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An Interview with Peter Lancaster

Higham, Nicholas J.

2005

MIMS EPrint: **2005.19**

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The University of Manchester
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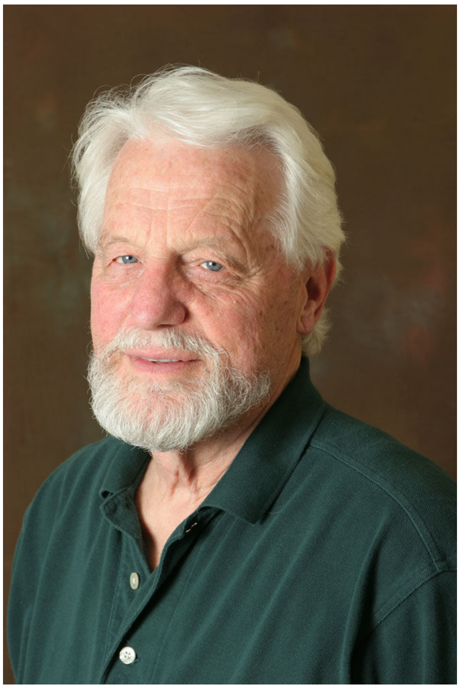
ISSN 1749-9097

An Interview with Peter Lancaster[1], by Nicholas J. Higham.



Peter Lancaster, March 2005

NJH: How did you get interested in mathematics?

PL: Wow, that's going back a long way. At school, mathematics was one of a few subjects that I seemed to be fairly good at, and therefore enjoyed. After school, I made a false start. When I first went to university I started in architecture, so I wasn't completely wedded to mathematics at that time. When the time came to make a change I knew that I could go back to mathematics and I did. So my age was 18 or 19 at that time. I hadn't taken a very strong interest in it before that age. Then I got caught up in the honours programme at Liverpool and the rest is history.

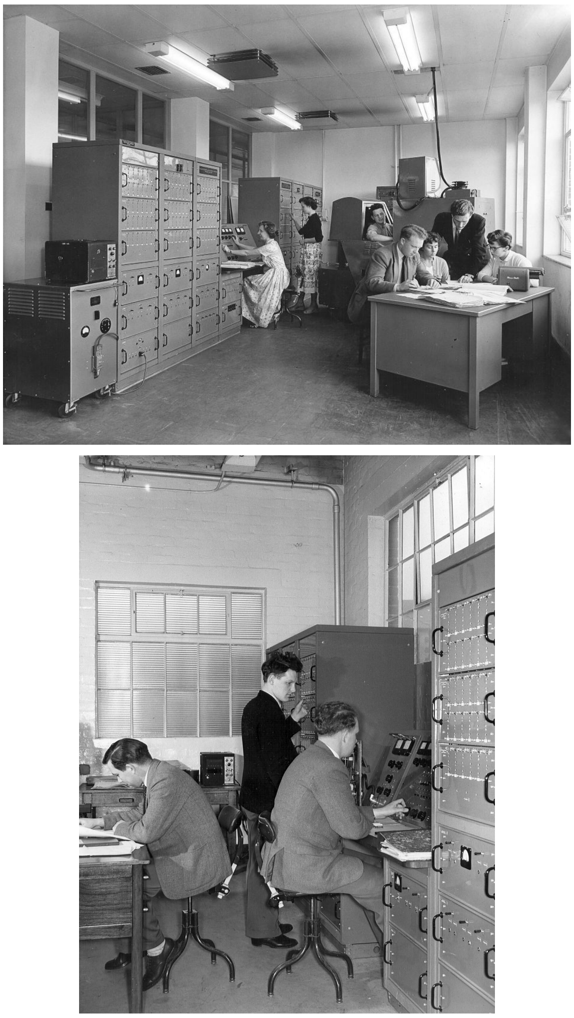
NJH: And I know that after your undergraduate degree in mathematics at the University of Liverpool you worked in the aircraft industry in the north west of England. What sort of problems were you working on and how did this influence your mathematical career?

PL: I was in the aero-structures group at what was then called Warton Aerodrome and was the research arm of the English Electric Company, which later became British Aerospace. I was involved in setting up mathematical models of aircraft structures for analysis of the "flutter" problem. First as one of a group led by Ivan Yates, but then being able to take more initiative as time went on (I was with the English Electric for five years). A number of methods were available. The book of Frazer, Duncan and Collar was our standard reference at that time, and it had some leads on iterative methods, for example. In this connection, the work of A. C. Aitken was quite influential. It was part of my job to work with people who were generating the structural data and then to put it into the mathematical model and do the computing. We did both analogue and digital computing to produce results which, hopefully, would guarantee the safety of the aircraft.

There was one interesting occasion when, in a sense, we didn't guarantee the safety of the aircraft. Naturally this was on a Friday. There was an incident with the aircraft being flight-tested at the time: a P1 prototype, which later became the Lightning (see `www.thunder-and-lightnings.co.uk`). They were flying through the sound barrier, so operating at about Mach number 0.95, and a flutter incident occurred involving the rudder. This scared the living daylights out of the pilot, Roly Beamont, but he managed to keep things under control and land the aircraft again. Of course we were all called in to work flat-out over the weekend, to try to understand why it happened, and to recommend changes to the structure. I remember we recommended subtle

[1]Interviewed at the University of Manchester, March 15, 2005. Numerical Analysis Report 468, Manchester Centre for Computational Mathematics, Manchester, England, June 2005. `http://www.ma.man.ac.uk/~nareports`

changes involving mass-balances which we were confident would be effective. But in the end they didn't accept our proposals. They made an engineering change: they just beefed up the structure instead, and took the expense of extra weight.



The English Electric Company aerostructures group in the analogue computer room in August 1955. Peter Lancaster is in the driver's seat (top) and in the middle (bottom). This machine was used a lot for the flutter analysis—without the cockpit simulator. Standing (top) is Ivan Yates, who went on to be a director of British Aerospace.

NJH: So you were solving eigenvalue problems?

PL: Yes, quadratic eigenvalue problems, in particular, for vibrating systems, the elastic system being the aircraft itself, and of course the tricky part was modelling the velocity-dependent aerodynamics. It was the interaction of the aerodynamics and the structures which was our concern and we felt, rightly or wrongly, that we had a pretty firm grip on the structure, but the aerodynamics was considerably more difficult. The theory of transonic flight was in its infancy then. There was a strong group here in Manchester at that time including James Lighthill and Fritz Ursell. They were the people who, in the 1950s, were developing the theory of shock waves.

NJH: What size were these eigenvalue problems?

PL: For computing vibration modes and frequencies the matrices would be approximately 20-by-20. But for flutter itself they were small. In retrospect, it is amazing what we could do with just two degrees of freedom (one bending and one torsion mode). We thought if we could get up to four or five (for control surface problems) we were being quite ambitious. But those degrees of freedom were the so-called modal coordinates, which describe the structure quite effectively and implicitly contain a vast amount of information. Near the end of my time there I could see the beginnings of the movement toward finite element methods where, instead of looking at modal coordinates, you'd look at displacements at discrete points on the structure as your degrees of freedom and link them with the so-called influence coefficients, and this would provide a representation of the structure. So these matrices were considerably bigger. I think this probably gave the finite element method some impetus, but of course finite elements developed as an effective tool quite a bit later than that.

NJH: What computing facilities were available?

PL: Desk-top calculators (Monroes and one or two Brunsvigas) were the standard equipment to begin with. However, the English Electric Company had its aircraft division in Warton (near Preston) and a digital computer division in Stafford. So we would occasionally travel down to Stafford to do our calculations on budding high-speed digital computers (the DEUCE). Of course, at that time, we could use only machine language. There was nothing else—no high level language. This probably turned me away from writing programmes for life. Nowadays I am happy to use a high level language, MATLAB in particular, but I lost my taste for programming in those early difficult years. We also had a fascinating analogue capability which included a life-size cockpit: by turning a key we could vary one of the coefficients in a quadratic matrix function and watch the effect on the eigenvalues on a screen.

NJH: Did you have any contact with Wilkinson at all at that time?

PL: I certainly knew of him and his work at the National Physical Laboratory. I first met him in Gatlinburg in 1961 and, later, he visited the University of Calgary on one occasion. This was about the time that the first edition of "The Algebraic Eigenvalue Problem" (1965) was nearing completion; if it wasn't already complete. I knew it was in production and I was very interested. Of course, I got a copy at the first opportunity

NJH: I recall that Olga Taussky used to talk about working on flutter problems in the 1940s.

PL: Yes, in the 1940s she was with the British Ministry of Aircraft Production and wrote Aeronautics Research Council (ARC) reports. (In the late 30s she and Jack Todd were teaching in London University and they married in 1939.) While I was at the English Electric I saw ARC reports written by Olga. This was where I first learned about Gershgorin, and some of the work of Helmut Wielandt. However, I didn't meet Olga and Jack until that 1961 Gatlinburg meeting. So I knew of them and their work, as with Jim Wilkinson, but didn't meet them until we all met in the United States. Later on, Jack and Olga made it possible for me to spend a year as Visiting Associate Professor at Caltech; this was '65/'66—and a great experience for me.

NJH: So from industry you went to the University of Singapore, where you spent five years. Why did you choose to go to Singapore?

PL: Because it was one of the few places that would offer me a teaching and research position. I was at the English Electric because this was a time of compulsory national service in the UK—it was not so long after the second world war. And so I was working in the aircraft industry for five years instead of doing two years of service in the forces. So as the end of that five year period approached I began to look around for something else to do. Since my student days I'd always aspired to a university teaching job. I'd actually completed a Masters degree while I was at the English Electric, under the supervision of Louis Rosenhead at the University of Liverpool, in which I surveyed the techniques that we used for solving the flutter problem. I knew it would be difficult to get into a university in the United Kingdom so, in the time-honoured British tradition, I looked further afield, to Australia, Africa, Singapore, or wherever. And Singapore was one of the institutions that made me an offer, so that seemed a good place to go.

NJH: And that was where you first met Richard Guy?

PL: Yes. Richard was the Acting Head of Department in 1957 when I was appointed. It was a small department but very active and stimulating. When I arrived I think there were only eight teaching staff, but we had regular seminars on a great variety of topics. For example, I dabbled in some problems in discrete mathematics for a while, stimulated by my colleague Eric Milner in particular, and by Richard Guy. So a stimulating group was already there, all willing to talk to one another about their research. Also, the university was the former Raffles College, so had a substantial history and a good library research collection. Those two factors were very important while I was beginning a research career.

While talking about this period, I should also say that Alexander Ostrowski had a strong influence on my career. I was working on iterative methods for eigenvalue problems and came across the work of Ostrowski. And I found a way to generalize some of his analysis to eigenvalue problems for matrix polynomials. I wrote to him about it. He was very kind and wrote back at some length and I like to think that I developed a little analytical style from what I learnt from Ostrowski. He was also one of the prime movers in the Gatlinburg meetings, so in 1961, when I was actually on leave in the UK, he arranged for an invitation for me to the '61 Gatlinburg meeting, where I met not only Ostrowski, but many of the other famous names in the field: Olga Taussky and Jack Todd, Jim Wilkinson, Leslie Fox, Collatz, Bauer, Henrici, and so on.

NJH: So that was when the Gatlinburg meeting *was* in Gatlinburg. Would it be the first?

PL: Yes, I think the meeting of 1961 was the first. And, of course, Householder was the principal organiser, so I got to know him too.

NJH: In 1962 you moved to Calgary. Was this the time that the mathematics department was actually being formed, or was it already in existence?

PL: Yes, it was just being built up. At that time there wasn't a full degree programme in mathematics (although my memory might fail me here). There certainly wasn't a full degree programme in engineering. We taught just the first two years of engineering mathematics and then those students would go to Edmonton to complete their degree. The science programme might have been a little more advanced, but it was certainly the plan to develop a full programme over a period of just a few years. The Head of Department at that time was John Peck, who is alive and well in British Columbia. John began life as a functional analyst, but became a computer scientist. He was one of the prime movers in the Algol language, and then became the first head of computer science at UBC. I think he had the right ideas and ambitions for the Calgary department. Again, it was a good environment for me. (The University of Calgary became an autonomous institution in 1965.)

NJH: How did your 1966 book, *Lambda Matrices and Vibrating Systems*, come about?

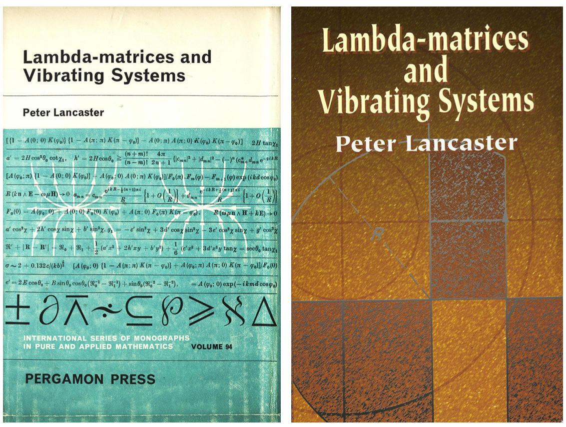
PL: That began life as my Ph.D. dissertation. In those days—and it may still be possible in British universities, or universities in that tradition—one could register for a degree while teaching. My first appointment in Singapore was as an assistant lecturer, but after a couple of years it was clear that my research was going quite well and I was managing to publish some papers. But I didn't have a doctorate and I was wondering about registering for such a degree. After talking to Richard Guy and Alexander Oppenheim, who was the Vice Chancellor at the time (another mathematician), I decided to register for a Ph.D. in Singapore. I completed that dissertation before leaving Singapore in '62, and the two external examiners were analyst Ian Sneddon from Glasgow, and Leslie Fox, a numerical analyst from Oxford. Ian Sneddon liked the subject matter and suggested that I write it up as a monograph in the Pergamon series that he was editing. So that's how it happened.

NJH: Only three years later you published the *Theory of Matrices* book. How did that come about, and what influence has the book had?

PL: In Calgary in the 1960s, partly because we were a new department and we were setting up new programmes, I was able to give a senior linear algebra course and, given my background, it tended to emphasize matrix analysis. I gave that course perhaps three or four times through the 1960s, and it seemed to me that there was a market for a textbook like that. I was strongly influenced by Gantmacher, but Gantmacher's volumes were hardly a textbook, and so one of my objectives was to write a textbook in the manner of the Gantmacher volumes. It seemed to meet a need and sold quite well, and perhaps because of the association with Gantmacher the Russians liked it and it was translated into Russian and became better known in the Soviet Union than it was in the West.

NJH: Was this a legal translation?

PL: No it was illegal. It was done shortly before the Soviet Union signed international copyright agreements. So although they published sixty thousand copies in two different printings it did nothing for me financially.



Front covers of 1966 (Pergamon) and 2002 (Dover) editions of *Lambda-Matrices and Vibrating Systems*.

NJH: When did your collaboration with Israel Gohberg begin?

PL: It began in February of 1975 and it came about because I knew of the work of the school of M. G. Krein and Gohberg through the 1960s. In fact, Krein had become aware of my Lambda Matrices book and, through an intermediary, asked for a copy of it, which I sent to him and, in return, he sent me a copy of one of his monographs. So I knew of Gohberg and then I heard through the grapevine that Chandler Davis in Toronto had arranged for Gohberg to visit Canada shortly after emigrating from the Soviet Union. So he emigrated from Kishinev, Moldavia, to Israel in the summer of '74 and then in early '75 he was already visiting Canada. I invited him to come and spend some time in Calgary during that visit. That's where our very rich friendship and collaboration began.

NJH: You went on to write three monographs in the 1980s with Gohberg and Rodman. How did that develop from that initial meeting?

PL: Our actual collaboration began a little later. I had a sabbatical in '75/'76 in the University of Dundee and Gohberg came over to Dundee. At that time I was publishing some papers on what we now call Jordan pairs and triples—ways of encapsulating the spectral data for matrix polynomials. He liked that and could see interesting ways of taking advantage of it, using and developing it, and that's where the collaboration began. At the same time Leiba Rodman was his Ph.D. student in Tel-Aviv, working in a similar direction, and so the collaboration between the three of us grew out of that. Then I had Leiba visit Calgary as a postdoctoral fellow from '78 to '80 when a lot of the ground work for the two later volumes was prepared.

NJH: Did you develop the books by first of all publishing the material in papers, or is there material that went directly into the books?

PL: Quite a lot went into papers first, although there was always a substantial amount of material that went directly into the books. There was a series of papers in Linear Algebra and its Applications, for example. At the time, I think it's fair to say that we, but certainly I, felt the factorization theory was the most significant mathematical aspect of what we were doing. We had a line of attack on that which seemed to be very fruitful and the core of that work was published in a paper in the Annals of Mathematics in 1980. That led to so many other things. You might say my research career began with quadratic polynomials and then the step from there to general polynomials of arbitrary degree is very natural, then to singular polynomials, and then to rational and analytic matrix functions and so on. So we fairly quickly moved into a broader spectrum of mathematics, including not only the canonical Jordan structures but also questions of perturbation theory, which play a central role in many applications. In that way we made connections with classical work of Rellich and Kato, for example.

NJH: You have written quite a lot of papers with coauthors. Do you have a particular way of working?

PL: By accident I think. I don't have a design that I repeat. I find that a rather difficult question to answer. In more recent years the collaborations have come about because younger people have visited Calgary, perhaps at my invitation, either as postdoctoral fellows or Ph.D. students. And so quite a lot of collaboration developed in this way with a younger generation. And, naturally, there is a certain pattern there which accounts for some of the collaborations. Others that don't quite fit this mould would be more senior people who have visited Calgary for a longer period. These include Pal Rozsa, Ludwig Elsner and, last but not least, Alek Markus, who is a mine of information on operator and matrix polynomials; I have really enjoyed this collaboration and learned a lot from it. But for me the secret is always face-to-face meetings and spending time together. I think perhaps I am a slow worker; it takes time for ideas to develop and evolve. So longish periods for meetings and interaction are needed to develop collaborations.

NJH: You spent sabbaticals at various institutions. Do any of those stand out and why?

PL: The one we've already mentioned is the first to come to mind; that was the University of Dundee in '75/'76. I think partly because that was the period when the collaboration with Israel Gohberg evolved, but also at that time I was very interested in approximation theory and so I was fascinated to be there in a group of people, almost a whole department as I remember, who were interested in either differential equations or numerical analysis. This was a very stimulating environment to be in. I talked at some length with Ron Mitchell about finite element methods, and the notion of upwinding schemes for appropriate differential equations was just being formulated at that time. I took quite an interest in finite element methods but also in the more fundamental aspects of approximation theory, the questions of approximation in two or three dimensions, and construction of interpolation schemes on square or triangular elements.

NJH: Do you have any comments on how the relationships between core linear algebra and numerical linear algebra have developed or changed over the years?

PL: I certainly find that the middle ground between those is very interesting. My sense is that in recent years, in the last decade or two, core numerical analysis has rather moved away from core linear algebra. Perhaps there's been a little divergence of interest there. For example, the work that we (and many other people) have done on indefinite scalar products and the use of the geometry of subspaces of various kinds remains to be exploited numerically. My feeling is that the theory has gone quite a long way in the last 15 or 25 years, but the numerical absorption of this, or the advantage that has been taken of this, could have been stronger. Perhaps it simply means that there is research to be done there in the near future to fill this gap.

NJH: Have you ever felt a result, paper or book of yours has not received the attention it fully deserved and if so which, and why?

PL: That's a difficult one. With a good friend and colleague in Calgary, Kes Salkauskas, we wrote an introduction to transform theory about eight or nine years ago which went up like a lead balloon. I have a gut feeling that it's worth more than that, and that it should have a better place somewhere in undergraduate teaching. This book was aimed at trying to present transform theory to an audience of people with minimal prerequisites in analysis, so it seemed to me that the potential audience was really very large. Probably an explanation for its lack of success is that it was based on a course that we ourselves designed, primarily for geophysicists in the Calgary environment. Therefore it's not a text that fits neatly into a standard curriculum and, consequently, hasn't had a lot of adoptions. But I hope and think that it's probably worth more—and maybe its time will come. (See *Transform Methods in Applied Mathematics*, with Kes Salkauskas, Wiley-Interscience, 1996.)

NJH: Another book that will be less known to your linear algebra colleagues would be your book on splines. Was that graduate level or undergraduate level, and what was the aim of that book?

PL: Well again, that developed from extension courses for geophysicists working in industry in Calgary. Of course, it is not just splines but it was one of the earlier introductions to splines and more general surface fitting. That did fairly well. I'm not sure that it's still in print, but it had quite a wide circulation in its time. Soon after we wrote the book the field moved very rapidly with the introduction of wavelet analysis. (See *Curve and Surface Fitting*, with Kes Salkauskas, Academic Press, 1986).

NJH: Do you have a favourite paper or favourite book?

PL: I'm most proud of the paper in the Annals of Mathematics (1980) that we referred to before. That and the paper with Leiba Rodman on Riccati equations (also 1980). I feel that our theorem there on the characterization of solutions via invariant subspaces was elegant and timely. I am also proud of the text *The Theory of Matrices* (2nd edition with Miron Tismenetsky), and of its longevity: thirty-six years in print.

NJH: You're still as active as ever, eleven years after retirement. To what do you attribute your enthusiasm for research, and your ability to develop new research topics and find new collaborators?

PL: Well it's really quite simple. I enjoy it and I see no reason to stop. I'm very fortunate in that I continue to get the support of the University of Calgary and the Canadian research council. Of course, that oils the wheels and provides the necessary infrastructure: a place to sit, a place to get your computer services and all the other services we need, the potential to go on supervising graduate students, and the freedom to make and accept invitations to collaborate. I've had maybe three Ph.D. students since I retired and three or four postdoctoral fellows coming and going. And I simply enjoy it. Not only the sense of satisfaction and accomplishment one gets from succeeding—achieving some small successes in research—but also the continued exposure to the younger generation is very stimulating and helps to keep us older folks alive and active.

NJH: You've been involved in the Pacific Institute of Mathematical Sciences since its conception, I believe. How important has PIMS been to you and to Canadian Mathematics research?

PL: For my research personally, it certainly had a beneficial effect through the development of the Banff Research Station and the workshops that I have been able to attend there over the last couple of years. It seems clear that my career would have gone on without it, but looking more broadly at the national scene PIMS has had a terrific impact on Canadian mathematics. Formerly, there was some sense of isolation in the west of Canada. The simple physical problem of separation by some thousands of kilometres from the power houses of Ottawa and Toronto, in terms of mathematics or politics, posed certain problems. The creation of PIMS has formed a balancing structure and a source of stimulation that we can relate to without having to cross a continent. So it's had great impact on, particularly, the two western provinces, Alberta and British Columbia, and, of course, to a lesser degree on the rest of Canada too. But the connections with the Pacific Rim are also very interesting and obviously stimulated by PIMS. For example, there are stronger links with Japan, Mexico, Chile, and even Australia (which is also a Pacific Rim country). This applies not only in research, but in all the aspects of PIMS activities; in education, industrial connections, and so on.

NJH: I know you love the outdoors and know the Canadian Rockies very well, and many visitors have enjoyed your hospitality in your log cabin near Banff in the lakes and mountains. How important have these outdoor activities been to you and how have they meshed with your research?

PL: Well, I see these activities, and my fortunate ability to continue these activities, as very important. It's my form of relaxation and recuperation, and it plays an important role in sustaining me. I certainly get very fidgety if I'm not able to go out and indulge these activities for a week or two. So, for me personally, it has been and continues to be very important. And fortunately my health allows me to continue to indulge this. By the way, this phenomenon is not unusual in the broader mathematical community. There are quite a few colleagues out there who share this enthusiasm with me, and this suggests that the two interests (of mathematics and mountains) support one another.

NJH: Have any theorems been constructed during hikes, maybe with colleagues?

PL: Particularly with Israel Gohberg and Leiba Rodman, who have spent prolonged visits in Calgary. We have done several of the traditional hikes together. I recall discussing this, that, or the other issue of our current preoccupation, whether the notion will go this way or that way, will this work or that work? That kind of discussion certainly took place among the three of us and, to a lesser degree, I'm sure it has happened with other colleagues and collaborators.

NJH: Your Lambda Matrices book was reprinted by Dover in 2002. In the last few years there has been a resurgence of interest in quadratic and more generally polynomial eigenvalue problems. Are you surprised that the book is of renewed interest after 36 years?

PL: Yes—surprised, and very pleased. It may also suggest that it was undervalued earlier, through the '70s. It went out of print with Pergamon press very early. I think they only printed a few hundred copies, but it continued to get citations through the rest of the twentieth century, mostly from the engineering community. I think there is some interesting exposition there which is only recently being recognised by the numerical analysis community.

NJH: You've written ten books according to my reckoning, including one second edition. Do you have any tips or advice for authors about book writing?

PL: Keep careful notes when you're lecturing. It may or may not lead to book writing but it certainly helps if that's the direction you want to go and if the book you want to write is something you've delivered in lectures. I'm not sure what else to say. I don't want to suggest that it's easy writing books. One has to be prepared to take the time and effort to write and then re-read and then re-write if necessary. This process is not to be underestimated. Perhaps another positive piece of advice would be to find good collaborators. Two heads are almost always better than one, particularly in the context of book writing. The broader perspective that you get from having two authors rather than one is generally very valuable.

NJH: What about the rate of writing, because it seems to me if you're not quite on the same wavelength as a co-author in terms of speed and the ability to focus 100% on the book for a while, it could be rather difficult.

PL: Yes, certainly it would be hard to find two people who could produce serious text at about the same rate. Development tends to be spasmodic. In my collaborations the writing has generally been divided. Each of the authors has a favourite part of the subject matter and will write the first drafts of this, that, or the other chapter. Then the other author(s) get to see this, comment on it, and suggest further developments from it. These are the benefits of the collaborative process. At the very least, the material gets a careful reading by one other person. In a good collaboration your ideas will not only get a good reading, they will also be pushed a little further and developed.

NJH: You're currently putting the finishing touches to the second edition of your 1982 book *Matrices and Indefinite Scalar Products* with Gohberg and Rodman. Have personal computers and LaTeX made book writing easier?

PL: My first reaction is yes, because we are deeply involved with LaTeX, particularly in the edition we are working on right now. This is the way in which we operate. Being rather naive about computer science in general, it seems miraculous to me that a 300 page draft can be whisked back and forth around the world to one keeper or another. This is part of our current collaboration with the three authors; the master version can move from the care of one individual to another and (at least while I am in Manchester) the three of us are on different continents. And the geography presents no difficulties. So my first reaction is yes, it's enormously helpful. My second reaction is perhaps a little nostalgic. It was also very nice to write material yourself, and in the earlier days of collaboration you would literally have a manuscript, your co-authors would see this and comment, and the manuscript would evolve as it passed between the co-authors. Then you would hand it over to the typist, hopefully a skilful one (I've been very fortunate in this respect), and you could forget about it until the typescript came back. There was something to be said for that era too. It wasn't quite so absorbing as handling your own TeX files turns out to be.

NJH: What are your future research plans?

PL: As you know, I'm really caught up in these inverse quadratic eigenvalue problems and I feel now that I'm just getting on top of them. I feel that I'm getting a good perspective and, for the immediate future, I am interested in polishing this material and bringing it together into a unified body of knowledge. I hope for the health and strength to go on with that and also with the work on pseudospectra (with Panos Psarrakos and Lyonell Boulton), which is quite fascinating. But then the work on a biological problem, the Markov process describing phylogenetic trees (together with Erich Bohl), is also very much in the forefront of my mind. I am eager to see that in print and I would very much like to see it developed further. But whether time and circumstance will allow me to develop three or four projects simultaneously remains to be seen.

NJH: Might we see another book?

PL: You might, yes. There again, the inverse eigenvalue problems might be a candidate. We feel that's relatively new material and, as I was saying before, perhaps it should all be brought together into a systematic form that could conceivably be a monograph. If so, this might be together with my recent collaborator, Uwe Prells.

NJH: Is there anything else that you think I should have asked?

PL: You might have asked about early influences, other than mathematical. There were two or three career turning points that I can identify which don't necessarily have anything to do with mathematics. One of these has to do with the curate of the Anglican church that I attended when I was a teenager. There was some question about whether I was going to be able to continue with a university education. My parents weren't all that enthusiastic; I didn't come from a family with either money or an academic background. But the efforts of Will Pugh, the Anglican minister in question, were quite critical in getting me into the mathematics programme at Liverpool, and allowing me to pursue that career.

Also, I can't over-emphasize the importance of partnership—in the marital sense. Having the back-up of a family, and an understanding wife, in particular, is for me a *sine qua non.*

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- Special issue of *Linear Algebra and its Applications* in honor of Peter Lancaster, Edited by H. Bart, I. Koltracht, A. Markus and L. Rodman, Volume 385, Pages 1-476 (July 2004), including a collection of Personal Notes by Peter's family and friends.