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MicroscopyPioneers

Aberrations, a Way of Life

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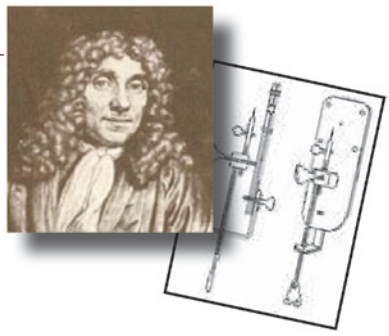

Editor's note: Peter Hawkes received the 2015 Distinguished Scientist Award (Physical Sciences) from the Microscopy Society of America. Dr. Hawkes was unable to attend the awards ceremony or his invited presentation; he sent this text to be read at the time he was to speak.

There is a certain ambiguity about "aberrations." In the days of routine requests for offprints of one's publications, I occasionally received requests from Departments of Psychology, perhaps in the hope of revelations about kinky physicists. Before I start looking at the past, let me first express my pleasure at joining this elite club. A glance at the membership list reveals that I have been on friendly terms with more than 20 of the physicists and 15 of the biologists, and it is a reminder of those who would have been strong candidates if they had not died before 1975. Walter Glaser of course, Bodo von Borries, and Ernst Ruska's brother Helmut come to mind.

In 1959, when I joined Mr Cosslett's Electron Microscopy Group† in the Cavendish Laboratory, I knew very little about lens aberrations. I had met the chromatic aberration of glass lenses in school physics, but I doubt if I knew much—if anything—about spherical aberration and the other Seidel aberrations (nor did I know any kinky physicists). Peter Sturrock, a pure mathematician, had studied for his Ph.D. in Cosslett's group in the 1950s, and I was told to read his *Static and Dynamic Electron Optics* and his papers in the *Proceedings* and *Philosophical Transactions* of the Royal Society. After mastering these, my project was to apply Sturrock's methods to the aberrations of quadrupole and related multipole lenses (Figure 1). At that time, only one publication contained expressions for the aberration coefficients of such lenses: Alexander Melkich's dissertation published in 1947 and based not on Hamiltonian optics and the eikonal method, perfected by Sturrock though introduced much earlier by Walter Glaser, but on the so-called trajectory method, favored by Glaser's only serious rival, Otto Scherzer. In the course of my investigations, I looked into the relation between system symmetry and permitted aberrations; from this, it emerged that sextupoles suffer from an aberration of exactly the same nature as the spherical aberration of round lenses and should hence be capable of correcting that aberration. Sextupoles have the big advantage over quadrupoles that they have no linear focusing effect and the equally big disadvantage that they exhibit second-order effects. The presence of the latter was so discouraging that I did not pursue the potential role of sextupoles as correctors—wrongly, for some 15 years later Beck and later Crewe, Rose, Chen and Mu, and Shao showed that sextupole doublets could provide correction, and a later design of Rose is at the heart of all the CEOS sextupole correctors.

I remained in Cosslett's electron microscopy group for 16 years in all, funded at different times by my college (I was a research fellow of Peterhouse), the Department of Scientific and Industrial Research, the Royal Society, ICI, and Churchill College (where I held a Senior Research Fellowship). The Peterhouse years were very happy ones. I came to know (or just to meet) such figures as the mathematicians Charles Burkill and Hallard Croft; the historians Denis Brogan, Denis Mack Smith, and Maurice Cowling; the archaeologist Grahame Clark; the Hans Christian Andersen scholar Elias Bredsdorff; the molecular biologists Max Perutz, Aaron Klug, and John Kendrew; and many others, as well as figures from the past such as E.M. Forster and Shane Leslie. But not all was "scholarship"; the college book club never overlooked a weak spot in a Fellow's armor: thus *Ash* by Bredsdorff's son was treated mercilessly, as was a (very readable) "menopausal" novel by another Fellow's wife, Menna Gallie. My Churchill years too were enjoyable partly because I was invited to join the wine committee whose task was to taste and buy large quantities of wine to lay down for future generations in the magnificent temperature- and

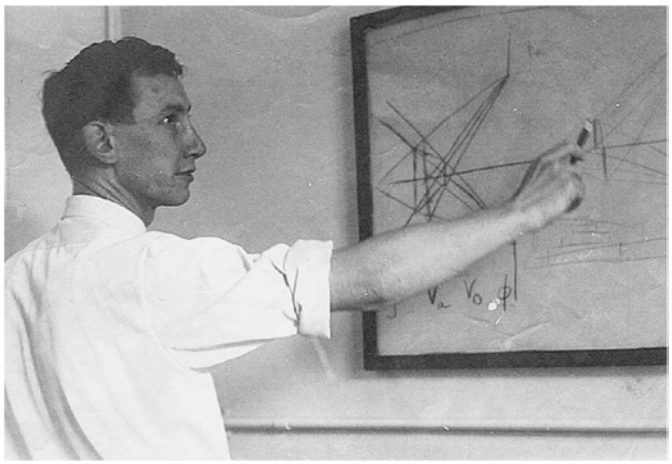

Figure 1: Peter Hawkes at Cambridge, about 1970.

†Cosslett was still "Mr" Cosslett, as his Bristol Ph.D. did not entitle him to be called Dr Cosslett in Cambridge – only when he acquired an ScD (Cantab.) did we start calling him Dr Cosslett.

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Figure 2: Peter Hawkes at a recent conference.

humidity-controlled cellars of the college. At a time when unisex clothing and long-haired men were not so common, one might overhear the Russian scholar Peter Squire (another member of the wine committee) asking his wife Natasha "malchik ili devushka?" (boy or girl?).

Back to Cosslett's EM group: the 1960s and 1970s were exciting years to be in electron optics, with Hans Deltrap showing that quadrupoles and octopoles can correct C_s (on an optical bench), David Hardy demonstrating C_c-correction, and Mike Thomson putting the early computers of the 1960s to work on electron lens problems, soon to be followed by Eric Munro. In 1966 Albert Crewe organized a workshop at Argonne National Laboratory, the object of which was to design a high-voltage aberration-corrected TEM and to persuade the NSF to fund its construction. (The doormats of the Zero-gradient Synchrotron building were embellished with quadrupoles.) With money no object, Crewe invited experts from Europe, Asia, and the USA to spend a month at ANL, all expenses paid, designing such a microscope. At the end, we presented our recommendations to our colleagues and to representatives of the major US funding bodies, unsmiling hard-faced men sitting in the front row. Nothing was forgotten. Architects showed plans of the building that would house the instrument, which was itself described in detail: gun, accelerator, lenses, quadrupole–octopole aberration corrector, image recording… Alas, the NSF said "no," but many long-term friendships were forged: I am still in touch with Ron Moses and remained in regular contact with Albert Crewe until his death. "I hope you won't find too many mistakes," he scrawled across one reprint. I didn't.

Cambridge was a pioneer in scientific computing. I remember the pleasure of Titan Autocode, the programming language of the Titan computer, which had a very thin manual: "All you need to know is here," it proclaimed. "If you cannot find the answer to a question, then you didn't need to ask it!" This was disconcerting at first but very refreshing in practice. Toward the end of the 1960s, I talked to Cosslett about digital image processing of electron images and suggested that we needed a computer such as the PDP. I expected him to offer to apply for funds, but instead he told me to apply myself. This was my first such experience, but all went well. We acquired a PDP-8 with 12 kbytes of memory and, with Martyn Horner's expertise to back them, Owen Saxton and Ralph Gerchberg showed that the "phase problem" could be solved by using the image and diffraction pattern of the same specimen area. The Gerchberg–Saxton algorithm has now spread into a vast range of scientific fields. Not long after, an application for funding to buy an Optronics Filmwriter was also successful. Cambridge was also one of the very few European nodes on the ARPANet, which was used by John Fitch to develop the computer algebra language CAMAL. I used CAMAL to evaluate aberration integrals for various field models, but this very user-friendly language was later superseded by other programs. However, as recently as 2009 Fitch was pointing out its attractions over more recent languages.

In 1978, Hermann Wollnik, at that time professor in the University of Giessen and well known in the electron optical world for his studies on the aberrations of spectrometers, wrote to suggest that a meeting designed to bring together the disjoint worlds of electron optics, spectrometer optics, and accelerator optics would be beneficial all round. He and Karl Brown (SLAC) invited me to represent electron optics, and a first meeting on charged-particle optics was held in Giessen in 1980. This was a great success, and cross-fertilization was visible at all levels. The second meeting was held in Albuquerque in 1986 with funding from Los Alamos National Laboratory, after which CPO conferences have been held every four years. I organized the 1990 meeting in Toulouse; since this included full-scale French lunches (four courses with unlimited wine and coffee) every day and a magnificent banquet in a nearby château, it was voted a great success. The optics was good too.

My attention was caught by papers on image algebra, a mathematical structure that treats entire images as elements of an algebra. This is especially attractive for mathematical morphology, a non-linear set-theory-based approach to image processing, but is also nice for linear image processing. I made one or two contributions to the subject, notably a paper entitled "The STEM forms templates" [1], and was for some years on the editorial board of the *Journal of Mathematical Imaging and Vision,* the leading serial on the subject.

In 1974 the future of Cosslett's group in the Cavendish was uncertain, and I received a letter from Bernard Jouffrey, Gaston Dupouy's successor in the Toulouse Laboratory of Electron Optics where the world's first high-voltage electron microscope was built, suggesting that I might like to join the laboratory with a view to launching image processing in Toulouse. My application to join the CNRS was successful, and in 1975 I moved to the Toulouse Laboratory, where I remained until (obligatory) retirement in 2002, followed by a few years as Emeritus Director of Research (Figure 2). Aberrations remained a preoccupation throughout all these years, marked by few high points. In 1997, I was invited to give a talk at the EMAG meeting in Cambridge [2] on the centenary of J. J. Thomson's demonstration that the electron was a particle, for which he in due course received the Nobel Prize. (A few years later, his son received the Nobel Prize for demonstrating that it is, on the contrary, a wave!) That meeting was memorable for two papers: Ondrej Krivanek and colleagues showed that they had succeeded in correcting the spherical aberration of a STEM by means of quadrupoles and octopoles. And Mick Brown launched the idea of a specialized laboratory

for aberration-corrected electron microscopy, which became the highly successful SuperSTEM facility in Daresbury. Another high point was the Royal Society meeting on "New possibilities with aberration-corrected electron microscopy" in 2008, at which I gave the opening talk on the history of aberration correction from Scherzer's unwelcome proof in 1936 that C_s and C_c are inevitable in round lenses, through the endeavors in Darmstadt, Cambridge, Chicago, and elsewhere to build correctors, leading up to the successful efforts of Harald Rose, Max Haider, and Ondrej Krivanek and their teams [3]. The story is brought up to date in a long paper in a recent issue of *Ultramicroscopy* [4].

Apart from time spent on formal research, I have greatly enjoyed editing the *Advances in Electronics and Electron Physics* (now *Advances in Imaging and Electron Physics*) and writing book round-ups for *Ultramicroscopy*. I inherited *AEEP* thanks to a friendship struck up with Bill Marton, founder editor, at the ANL workshop mentioned above. After his death in 1979, his widow Claire took over the editing, and soon after she invited me to meet her in Geneva—she regularly spent her summers in Switzerland—to discuss the future of *Advances*. But when I arrived in Geneva and went round to her hotel in the evening, I met her doctor on the stairs and was told that she could not receive visitors as she had just suffered a serious stroke. I returned the next morning to discover that she had called a Swiss friend during the night and asked her to come and collect her in the small hours! The bird had flown and the hotel could not even give me the friend's address or phone number. I therefore returned empty-handed but was soon approached by Mr. Erwin Cohen, everyone's dream of a publisher, at Academic Press who asked me to take over *AEEP* provisionally. I accepted, and provisionally rapidly became permanently. I have remained editor-in-chief ever since. Many distinguished scientists have contributed to our pages, and biographies of several pioneers of electron microscopy have appeared: Ernst Ruska, his brother Helmut, Bodo von Borries, Dennis Gabor, Jan Le Poole, and John Reisner contributed a masterly history of the electron microscope in the USA; his author's preface should be read by anyone thinking of writing history [5]. Two thick volumes have been devoted to "The Beginnings of Electron Microscopy" and "The Growth of Electron Microscopy," while the story of the scanning electron microscope is traced in great detail in a tribute to Sir Charles Oatley. Other memorable volumes have covered "Aberration-corrected electron microscopy" and "Cold field emission and the STEM."

I also must say a few words about *Ultramicroscopy*. I had known Elmar Zeitler, the founding editor, since the ANL workshop in 1966, and, soon after he launched *Ultramicroscopy*, I sent a poem in response to a call for a better title [6]. This was the start of a long and highly entertaining correspondence with Zeitler's secretary Judith Reiffel, who recalled our exchanges in her contribution to Elmar Zeitler's Festschrift [7]. I had the sad but satisfying task of writing a tribute to her after her death in 2002, published (of course) in *Ultramicroscopy* [8]. Relations with subsequent editors and their assistants have been just as close: Peter and Jenny Kruit, Paul Midgley and Anne Chippindale, and now Angus and Keiren Kirkland. I note somewhat wryly that the number of downloads of my round-ups in *Ultramicroscopy* is far greater than the number of requests for reprints of my scientific publications in *Optik*—perhaps the presence of jokes in the round-ups is part of the explanation. . .

Publishing also involved writing and editing books, solo or with a co-author. The three volumes of *Principles of Electron Optics* [9] by Erwin Kasper and myself took many years to assemble. We each drafted sections of the books and then criticized and re-wrote them! The younger generation may be amused or amazed to learn that the text was typed in Tex by Sabine Ströer, the wife of one of Kasper's research students; when complete, it was sent to Academic Press in London, together with the drawings (Indian ink on thick drawing paper). Their copyeditor estimated the space needed for each figure, and Frau Ströer modified her files accordingly and produced a printout with blank spaces above the captions. Meanwhile, Graphic Arts at Academic Press had produced scaled versions of the drawings, and I spent a day or two with a pleasant and very skillful young woman in the Academic Press office gluing the figures into place. (One is upside-down, but no one but me has noticed it.) *Science of Microscopy*, which John Spence and I edited [10], is memorable for battles with an incompetent company to whom Springer sub-contracted production (but never again!). And I must not forget *Biophysical Electron Microscopy* [11], edited with Ugo Valdrè, an unusual subject at that date (1990). He and I have recently written a short account of the achievements of the Cambridge HVEM [12].

How am I to conclude these random thoughts? Perhaps with the words of a song. With honorary membership of the French Microscopy Society, Fellowship of the Optical Society of America, and now of the MSA: "Who could ask for anything more?"

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