Introduction

As did many investigators in the last century, I started research as a solitary worker in anatomical science. Indeed, my earliest investigations in the 1950s involved dissections in comparative anatomy. Dissecting is a solitary activity. Today, much research involves work at the computer keyboard and this too is often a solitary activity. Today's research lab can degenerate into several desks, each one with a computer, and several research students who may *never,* well hardly *ever*, talk to one another. One of my aims has always been to breach such isolation.

I have, therefore, always been involved in collaboration. In the early days, whether with colleagues or research students, this was generally with individuals, who, like me, had degrees of anatomical expertise, who were anatomists.

However, for a better understanding of the human and animal anatomies, functions, and, later, behaviours, later still, development and evolution, into which anatomical dissection has lead me, I have found that collaborations and consultations with workers in other disciplines (statisticians and applied mathematicians, computer scientists and programmers, applied physicists and engineers) and the use of new concepts and technologies (from mathematics, physics and engineering) have been extremely important. This was not usually available to anatomists in my youth. Hence arose the title of my first book in 1973: *Form and Pattern in Human Evolution: Some Mathematical, Physical and Engineering Approaches*, University of Chicago Press.

Thus, as soon as it was clear that dissection of muscles and observations of bones lead into measurements and their analysis, it became obvious that simple statistics, means and variances, Chi square and

Student '*t*' tests, were not the whole answer. My mentor, Lord Zuckerman (though he would have been horrified at the modern term mentor) insisted that I should visit R. A. Fisher at Rothamstead Experimental Station. Fisher, of course a most senior statistician, was very nice to me but quickly said:

'You should talk to my young man, Yates'.
Yates was no longer a 'young man', and was even then very senior, was equally nice to me but quickly said:
'You must talk to my young man, Healy'.

Michael Healy really was young, about my age. Thus was I introduced to matrix algebra, multivariate statistical methods, programming in binary code, later, Mercury Autocode, and the main frame computers of those days, huge 'tube' or 'valve' machines, first the old Elliot at Rothamstead and later the English Electric KDF 9 in Birmingham.

Years later when I went to the University of Chicago I was worried that I might have lost the Fisherian kind of consultation and collaboration. But at Chicago I immediately discovered (of course) Paul Meier, David Wallace and Bill Kruskal and was amazed to find the same excellent relationships as the Rothamstead group had given me. Later still, when sitting next to, the, by then, venerable Tukey as joint guests of honour at a dinner of the Mathematical Geologists of America in Kansas, I told him about the serendipity of my meetings with these British and US statisticians. He laughed. He knew, as I did not, that the Chicago statisticians were mostly Tukey's students, that there was a continual exchange of students between his lab and Fisher's, and that all these people had developed the same unusual abilities to talk with and advise biological scientists who had not too much knowledge of statistics. Further help in the United States was also given by others. Joel Cohen (who almost immediately left for Harvard) also helped me with multivariate statistical programmes. Peter Neeley (who almost as quickly left for Kansas) helped me with the neighbourhood-limited classification. D. F. Andrews of Bell Telephone Labs (as it was in those days) provided the method of high-dimensional analysis which he had developed using some of our

own data. L. A. Zadeh's work (though I never met him) provided early thoughts about fuzzy sets in understanding biological forms. This has even continued with my move to Australia where Norm Campbell (CSIRO) mostly through his later edition of Reyment and Blackith's book, and Adrian Baddeley (University of Western Australia) have been so extremely helpful. I have always been blessed with mathematically gifted colleagues with real abilities to understand my biological problems.

Those first dissections also lead to my taking an experimental stress analysis course in the early sixties in the Department of Mechanical Engineering at the Royal College of Advanced Technology at Salford (now the University of Salford). Other students came to the practical classes with cold steel beams and oily crankshafts; I produced wet, warm and bloody butcher's bones. My mechanical engineering teachers were somewhat horrified, but *very* intrigued.

Similar relationships with many other scientists in the mathematical, physical and engineering worlds, and the 'borrowing' of many of the technologies in their laboratories, also proved essential. Often these took me 'outside' university and 'into' industry. One of the first of these links was with the late Ken Sharples of Sharples Stress Analysis, UK, and an introduction to direct photoelastic analysis. Others were colleagues using reflection photoelasticity at Westland's Aircraft (as it was then) in the Isle of Wight. (This aircraft link reminded me of my father's involvement with Mitchell and the Spitfire at Supermarine Works, Southampton, before the war). These industrial colleagues acted totally altruistically; this was long before the miserable constraints of 'user pays' that have so damaged universities. Industrial collaborations continued when I moved to the United States, where I was involved with Gregory Pincus (Image Analysis at the University of Wisconsin), John Davis (Geology, Kansas State Geological Survey) and Berry and Marbles (Geography, also in Wisconsin). They were all using different kinds of industrial image analysis and optically generated Fourier transforms to examine materials such as aerial pictures of jungle tree tops, sections of oil-bearing rocks, and tree-like patterns of river branches. But I saw in those

techniques methods for examining radiographs and sections of bones. Dr Harry Yang in Chicago was my first MD/PhD student who greatly benefited from this technology. Today, of course, Fourier analysis is carried out on desktop computers using Fast Fourier Transforms. But I did not do this until my move to Australia and it required help from my graduate student (now Dr) Alanah Buck (currently a Forensic Anthropologist), programmer, as he was then, Iain Sweetman, and medical physicist, now, Professor Roger Price.

I graduated from the earlier photoelastic stress analysis to computer-derived finite element analyses (FEA) of stress and strain and worked first with another graduate student (later Professor) Artyan Hsu in Southern California. Even in Australia, such relationships have continued with Engineers Chris Windsor and Wayne Robertson in geomechanics at the CSIRO and the University of Western Australia. They introduced me to the Fast Lagrangian Analysis of Continua approach to FEA. As with the statisticians, this relationship with engineers continues to this day with my honorary appointment as Professor of Bioengineering at the University of Hull and new collaborations with Biomedical Engineer Professor Michael Fagan and his staff and research students.

Integration with other disciplines has taken me into areas that questioned the (then) standard ways of thinking about evolution. They involve new integrations of structure, function, genetics and development in evolution, an approach to evolutionary biology that is sometimes called evo-devo. A Chicago undergraduate research student (now Professor at Johns Hopkins), Rebecca German, who has maintained contact with me and in recent years visited Australia many times, has been seminal in this regard. So too, has been another frequent visitor to Western Australia, Professor Brian Hall, of Dalhousie University, of whom it can be said he is one of the 'fathers' of 'evo-devo'.

Most recently of all, my collaborations have involved computer mimicry of evolutionary processes. This has depended upon Dr Ken Wessen, originally my doctoral student in theoretical evolutionary biology but who already held a PhD in theoretical physics. His first draft of a Research Master's Thesis in Human Biology

easily transmuted into a Doctoral Thesis worth a Distinction, and then into a Cambridge University Press book: 'Simulating Human Origins and Evolution' (Wessen, 2005). Now with a job in industry, he earns far more than any professor; he continues this modelling research; it is nothing to do with industry; it is how he gets his intellectual jollies!

Alongside this complex set of scientific relationships, and probably because my early education included a medical degree, I have also always kept a weather eye open for medical problems. As a result I have become involved in such topics as vitamin B12 deficiency syndromes, mechanical efficiency of bone prostheses, bone structure in relation to gravitational physiology, early diagnosis and late fracture implications of osteoporosis, anatomy, function and disease of the human incus, and bone growth impairments of iodine deficiency, hypothyroidism and especially the complications of cretinism. Much of this work has been underpinned by extensions of my interests in the mechanical efficiency and adaptation of spongy bone. Some, the vitamin B12 studies and the iodine deficiency effects (with Peter Obendorf and Ben Kefford), were totally serendipitous.

It sometimes frightens me that so many contacts like the above were made serendipitously. How many even better contacts have I missed because of the individuals that I did NOT happen to meet? Indeed, my 'major professor' Lord Zuckerman once hinted to me that I was way behind, that there were far better techniques that he knew about; but that, as Chief Science Advisor to the government, he could not tell me about them; they were classified!

Always these collaborations require that I thank the nimble minds, nimbler than mine anyway, of a series of research students, post-docs and other colleagues over the years, and, happily, this continues into the present. Of course, one usually thinks first of the well-known seniors who were responsible for one's career. Yet my initial stimulus came from the enthusiasm of an unknown headmaster of a tiny primary school in a small Scottish village during the second World War. There was no science in the classical curriculum in Scotland then. But he 'knew', somehow, that this small boy was interested in science. He introduced me to the ideas of

Wegener, Goethe, D'Arcy Thompson and Solly Zuckerman when I was 9 years old!

As a result, by 1952, I may have been the only person in the world who knew about the movements of the continents but who did not know that Wegener's ideas were not accepted for almost half a century. By 1952 the ideas of plate tectonics had become centre-stage. I could not understand what all the excitement was about. I had always known it was so. Likewise, I understood very well that the skull was simply a series of fused vertebrae. It made sense to me. I did not know that Goethe had it wrong until, in 1955, I came to read Gavin de Beer's tome on the vertebrate skull. The new developmental studies in this last couple of decades show that Goethe was a little bit more right than most of us thought, though, of course, for the wrong reasons. I certainly did not understand the mathematical formulae and Greek quotations in Darcy Thompson's 'On Growth and Form'. But there is much in that book to touch a nine-year-old. His pictures of the struts in the interior of a bird's wing were so like the struts between the wings of the biplanes of my childhood. The relationships between stress and architecture of the bridges that he presented were so obvious in the bridge over the Forth that was just down the Firth from my Scottish childhood home. His Cartesian coordinate transformation diagrams of biological forms were fascinating, even to a small boy. His pictures have been with me all my life. They have figured in my investigations even recently through thin plate splines, morphometrics, biomechanics, Fourier transforms and tensegrity towers.

And finally, of course, how could I have known that I would later actually work with Dr Solly Zuckerman, author of that fourth text *Functional Affinities of Man, Monkeys and Apes* (1934) that my headmaster gave me all those years ago?

I thus also owe much to famous people, especially to Lord Zuckerman himself; I think I was his last full-time student. Eric Ashton and Tom Spence with whom I worked in Zuckerman's Department of Anatomy at the University of Birmingham were constant exemplars, colleagues and friends. Indeed, I owe much to that entire department in Birmingham, even to the wider range of academics

with whom Zuckerman was associated in those seminal years. All were critical to my own scientific development. When Zuckerman retired (from the chair in Birmingham, he never retired from problem-solving until his death in 1995) his efforts at the University of Birmingham had grown Anatomy from about six academics in 1945 to, at the time of his retirement in 1966, as many as 53. In those few years, he had increased the department's funding from the widow's mite of a typical anatomy department to one that was third largest in the university, being exceeded only by physics and chemistry. He had moved the department from being almost solely occupied with teaching medical anatomy to medical students to one encompassing many other new 'anatomies'. These spanned not only the anatomy of humans, but also the anatomies of apes, monkeys, mammals, vertebrates and even many non-vertebrates. They included the anatomies of reproduction, development, growth and contraception, the anatomies of cells, sub-cellular organelles, membranes and molecules, and the anatomies of brains, behaviours, cognition and psychology. What other anatomy department of those days (or of any day) had biochemists, physicists, engineers, psychologists, schoolmasters, several FRS's, and several peers of the realm on its staff. And all this without the need to change the name!

Other academics outside the Birmingham department also influenced my career: Professor J. Z. Young who, together with Solly, examined me for my Bachelors degree in 1955. Professor Young:

'What do you see down that microscope my boy?'
'The substantia nigra, sir', I said.
Covering the microscope's eyepiece with his hand:
'Is the black stuff inside or outside the cells?' he asked.
Of course, I had no idea, I was not a brain person — but I gave an answer based on standard biological principles. I have never dared go back to see if my answer was right, though I suspect it was.
Solly and JZ roared with laughter. They *knew* I had guessed.

And there were others: A. J. E. Cave, Professor of Anatomy at Barts (St Bartholomew's Hospital Medical College of the old days).

He of Neanderthal fame! Alec Cave knew that the hunched posture and the supposed shambling gait, on its way to modern human walking, as it was supposed, of the brutish Neanderthal was just severe osteoarthritis in a skeleton of an old man. He examined me for my PhD.

'Did you write that thesis?' — indicating my doctoral volume on the table.
'Yes.'
'Then you pass' he said and immediately signed the examination form and appended:
'Highly Commended'.

For the next three hours we had a fascinating discussion about the great ideas and great people of anatomy in earlier days (that was the real examination!) And that discussion with him continued over the years. I had the feeling that he and I were those last anatomists to dissect the really large animals of this world — we had both dissected whales — I was not correct! He was a wonderful scientist who, though he did not know one end of a statistic from the other, understood intuitively just why we were using multivariate statistical methods to characterise animal form and pattern. He had his telegram from the Queen on his 100th birthday but died soon after. It is an especial privilege for me to be able to recognise his part in my life.

Zuckerman's departure from Birmingham in 1966 spawned a diaspora from Birmingham that continued for many years. I was the first emigrant myself in that same year. I then enjoyed further seminal decades at the universities of Chicago, Southern California and Western Australia. I have found, everywhere in the world, members of that original 'Zuckerman Mafia'. Each time that I have moved, I have, while keeping the old: old colleagues, old students, old problems and old techniques, also extended my grasp to the new; and all are part of my acknowledgement.

Perhaps, however, the people to whom I owe most, the people who have contributed most to my researches over the years, indeed, the people who keep me mentally alive even now, are none of the 'old

greats' or the 'accidental colleagues'. They are the 'new greats': the students and colleagues, mostly much younger than me, many of them now professors in their turn, in all parts of the world, some of them, however, still students, of mine, or of my students, or even my students' students. Most of them have shown that many of my original 'great' ideas could be 'improved upon'; were, in fact, 'not quite right'; were, actually, even, 'totally wrong'. Yet, a few of them have found that some of my earlier scientific heresies were not so far from reality. It is through these people that I am still able to be involved in the new developments in our discipline. They keep me alive. They are really the ones to be thanked. The current crop are graduate students in Anatomy and Human Biology and in Forensic Science at the University of Western Australia, and in Anatomy and Bioengineering at the Universities of York and Hull, and the Hull York Medical School, UK. They all give me enormous pleasure and help. All are present in spirit as I look at my first 50 years in science, and as I contemplate the many research lines with which I will continue to be involved (hopefully) in the next decades as an honorary investigator.

It is worth recording what has happened to some new younger colleagues: honours students, graduate students and post-doctoral colleagues, in these last few years. Seven of them (all strong students with funding) have opted to leave academia because of the perceived lack of value that our society, our government, our country, even academia itself, places nowadays upon the research and teaching career. You would think this would be a worry; yet no one seems willing to do anything about it.