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Anecdotes about the Early Days of X-Ray Optics[1]

Albert V. Baez

Vivamos Mejor, Greenbrae, California

An anecdotal description of the trials and tribulations encountered by the first investigators in the field of x-ray optics starting in the late 1940s and how they managed to overcome them is provided. Some of the players, in addition to the author, included Paul Kirkpatrick, Hussein El Sum, and Howard Pattee of Stanford. At the University of Redlands we became interested in producing an x-ray microscope based on the concepts of holography which Dennis Gabor had demonstrated. This led to correspondence with Gabor and the opportunity to meet him and many other investigators at the first International Conference on X-Ray Microscopy and Microradiography held in Cambridge, England, in 1956. With the help of V.E. Cosslett and William Nixon, a point-focus x-ray tube was obtained by the University of Redlands for its experiments in x-ray holography in the 1950s.

INTRODUCTION

Much of what I have to say here appeared as a guest editorial in the first volume of the *Journal of X-Ray Science and Technology* in 1989, but I have tried to make this presentation different by placing more emphasis on anecdotes.

Except for an experiment on in-line holography which I performed in 1990, I have not done any research since the 1960s. But I was fortunate to have been a student of Paul Kirkpatrick at Stanford in the 1940s when he proposed a means of producing optical images with x rays using grazing incidence reflection. I thus got in on the ground floor of a field of research which has since boomed. The title of my dissertation was *Principles of X-Ray Optics and the Development of a Single Stage X-Ray Microscope*. It is quite possible that this was the first time the term ''X-Ray Optics'' appeared in print.

I was attracted to the International Conference because it presented an opportunity for me to learn what is going on today in a field of research with which I am now out of touch, but in which I was deeply involved in the pioneering days, and which now seems to be ready to take off in the 21st century.

What I have to offer is a glimpse of how research was done in the very difficult period right after the war when resources were very limited and the pent-up energies of university scientists were ready to be deployed on research. My objective is not so much to inform with details of research but rather to entertain with anecdotes about the early days of x-ray optics in an appropriate manner.

[1] This invited paper was presented at the plenary session of the *International Conference on Soft X-Rays in the 21st Century* held in Midway, Utah, January 8–11, 1997.

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TEACHING PHYSICS—WAGNER COLLEGE AND STANFORD UNIVERSITY

My research began at Stanford University in 1946—but first let me give you an anecdotal description of how I got there. During the 1940s there was a great demand for individuals who could teach college-level physics in a war-related program called ASTP—Army Specialized Training Program. I did not have a Ph.D. but I did have several years of experience teaching college math and physics in a small liberal arts college—Wagner College of Staten Island, New York. I started teaching calculus and other math courses, but before my first semester was over, the president called me in for an interview. ‘‘Can you teach physics?’’ he asked. I said ‘‘yes.’’ It was between semesters and his only physics professor was going to be dismissed. ‘‘Well,’’ said the president, ‘‘you will start teaching physics in a couple of weeks, at the end of the first semester.’’

During the next four years something happened which would be absolutely impossible today. I was promoted every year, starting with the rank of instructor, through assistant professor and associate professor, and finally to tenured professor of physics and all I had was a master’s degree in mathematics.

My brother-in-law Bob Bridge came to visit us in Staten Island in 1944. ‘‘What are you doing here?’’ he asked. ‘‘They need you at Stanford.’’ He was a premedical student there and had taken his required physics course under Professor Paul Kirkpatrick, who, as the senior professor, was also acting head of the Stanford physics department while all the other physics professors were off in places like Los Alamos building bombs or at the Radiation Lab in Cambridge developing radar.

When Bob went back to Stanford he told Kirkpatrick that I was the best physics instructor in the world and that Stanford should hire me to help with the ASTP program. There is a human drama here which I don’t have time to relate, but I was offered a teaching position at Stanford with no guarantee that I would be permitted to do graduate work. To make the transition we sold our home on Staten Island, obtained gas-rationing coupons to drive across the continent, started in the middle of a snowstorm in Staten Island right after Christmas, and headed for California in an old Ford V-8 with my wife, her sister, and two children. Many years later an article referred to me as ‘‘a fugitive from tenure.’’

RESEARCH AT STANFORD IN THE 1940s

Kirkpatrick and I shared an affection for teaching. We loved to invent spectacular demonstrations for the physics classes. Soon the war was over and the other physics professors began returning to Stanford. They were all anxious to start research again. Felix Bloch wanted to get going on nuclear magnetic resonance—for which he later won a Nobel Prize—and Bill Hansen wanted to build a two-mile-long linear electron accelerator. Paul Kirkpatrick, who had stayed on campus to direct the department, was also anxious to get back to his x-ray research. Professor Bradbury stayed on at Los Alamos as its director after Oppenheimer left.

I began taking courses toward a Ph.D. in physics. One day Kirkpatrick told me he had an idea of how to build an x-ray microscope and asked me if I would be interested in working as his graduate student. I jumped at the chance to work on research with a person whom I had come to admire as a teacher.

I learned the basics of x rays under Kirkpatrick in a department which had achieved recognition for x-ray research under investigators like D. L. Webster. Kirkpatrick explained to me that although x rays cannot be reflected when they approach a surface at normal incidence, Compton had already observed that x rays could be reflected at grazing incidence, that is, at angles of a few degrees from a tangent to the surface. But he did not focus the x rays.

Incidentally, the going rate of pay for research fellows at Stanford in the 1940s was 35 cents an hour. I supplemented this income by teaching a calculus course in the math department. But we had three children and my wife, now called Joan Baez Sr., helped me financially by running a boarding house near Stanford. Fifty years later, she published a book about it entitled *Fund Raiser for a Ph.D.*

The war had devastated the department's financial resources for research. I remember that when we started building equipment for our experiments Kirkpatrick said to me, ‘‘Al, I want you to go to the scrap metal pile in the machine shop and see what pieces we can utilize in our preliminary experiments.’’

My earliest experiment was to observe, with dark-adapted eyes, x rays landing on a fluorescent screen after they had bounced, at grazing incidence, from a flat glass plate. I had built a device that could apply torques to the plate to bend it and thus change its cross section from a straight line to a circle of very large radius. The reflected beam changed from a rectangular shape to a straight line after reflection. I was observing, in effect, how changing the curvature of a mirror enables it to produce focusing.

I felt a sense of exhilaration on seeing the focusing taking place on the fluorescent screen as I turned the knob which controlled the simple bending mechanism. I thought at the time that I was possibly the first person to have actually observed x rays being focused by a curved mirror. I know now that others had probably observed this before me. But the thought that my experiments might lead to an x-ray microscope, which as yet no one had built, was exciting. I began to wonder what one might be able to see with an x-ray microscope that could not be seen with an ordinary microscope. I imagined the thrill that Galileo experienced while exploring the heavens with a light telescope. Little did I know then that my research would eventually lead to the construction of x-ray telescopes.

NEW LIGHT—NEW KNOWLEDGE

A student once asked what were the advantages of an x-ray microscope. I said that ordinary microscopes work with visible light ranging from red to violet in the wavelength range of roughly 4000 to 7000 Å. I explained that this was only a small part of the total electromagnetic spectrum but that *information* could also be carried by invisible light such as ultraviolet and infrared and that x rays were part of this invisible spectrum way beyond the ultraviolet down to x rays with wavelengths as low as 100, 10, or even 1 Å. I began to use the expression ‘‘new light—new knowledge.’’

One day I invited students to a party at my house where I had brought the lab's ultraviolet source. My plan was to turn out the room lights and observe what you could see when you shone the invisible ultraviolet radiation on people's teeth, fingernails, and eyeballs. The students got a big kick out of seeing how these and other objects

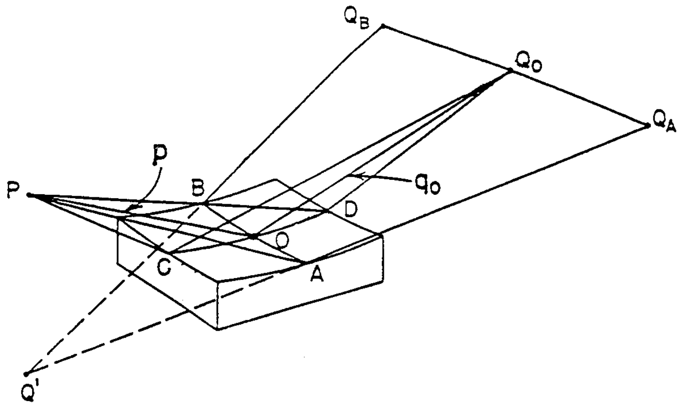

FIGURE 1

fluoresced under the black light called ultraviolet. One student showed a gaping black hole where a tooth seemed to be missing. When we turned on the room light, we saw there was a tooth where the gaping hole had been. We had discovered that the student had a false tooth that did not fluoresce under the ultraviolet light. I exclaimed, ‘‘New light—new knowledge!’’

Suddenly a young woman guest told us that she had recently become engaged and was wearing her engagement ring. She had heard that diamonds fluoresce under violet light and wanted to see what her ring looked like under this ‘‘black light.’’ Once again we turned off the room lights. She put her hand with the ring on it under the ultraviolet light. It did not fluoresce! New light—new knowledge!

X rays belong to the invisible range of electromagnetic waves which can carry information different from that which is carried by visible light. This is, in fact, the basic reason for going to soft x rays in the 21st century to explore their particular properties.

CROSSED MIRRORS: KIRKPATRICK’S CONTRIBUTION

Kirkpatrick’s main contribution to the art of x-ray image formation using mirrors was the introduction of a second mirror. Figure 1 shows schematically what a single curved mirror can do to the beam that emanates from a point x-ray source. By choosing the right curvature a single mirror can squeeze the reflected beam down from a rectangle into a single line.

Kirkpatrick suggested that if a second mirror were placed in the beam at right angles to the first, the line could be squeezed down to a point as shown in Fig. 2. This figure is instructive because it shows that some of the radiation from a point source misses both mirrors and lands at spot d. Some of it is focused by the first mirror only and lands as a horizontal straight line segment at h. Some of the radiation hits only the vertical mirror and lands as a vertical straight line at v. The radiation that hits both mirrors converges to a point at hv. In other words, radiation that emanates from a point in the object plane lands at a corresponding point in the image plane.

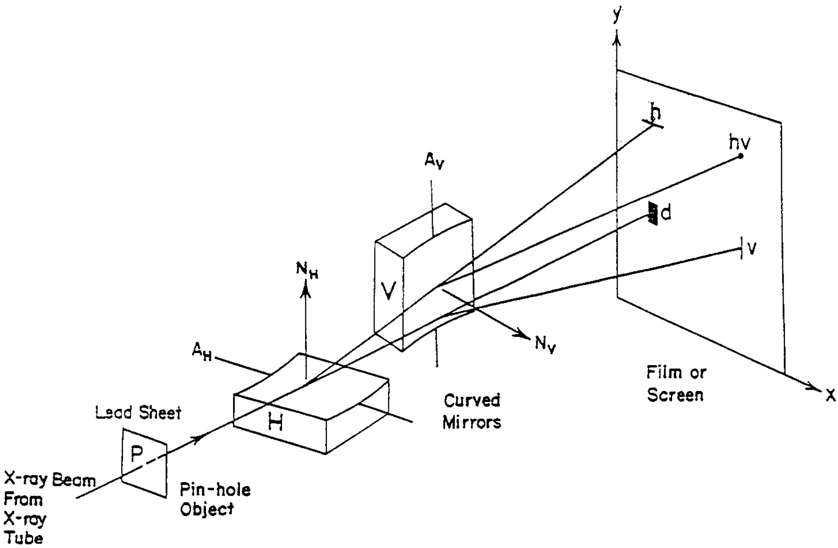

FIGURE 2

An extended object, which consists of a manifold of points in the object plane, is focused as an extended image in the image plane. This one-to-one correspondence between points in the object plane and points in the image plane is what makes it an image-forming device. If the parameters of distance are chosen properly, with an image distance that is larger that the object distance, magnification takes place. The resultant combination is an x-ray microscope.

Some time after we had achieved success in forming x-ray images in this way, Kirkpatrick was asked to give a presentation at the Stanford physics department's Journal Club, as it was then called. Using visible light to illustrate the idea of crossed optical elements, Kirkpatrick first used a pin-hole as an object and a single cylindrical lens as a focusing device. When the object is a single point source a cylindrical lens produces a single line as an image. He showed this on a large screen.

Kirkpatrick then used a rectangular mesh as an object and at first, using only a single cylindrical lens, he obtained a set of parallel lines as an image of the mesh. These were projected onto the screen. Next, he predicted that when he introduced a second cylindrical lens into the beam at right angles to the first he would get a focused image of the entire mesh. There was a murmur of disbelief among the physicists in the audience—which included at least one future Nobel Prize winner. Kirkpatrick paused and then put the second cylindrical lens into the beam. This time the rays that got through both lenses formed a perfect and enlarged image of the wire mesh, proving that these two cylindrical lens elements placed at right angles to each other behave like a single convex lens. The audience broke out into applause. By using a simple demonstration, he had proved that crossed optical elements behave like a single ordinary lens. He became a role model for me as a teacher.

The original copy of my dissertation once weathered a severe storm on the Boardwalk where I live. If you were to thumb through it you would be reminded of what it took in the 1940s, long before the advent of the word processor, to type up a dissertation. The diagrams were all drawn by hand. A secretary had to type every word without errors. The original is in the Stanford library; even my copy is a carbon copy. The drawings were drawn for me by Howard Pattee, who also got a Ph.D. under Kirkpatrick. He was later co-author, along with V. E. Cosslet and Arne Engstrom, of the book *X -Ray Microscopy and Microradiography,* which contained the proceedings of the first symposium of its kind, held in the Cavendish Laboratory at Cambridge, England, in 1956.

Many investigators presented papers there in what is now called x-ray optics. Dennis Gabor, the inventor of holography, was a distinguished visitor who had many opportunities to discuss the potential of an x-ray microscope based upon holography with Paul Kirkpatrick. Gabor had already written that if a hologram is produced with radiation of wavelength L1 and reconstructed with a wavelength L2 there would be a magnification of magnitude L2/L1. This opened the possibility of using an x-ray wavelength for L1 and a visible light wavelength for L2. The ratio L2/L1 could easily be 500 and possibly as high as 5000. These magnification figures made the prospects of an x-ray microscope based upon holography seem quite exciting.

I, too, became excited about the possibility of making an in-line x-ray hologram. I figured that if a point source of x rays were available, it would be interesting to try to make an x-ray hologram with it. Soon thereafter we learned that Kirkpatrick had assigned another graduate student, Hussein El Sum, to explore this possibility for his doctroral thesis. El Sum began by repeating Gabor's experiments using visible light in order to consider the possibility of using x rays later. El Sum's dissertation was one of the first to be put onto microfiche and thus became available to many students of holography. It was so widely read that some called it the ''bible'' of experimental holography.

RESEARCH WITH UNDERGRADUATES AT THE UNIVERSITY OF REDLANDS

After getting my Ph.D. at Stanford I went to teach at a small university with no track record in research—the University of Redlands in Southern California. They gave me a reduced teaching load so that I might introduce research done by undergraduates. This was an idea that appealed to the Research Corporation, and I received some financial support from them to get started.

Kirkpatrick had suggested to me that in-line holography with visible light was an ideal topic for research with undergraduates because the equipment was inexpensive and readily available. I figured that if they learned to make holograms with light they would be prepared to extend this knowledge to do similar research with x rays—if and when we were able to find a suitable point source of x rays. Except for a zirconium arc, some interference filters, and a supply of very fine grain spectroscopic plates, everything else we needed, including optical benches, was already available in our undergraduate optics laboratory.

So, my students and I were soon making in-line holograms and reconstructions 10 years before the invention of the laser. Some of these holograms, we thought, were better than those which Gabor had published. The Research Corporation grant enabled me to give summer jobs to a team of about five undergraduates. We worked as a team not unlike the teams of graduate students which Kirkpatrick had formed at Stanford. This confirmed my hunch that students should be given the chance to perform real research before entering graduate school. On the basis of this experience, the Research Corporation supported the work of Chemistry Professor Robert Maybury, who came to Redlands because of my activities in research with undergraduates.

We had heard that Cosslett and Nixon at Cambridge University in England had built a tube which was essentially an x-ray source with a diameter of 1 μm. They were using it to produce microradiographs of unprecedented resolution and clarity. We wanted to use it as a source for in-line x-ray holography. I went to visit them in England to explain our need for such an x-ray source. They said they would be willing to build us such a tube for $1000.

I then went to the National Research Council in Washington, D.C. They seemed interested and asked me to write a proposal. I explained my idea that, after learning how to make in-line holograms with visible light, we would be prepared to try our luck with x rays. What we proposed to do was to make an in-line x-ray hologram with the Cosslett–Nixon tube and make reconstructions with visible light, thus making Gabor's dream of a holographic x-ray microscope come true.

Before long my team of undergraduates understood holography so well that one morning, when a representative of the National Research Council arrived to visit us to see if they should pay for a Cosslett–Nixon tube, I happened to be teaching a class. So my student Don Robinson met him, took him in hand, and explained holography so clearly to him that he was impressed and went back to Washington to say, ''Give Baez the money he needs to buy the tube. If an undergraduate can explain holography that clearly to me they must be doing something right.'' We got the money. They also gave me enough to bring Nixon to spend a semester at Redlands to help us set up the Cosslett–Nixon point focus x-ray tube. Our success eventually prompted the American Cancer Society and the Office of Naval Research to give us grants.

Nixon and I hit it off very well. Having come from the cold and damp climate of England, he loved California's sunny weather. So our families took many opportunities to travel together to nearby mountains, deserts, and the ocean. We were in great demand at nearby universities to give lectures on x-ray optics. We even appeared on television. Our families referred to us as ''the Laurel and Hardy of X-Ray Microscopy.''

When W.C. Nixon failed to obtain more than one diffraction fringe in a projection radiograph which was, in effect, a hologram of a silver grid, we were forced to think about, and better understand, the meaning and importance of both temporal and spatial coherence. To be honest, if we had fully understood those concepts then, we might not have embarked on an attempt to make a hologram with a tube whose focal spot diameter was as large as 1 μm. But then we would have missed out on the fun of using the Cosslett–Nixon tube to make spectacular microradiographs.

CONCLUSION

I will end my reminiscence here for lack of time. Still to come would be the Cambridge Conference on X-Ray Microscopy and Microradiography in 1956 where the research at Redlands done by El-Sum, Baez, and Zeitz were reported and where I met Dennis Gabor for the first time. I became a correspondent of his, and it turned out that he had been a referee who approved of the publication of my paper on the construction of a zone plate for x rays which appeared in the journal *Nature*. He mentions my work on x-ray holography in his Nobel speech. I have a sad handwritten note from him shortly before his death saying he was too weak to respond to my letter.

Between teaching summer school courses at Harvard, I visited Professor Whipple, director of the Smithsonian Astrophysical Observatory, and explained to him my idea for the construction of a Fresnel zone plate that could focus x rays. He gave me a small grant that permitted me to build the first such device as well as the first prototype of a high throughput multiple mirror x-ray telescope. There are still many anecdotes to tell but there is not enough time. The samples I have given will suffice to give you the flavor of my experiences.

Incidentally, I eventually also gave up my tenured position at Redlands to join the staff of Unesco in Paris to direct its activities in science education. So I was, indeed, a fugitive from tenure twice.

MY LAST EXPERIMENT

After a hiatus of many years out of the laboratory, I got an idea for an experiment to prove experimentally the three-dimensional nature of in-line holography. Although it was performed with visible light, it may have implications for soft x-ray image formation. I performed it with Dr. George Castro of the IBM Almaden laboratory.