu, (Cambridge Press)

3. Digital Holography and Three-Dimensional Display: Principles and Applications, Oct 29, 2010, by Ting-Chung Poon, (Springer)

4. Digital Holography and Digital Image Processing: Principles, Methods, Algorithms Nov 30, 2003, by Leonid Yaroslavsky, (Springer)

5. Digital Holography: Digital Hologram Recording, Numerical Reconstruction, and Related TechniquesNov 5, 2010, by Ulf Schnars and Werner Jüptner, (Springer)

6. Digital Holography for MEMS and Microsystem Metrology, Aug 15, 2011, by Anand Asundi, (Wiley)

7. Digital Holography and Wavefront Sensing: Principles, Techniques and Applications Jul 24, 2014, by Ulf Schnars and Claas Falldorf, (Springer)

8. Digital Holography: Digital Hologram Recording, Numerical Reconstruction, and Related Techniques Nov 29, 2004, by Ulf Schnars and Werner Jüptner, (Springer)

9. Methods of Digital Holography, Oct 22, 2013, by Leonid Yaroslavskii, (Springer)

10. Digital Holography Microscopy applications: Three Dimensional Object Analysis and Tracking, Apr 1, 2009, by Cedric Schockaert, (VDM Verlag)

11. Digital Holography and Interferometric Metrology of Optical Fibres: Digital Holographic Phase Shifting and Interferometric Characterization of Optical Fibers, Apr 21, 2011, by Hamdy Wahba, (VDM Verlag)

12. New Techniques in Digital Holography (Iste) Apr 6, 2015, by Pascal Picart, (Wiley)

13. Digital Holography, 2000, by Rodney M. Powell, (PN 2000)

14. Improving reconstructions of digital holograms: Speckle reduction and occlusions in digital holography, May 13, 2014, by Jonathan Maycock and Bryan Hennelly, (LAP Lambert)

15. Digital Holography, by Schnars, Ulf, Jüptner, Werner, (Springer,2004), Oct 4, 2004

16. Digital Holography Microscopy applications: Three Dimensional Object Analysis and Tracking, by Schockaert, Cedric, Jan 1, 2001 (VDM Verlag)

17. Scientific and Industrial Applications of Digital Holography, Feb 4, 2013, by Gyanendra Sheoran, (LAP Lambert)

18. Image processing for digital holography: Specifically digital holographic microscopy, Jun 17, 2011, by Karen Molony, (VDM Verlag)

19. Digital Holographic Microscopy: Principles, Techniques, and Applications, Aug 12, 2011, by Myung K. Kim, (Springer)

20. Introduction to Modern Digital Holography: With Matlab by Poon, Ting-Chung, Liu, Jung-Ping, 2014, (Springer)

It seems to me that the major applications for digital holography is it is being pursued are in microscopy, because digital holograms offer the advantage that images can be refocused after they are recorded. For industrial applications, I think that image-plane, phase-stepped digital holography still offers the simplest and easiest way to get static or vibratory displacement data.

As of this year, I have spent as many years with my own business as I spent working at United Technologies. I left UTRC a month before I would have been there twenty-one years, and that was my longest term of employment. How do I feel about having spent the last twenty-one years with my own business? It has been very interesting and satisfying in many ways, despite the loss of my first wife and the problems of growing older. My business has made money, which was the objective, but it also gave me a level of freedom that I never enjoyed before. My time was essentially my own to spend as I wished, so long as I produced on time the products I needed to sell. It gave me the opportunity to spend my days in my own home working on things I wanted to work on. I do wish that my business had developed into something larger, and I regret that it will probably die when I leave it because I have no one to pass it on to.

Another thing I regret is that holographic interferometry has not been recognized more by the Optical Society of America. I was just twenty-seven fifty years ago when I discovered it with Robert Powell, and I always felt that I should have received the Adolph Lomb Medal, which is awarded to persons who have made accomplishments in Optics at an early age, i.e., before 35. I have received awards from the Society for Experimental Mechanics, the Hetenyi award and the Murray Medal, but no recognition from the Optical Society for the revolution Powell and I created in the field of interferometry. Perhaps that is because the applications of holographic interferometry are more it the field of mechanics than in optics per se. It may also be because I have not been active in committees in the OSA.

Another thing that I occasionally think about is what might have happened had I been able to join a university back in 1971 instead of rejoining industry. Surely, my life would have taken different turns. It might also have been interesting had I been able to join a government research laboratory. Having joined an industrial laboratory, there is also the possibility that I might have left UTRC and sought to join the General Electric research laboratory in Niskayuna, NY. At least one member of the UTRC staff that I know did that, and I believe he is happy with his situation there. But all this is speculation – one can never know what one would have been found on that the road not taken.

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