

Out of Africa into the wilds of science

By Joe Landsberg

There are all sorts of ways of telling a life story. Since this one is focused on my path from farm boy to a player – albeit a modest one – in the international science game I will, obviously, focus on my education, the contacts that enabled me to get into science, and the various jobs I held as I scrambled my way up the ladder. That will leave out great parts of the personal side, of the difficulties that my pursuit of this particular career caused my wife as she carried the main burden of bringing up four girls in several different countries, often, particularly in the early stages, with very little money. But I have written about all that in detail elsewhere, and she's not going to demand further recognition here. She's a brave and loving and able lady, and we notch up 60 years of a successful marriage in 2023.

The farm I grew up on was in Rhodesia – the country now called Zimbabwe. My father grew tobacco in the post WWII years and, for a variety of good reasons, like lack of capital, it wasn't a particularly successful enterprise. But the farm was 3500 acres of beautiful African bush and savannah; a great place for a boy to roam with an air rifle – and later more lethal guns, with which I did considerable damage to the local wildlife. I regretted the shooting as I reached adulthood, but it was very much part of the culture. I didn't go to 'proper' school until I was eleven years old; my mother taught me and she obviously did a good job because when I did go to boarding school, in Salisbury (now Harare) I was at no disadvantage. (*Fig. 1*)

My academic results through school were marginal, but it didn't seem to matter much—there was no prospect of further education; I knew nothing about universities and Rhodesia didn't have one, so I got a clerical job in the government, in Salisbury, and spent a lot of time playing cricket and tennis and shirking my extremely boring work. I finally realised that there was no future in that job or way or life, a sentiment shared by my boss who was glad to see me go. But the question was: go where? To what? The possibility of university in South Africa came up in family discussion; my mother was a graduate—a rarity in our society—so she knew what was involved. The problem was that I had a very undistinguished school record with end results that were only just good enough to get me into university. And then there was the question of what to study and how to pay. In the end I applied for entrance to the University of Natal, Pietermaritzburg, to study agriculture. My parents,



Fig. 1. Early years on the farm: our house; my mother and one of her sisters; my younger brother.

very nobly, found the money to cover my first year, but that was all they could do. I was accepted by the university, and off I went into the unknown at the start of the next academic year. I scrambled through that first year, and got government loans to cover my costs from then on. It was a four-year degree and, to everyone's surprise I ended up doing well. I was also engaged to marry a beautiful girl.

Finding a job back in Rhodesia was easy. I was white, in a society where that was a guarantee of privilege, and well educated. I got a job as a junior research agronomist in the Department of Research and Specialist Services (DRSS) of the Rhodesian government and was posted to a place called Matopos Research Station, in the south of the country, near the city of Bulawayo. Matopos was 60,000 acres of semi-arid bush and savannah; near a national park, a wild area characterised by massive granite hills (known as 'kopjies')¹. It was a great place to work. (*Fig. 2*) The main focus of the research station was rangeland and livestock management, but there was a small team of three agronomists—of which I

¹ Pronounced 'kopjies'; an Afrikaans word



Fig. 2. Diana and me on top of “World’s view”; Matopos hills.

was one—with several technical assistants and a number of laborers to help run the various trials and experiments that we carried out. I lived in bachelors’ quarters until I was married, in February 1963, when we qualified for a house on the station.

The Agronomy group was responsible for research on crops that could be productive in that environment—mainly sorghums (1), millet, and some varieties of short-season maize (corn). It was a summer rainfall area, with wide variation in annual totals and length of the water-controlled growing season. Our research was concerned with the management of crop cycles to optimise growth during the periods when water was available, and ensure some yield in poor years. Besides my duties helping run and analyse field trials on the grains, I set up my own project on the water requirements and irrigation of alfalfa, which led to some of my first published papers (2, 3) and provided the basis of a masters degree by thesis, as an external student at my alma mater. The work on crop water requirements led me to study the environmental factors that affected plant water use, subjects that had not been covered in my degree, but which I found immensely interesting—an interest that carried through my whole career. I measured ground cover, described root distribution profiles and assessed plant water status using relative turgidity. I also came to recognize the importance of radiation absorption as the main driver of crop growth, a recognition that carried through into much of my later work. (*Fig. 3*)

But, although I enjoyed the work, and the environment, I was beginning to realise that, if I stayed at Matopos, there was little prospect of a research career that would allow me to test myself against the science being done by people at internationally recognised institutions. DRSS was too small and not



Fig. 3. Precision irrigation, African style. Water flowed through holes in the boom, which was carried back and forth across the plot to apply water evenly. Flow rates were calibrated and amount applied determined by timing.

funded for any sort of basic or explanatory research—it was a service organisation in a small country in the middle of Africa. We had only very basic equipment—for example my radiation data were calculated from relationships between sunshine hours and incoming radiation.

While we were at Matopos we had a visit from the first internationally established scientist I had met: Dr J.S.G.(Jim) McCulloch, a physicist and senior scientist at the East African Agriculture and Forestry Research Organisation (EAFRO), who was working on methods of measuring plant water use. McCulloch's visit was to be very important for me, few years later.

As part of the requirements for my Master's Degree I had to attend a week of seminars and discussions at Natal University, where I met the senior agronomist for the South African Sugar Association (SASA), Gerald Thompson, who was doing his PhD on sugar cane water use. Because of my interests the meeting resulted in a job offer from the SASA, in Gerald Thompson's Agronomy section. Thompson gave me to understand that the SASA was far better equipped and would offer me the opportunity to

do much more basic research than we could do at Matopos. So we moved to Natal, to the large and impressive sugar research institute at a place called Mt Edgecombe, north of the city of Durban. Just in passing: one unforeseen benefit of the move to South Africa was that I avoided being called up for military service in the ugly war for independence waged in Rhodesia in the late 1960s and early 1970s by the African nationalist movements. I would have been involved had we stayed in Rhodesia. I did not deliberately dodge active military service, and but it wasn't something I was looking for.

Scientifically, the move to South Africa was a mistake. Gerald Thompson misled me. He was an ex-airforce officer, a martinet who was not interested in allowing me any leeway to do the kind of science I wanted to do. They did indeed have better equipment than I was used to: radiometers (the first I had seen), small anemometers for wind profile measurements, soil moisture measuring equipment (neutron probes) and a couple of massive weighing lysimeters that could give daily water use by the cane crop. But my dreams of 'getting my teeth into' explanatory science were smartly knocked on the head: I was required to do service research—white peg agronomy. Furthermore, SASA was not interested in publications in the scientific literature. I did 'moonlight' some studies on cane microclimatology and analyses of growth in relation to water use and radiation absorption, and worked to educate myself in environmental physics and the effects of weather on plant growth, but there was no question of being allowed to submit anything to journals. The situation is perhaps best summed up by the fact that, working from a research station in the 'back blocks' of Rhodesia I published five papers in the two and a half years I was there; three of them were in good quality journals. I published no papers after four years at the SASA

It became clear, after a couple of years, that the job was a dead end, and that if I wanted to do scientific research I would have to move on. So I started to look for opportunities. I hoped to get a chance to go overseas, but made no progress, until a glimmer of a chance was provided by another visit from Jim McCulloch, who had become the director of the Institute of Hydrology at Wallingford, in England. Jim came by to see an old friend and colleague of his from EAFRO, and remembered meeting me at Matopos. I asked him if he knew of any opportunities for me in the UK. He was not hopeful—I did not have a PhD and had a slim publication record—but he said he would 'keep an eye out' for anything that came up.

I assumed that would be the last I would hear of it, but I totally misjudged Jim McCulloch. Some months later I had a short letter from him saying that a bright young scientist called Paul Jarvis had just come back to the UK from a post-doctoral spell in Australia, and had landed a very big research

grant from the Natural Environmental Research Council (NERC), which funded McCulloch's institute. Jarvis was building a team to study canopy processes and productivity in Sitka spruce, near Aberdeen, in Scotland. McCulloch suggested I might write to him, although he repeated that I shouldn't get my hopes up as I didn't have the qualifications Jarvis would be looking for. So I wrote to Paul Jarvis and, in due course, to my astonishment and delight, received a letter from him offering me a one-year trial to study canopy processes with his team. It seemed that Paul could not find anyone, from anywhere around the world at that time, who had the qualifications he was looking for, and he persuaded the panel that was monitoring and overseeing his program to allow him to give me that one year, so he could get on with the project. During that year he would look for someone better qualified. This was an entirely justifiable position: I knew nothing about tree physiology and on the basis of my qualifications and cultural background (I was completely unfamiliar with the British academic approach to science) could have been a complete disaster for the Aberdeen group.

So, to the alarm of my parents-in-law, Diana and I with our four small girls, embarked on a ship to the UK and thence by road and rail to Scotland. We had never been overseas before, and had to find Aberdeen in an atlas. When we got there, it was a major culture shock, as well as a shock in terms of the weather, which was quite unlike anything we had ever experienced. We also struggled financially, since my fellowship was intended to cover the living costs of one person—not six.

From the science point of view, I was indeed underqualified for the project and had to work very hard to make a contribution to it. But Paul kept me on after the first year: he found someone else much better qualified on paper, but that person did not fit well into the team—he came for a while and then left, while I stayed. I was very keen to do my PhD at Aberdeen, but the university regarded me as an alien and liable for aliens' registration fees. There was no money available for that. I did some respectable research; went to my first scientific meetings in the UK, and my first international meetings—in Czechoslovakia, in September 1969; a famous meeting held in the shadow of the Russian invasion of that country, and in Sweden in 1970, where I presented my first paper to an international audience. At those meetings I met and got to know many of the leaders in our field of environmental physiology, as we were beginning to call it. I had arrived where I wanted to be—in the community of science. Whether I belonged there was another matter. I was starting to publish regularly; respectable papers if not earth-shattering, so I was on my way.

Paul Jarvis was brilliant, driven, slightly erratic and not always easy to work with—but that probably cut both ways. Despite a few 'bumps in the road', we became firm friends, a relationship that lasted

for 50 years; the rest of Paul's life. He was already established, believed completely in the principles of open science and worked very hard at maintaining his network of contacts. I benefitted from that. I learned a great deal about tree physiology and the physics of canopy-atmosphere interactions, usually managing to keep up with current knowledge. During my time at Aberdeen I met some very good scientists, mostly post-doctoral, who came to work with us for varying periods: John Norman; Merv' Ludlow, Neil Turner, Mike Beardsall and, importantly, Sune Linder. Sune was completing his PhD at Umeå in Sweden and became a close personal friend. John Monteith, the doyen of our field, was a member of the NERC panel with oversight of the Aberdeen project and came up several times; I also met him at scientific meetings and got to know him well enough to call him a friend, although I would not claim he was a close one.

After almost three years it was clearly time for me to move on, not because of any problems with Paul or the work of the team, which I enjoyed and was getting comfortable with, but because I had a young family and I needed a permanent job. Paul offered me at least one extra year but, unfortunately, it wasn't enough. I was now well enough known in the UK science community to 'put it on the grapevine' that I was looking for a position. Monteith indicated a couple of possibilities to me and I ended up taking my first team leader position at Long Ashton Research Station, a unit of the Agricultural Research Council in the west of England, with affiliations to the University of Bristol. I headed a new group called the Microclimatology Section, concerned with the effects of weather on the growth and productivity of apple trees.

My team at Long Ashton was initially three people, but grew to be six to eight, depending on whom we had as visitors and students. Our work was centred around one heavily instrumented orchard, but various members of the team carried out a variety of subsidiary projects. Because the Research Station was affiliated to the University of Bristol, I qualified as a staff member and was able to register for my PhD. (This had actually been a condition of my employment: the university panel that appointed me was unhappy about the fact that I did not have a PhD, and stipulated that I should work towards getting one.) To get that out of the way, I registered quite soon after I took up the job and was able to use some of the work we were doing towards the degree, which I completed successfully in three years.

I spent 1976 at the University of Western Australia, following an invitation from a professor at that university, whom I had met in Sweden in 1970, to teach micrometeorology/environmental physics to students of agriculture in the later stages of their degrees. That was hard work, but in other ways the

year was very productive. I collaborated with a brilliant mathematician, who was interested in applying his skills to a new area, in an analysis of water movement through plant roots. This led to a paper (4) that I think was one of the best things I ever did scientifically, although its impact wasn't enormous. I also published several more papers (5,6,7) from collaboration with people in Western Australia. During the mid-year break in Western Australia Diana and I, with our children, drove right across the country, visiting scientists at various institutes and universities in the eastern states. The contacts made on that trip were to be very important later. We also did a short trip to New Zealand.

At Long Ashton I organised a successful international conference on 'Environmental effects on crop physiology', the proceedings of which were published by Academic Press (8). I also kept in touch with the forest physiology community, some of whom, in 1973 or 4, organised a 'moving meeting', which visited several locations in the UK where research projects on exchange processes and tree physiology were being carried out. At that conference I met Professor Richard (Dick) Waring, from Oregon State University. We got on well together and established a friendship that has lasted until now—50 years. Paul Jarvis and Sune Linder were also at the meeting, which saw the beginning of a relationship between the four of us that was not only personally important—good friends are always to be valued—but, we liked to think, was important in the development of our field of forest ecophysiology. (*Fig. 4*).



Fig. 4. Left to right: Paul Jarvis, Joe Landsberg, Dick Waring and Sune Linder at a post-graduate course in Estonia, 2006.

In 1973 Paul Jarvis brought his whole group down to Long Ashton and, together with my group, then seven people, we carried out a highly intensive day/night study at our orchard site, comparing the performance of instruments and equipment as well as obtaining the most detailed data ever recorded on the interaction between orchard trees and their environment. The project was fun as well as scientifically rewarding (9).

A project of some note that I was responsible for from Long Ashton, was funded by NATO (most people were not aware that NATO provided some science grants, apparently to encourage collaboration and understanding among its members). We worked with a French group based in Montpellier, in the south of France. At Long Ashton we grew 400 young, clonal apple trees in large (approximately 60 litre) garbage cans, all filled with a uniform soil. After a couple of years, we trucked half those trees to Montpellier and, over the next two years, carried out a series of matched measurements, such as leaf

photosynthesis, various water relations measurements, careful growth and fruiting observations etc. at both sites. Then all trees were harvested and their top growth and root mass determined. The contrasting climates in western England and the south of France provided us with excellent data on the growth, productivity and carbon partitioning of apple trees. Members of the collaborating groups wrote a number of papers from that project (see, for example, 10). (Fig. 5.)

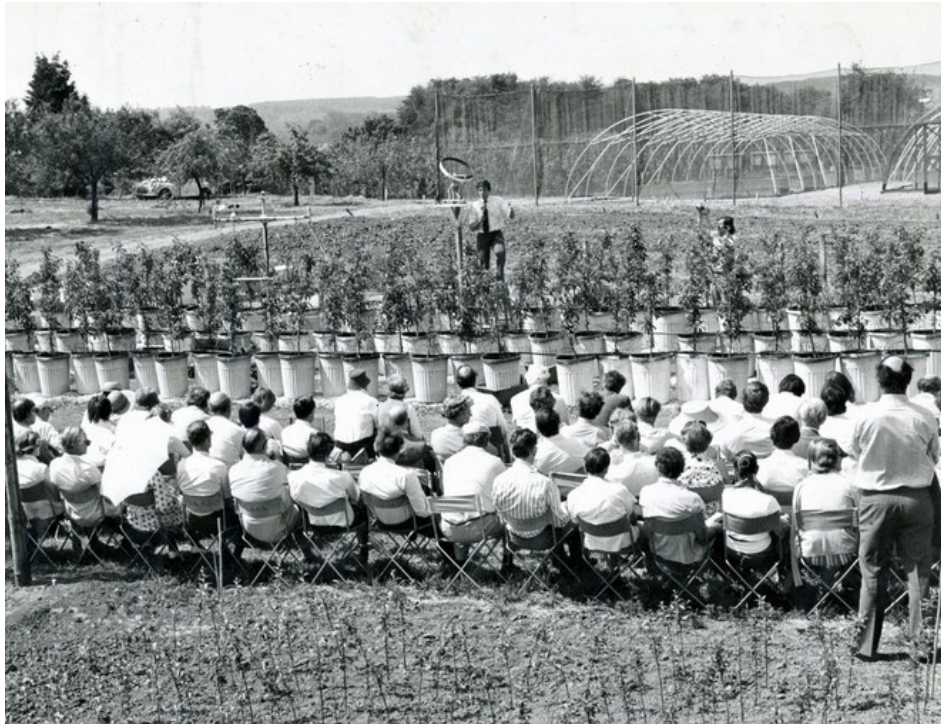


Fig. 5. Addressing a group of apple growers about the NATO-funded project. The trees in bins are those that stayed at Long Ashton; they later grew considerably bigger.

I spent 10 rewarding years at Long Ashton: my group was productive and we all, as well as students, and I think our various visitors, benefitted from the work we did together. But in 1980, while at the University of Sydney as the Lawrence William Paulett Scholar, in the Department of Agriculture, a short-term (3-month) scholarship, I was interviewed in Canberra for the job of Chief of CSIRO's² Division of Forest Research (DFR). This came about because I had applied for the job, following encouragement from a friend, respected scientist Professor Fred Milthorpe, who was at Macquarie University. To the surprise of many, and some consternation among Australia's forestry community, I was appointed to the position.

² Commonwealth Scientific and Industrial Research Organisation

The Division—more than 200 people, based mainly in Canberra but also scattered in regional stations in the states (Australia is a federation, like Canada) was by far the largest research group concerned with forests and forestry in the country. For that reason, the job was quite political; it was a very steep learning curve. My appointment was for a seven-year contract, with guarantee of subsequent employment. The foresters and state forest departments were unhappy about the fact that I had no background in forestry, Australian or otherwise, seemed unlikely to direct the division along traditional paths, and possibly might not be amenable to pressure to do the kinds of research they wanted—largely conventional mensuration in their areas (they wanted a service from the Commonwealth), as well as work such as evaluation of harvesting equipment. They were right: my ideas about science and what science in forestry should be about were very different to those of most of the forestry people. As chief of that research group I was an important player in forest research in Australia and sat on various committees and advisory panels where I often had to defend my policies and the direction I was taking the division—not to mention private conversations and interactions. But DFR made considerable progress in those seven years.

Among the appointments I was able to make to help me change the direction of the division was Sune Linder, (Fig. 6) who made a great contribution to our work. Sune only stayed with us for three years; he was then offered the Chair of Forest Ecology at Uppsala—his alma mater—and I could not expect him to turn that down. He was instrumental in getting a major, detailed irrigation/fertilisation experiment—modelled on one he ran in Sweden—up and running. He provided the leadership and organisation; I provided funding and pressures on staff to change their directions and work with him. The success of that experiment, which eventually ran for 30 years, can be seen by reference to the volume of Forest Ecology and Management devoted to it (11). Paul Jarvis also came and worked with



Fig. 6. Sune Linder was instrumental in designing and maintaining long-term experiments in ecophysiology in Sweden and Australia.

my physiology group for six months, and I appointed Chris' Beadle, who had been one of Paul's post-doctoral students, to strengthen the group in Tasmania. Early in my tenure I organised a conference, including CSIRO, state and university forest scientists, to try to get everyone one 'on the same page' (12); I also wrote a book to outline the state-of-the art of forest ecophysiology, as I saw it at the time, as a guide to my philosophy (13). I attended meetings overseas, including one run by Dick Waring in Oregon, and spent three months in Tennessee with Bob Luxmoore at Oak Ridge to help him write up an IUFRO conference (14). There were other conferences and books.

At the end of my contractual period CSIRO decided that the Division of Forest Research was to be amalgamated with the Division of Wood Technology to create the Division of Forestry and Forest Products. I declined the opportunity to apply to be chief of that division: I knew nothing about wood processing and was not interested in it, and I knew that the forest industries would oppose my appointment and make life uncomfortable. So, I stepped down and spent a few years doing research in the Division of Wildlife and Ecology, then as Director for Natural Resources Management in the Murray-Darling Basin Commission, the technocratic body charged by the Federal and state governments to oversee development and management of the huge catchment area that contains much of the most important agricultural land in Australia. Then, on Dick Waring's suggestion and supported by him, I negotiated for, and was appointed to, a position as a Program Manager/advisor in NASA's Terrestrial Ecology Program, in Washington DC. It was not a successful period, of little value, I suspect, to NASA or to me. I was unfamiliar with the American science culture and how things worked and, since no-one briefed me or discussed with me what I was expected to do, I was ineffectual. However, I was involved with the remarkable BOREAS program, met a lot of interesting people and learned a great deal.

Returning to Australia I re-joined CSIRO for the last period of my career. I was able to organise funding for Dick Waring to come and work with me, and together we wrote the 3-PG model, fulfilling aspirations long-held by both of us to write a forest model driven by physics and physiology, but producing output of value to forest managers. We worked well together and the model we produced was successful from the beginning, made more powerful and of general value by Nicholas Coops, who translated the code into a form that could use remotely sensed data, so the model could be applied over wide areas. My friend and colleague Peter Sands, In Tasmania, wrote excellent software and we made it freeware to all who wanted to use it (see the 3-PG website, University of British Columbia).

After Dick returned to the US, I spent three months in Finland, as a visiting professor at the University of Helsinki. While I was there, CSIRO told me they would like me to retire, basically because I held a senior position and was expensive, and they had made no provision for the years up to normal retirement age (I still had five years to go). I decided there was no point in arguing about it, so I took retirement. The Finns elected me to the Finnish Academy of Science and Letters, which was very pleasing.

The following years, working from home, were scientifically productive. I wrote a number of papers and taught a series of post-graduate level courses on forest ecophysiology that Sune Linder, with funding from the Nordic Foundation, organised in the Nordic countries, Estonia, South Africa, Chile, and Brazil (*Fig. 7*) Dick and Paul also taught at some of those (highly successful and rewarding) courses. I went to South Africa to teach there and also to a IUFRO meeting in the famed Kruger National Park (1997) in which Sune, Dick, Paul and I all participated.

In 2000 the University of Christchurch at Canterbury in New Zealand offered me a three-month fellowship: the Erskine Fellowship, in the Department of Forestry. I wrote a book with Peter Sands (15), a solid piece of work which I am very comfortable with, and another, much smaller, book with Dick (16), aimed at a very different readership. It has not 'set the world on fire', but I am happy with it and glad we did it.

In 2020, to my astonishment and delight, Dick, Nicholas and I were notified that we had won the very prestigious Wallenberg Prize, awarded by the Marcus Wallenberg Foundation in Sweden. It is said to

be the nearest that forest science gets to the Nobel Prize, so it has been a gratifying end to my career as a scientist.



Fig. 7. The teaching group at the Brazilian post-graduate workshop in 2009.



Fig. 8. All of the post-graduate courses contained both field and laboratory segments (Brazil, 2000).

It will be clear, from this condensed account, that I have been able to fulfil the aspirations that started to develop in the ‘back blocks’ of Rhodesia back in the 1960s: I found I could compete in the world of international science, and make some modest contributions. It has been a long, and sometimes rocky road but, while there are things I regret and would like to have done differently, or not done, I don’t regret any of the major moves—from Africa (Fig. 9) to Scotland to England, to Australia; the working periods in the US and New Zealand and Finland. I am fortunate in my magnificent family, and it’s been satisfying being able to utilise the skills that I have. I also have as friends some great people, most of whom are scattered around the world, but it’s easy to keep in touch, at least occasionally, with modern technology.



Fig. 9. In 2015, I went to Rhodesia (now Zimbabwe) to take part in a game count.

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