

The Nutrition Society 1941 – 1991

*Presidents and Honorary Members:
Their Stories and Recollections*



Compiled by
Elsie M. Widdowson

The Nutrition Society

1941–1991

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Elsie M. Widdowson CBE, FRS, DSc

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The Nutrition Society

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Preface

In 1970 Drs A.M. Copping and E. Cruickshank made some tape recordings of the reminiscences of British investigators who had contributed to the science of nutrition. This was done at the request of Dr W.J. Darby of Vanderbilt University, and the tapes were sent to the USA. Among those who recorded their recollections were two past Presidents and one Honorary Member of the Nutrition Society, all of whom have since died. At the International Congress of Nutrition in Brighton in 1985 I asked Dr Darby if he would send me copies of those tapes. This he did, and I should like to thank him for his co-operation. The recordings were transcribed, and they formed the beginnings of this little book. Five past Presidents and other Honorary Members subsequently made recordings, and six others made their contributions in the form of typescripts. In 1981 Dr E.H. Kodicek made a tape recording for the Archives of the Imperial War Museum, and I have used this to set out in his own words the description of his early life and escape from Czechoslovakia. The stories of the other five past Presidents have been written by colleagues who knew them well. I am very grateful to Professor R.A. McCance, Dr A.M. Copping, Dr M.E. Coates and Dr W.F. Cuthbertson for all they did to help me to complete the collection of stories of past Presidents of the Society.

I also thank Dr J. Hammond Jr. for helping me to write about his father, and Mrs J. Kodicek and Mrs Ivana Adie for the trouble they took to ensure that my transcription of Dr Kodicek's tape recording was correct and to put it right where it was not. Dr Margaret Ashwell provided assistance with the transcription of the 1970 tapes, and Mrs Daphne Tabor was responsible for all the final typing. I thank them one and all.

Mrs June Schulkes, Executive Secretary of the Society since 1968, has been a guide and counsellor to eight of its Presidents. We all owe much to her. I thank her particularly for all the help she has given me in the preparation of this book.

I hope that the stories and recollections of those who have been regarded sufficiently highly by members of the Nutrition Society to have been elected Presidents and Honorary Members will provide interest and inspiration to present members, and to others who are concerned with the history of the science of nutrition over the past 50 years.

Elsie M. Widdowson
November 1990

Lord Boyd Orr

Director,
Rowett Research
Institute, Aberdeen
Chairman 1942–1945



I began experiments at Glasgow University before the First World War. I had a very good education. I took science and then I took medicine. After eight years I was still at Glasgow. I was offered a research scholarship in physiology and I began to study the metabolism of protein. At that time all that was considered necessary for a diet was calories for energy, and protein.

After the war I went to Aberdeen to establish a centre for research in the

nutrition of farm animals, and that became the Rowett Institute. There were four main lines of inquiry. One was in grazing animals. We analysed grasses from different parts of the world, from the Falkland Islands to the north of Europe, and we correlated the analyses with diseases that occurred in grazing animals. We discovered that some diseases were due to deficiencies in mineral salts in the grasses. There was a deficiency of phosphorus in grasses from South Africa, and of calcium in western areas of the world.

Another line of work was on pigs. The pig is a very good experimental animal because it grows so rapidly and has the same kind of diet as human beings. Any kind of deficiency shows up quicker in pigs than in other farm animals. We discovered that diseases like rickets in pigs could be cured by adjusting the diet. By this time newer knowledge about deficiencies of vitamins and minerals was available to us, and we could apply this to farm animals.

Then, being trained in medicine, I realised that some of these diseases in animals were similar to diseases in human beings, and I began an inquiry as to whether the diseases in human beings were due to the same deficiencies as in animals. At the Rowett Institute there was no authority to work on human nutrition, but David Lubbock, who had been trained by Gowland Hopkins in Cambridge, didn't need a salary, so he was able to do this work. We wanted to find out if the health of children could be improved by improving the diet. We started experiments in Belfast in Ireland. The children were given a pint of milk a day at school, and other children were given a biscuit with the same calorie value as the milk. We discovered that the children began to grow faster, and the addition of a pint of milk a day made a big improvement in their rate of growth and general health. People didn't believe this and said it was done to help farmers. So the Department of Health got their experts to make an experiment to see if they got the same results. They found exactly the same thing. They published a paper showing that the consumption of milk had a marked effect on the health of school children. We showed these results to Walter Elliott, who was a Cabinet Minister. Dr Elliott had worked at the Rowett Institute long before this, where he had got a DSc, and he became a Fellow of the Royal Society. He introduced a Private Member's Bill in Parliament, enabling Scottish local authorities to provide free or cheap milk for children in schools. Then the next year there was a Bill for England, and that started the Milk in Schools scheme.

In the 1930s farmers were in a bad way and were going bankrupt. The Government set up Marketing Boards with power to limit the production of food and raise the price of food to make it profitable for farmers to produce it. That didn't work very well, so a Committee was set up to inquire into the work of the Marketing Boards. I was put on the Committee, and I was always pushing the Committee to agree that instead of the profit of farmers, what we should be concerned with was the amount of food that was needed by the people in the country, and to fix the price at one that the poorest people could

pay. I was very unpopular with the Civil Service. We went to the Ministry of Agriculture and Food and told them that prices were limiting the amount of food people ate. We suggested that there should be an inquiry into how much food was needed in the country, and what price the working classes could pay for it. The Ministry agreed that this was a good thing to do, and that it should have been done before the Marketing Boards were set up. So a dietary survey was carried out on about 1000 families in Britain, and the results showed that about a third of the people were so poor that they could not have bought enough food for themselves, even if they had been told by a nutritional expert what to buy. The diets were also deficient in some of the minerals and vitamins, and the health of the community and the incidence of deficiency diseases corresponded with the nature of the diet. Then we worked out the amount of additional food that was needed and the price that was needed to pay for it. Lord Linlithgow, who was the head of a Board limiting the import of food to keep up prices, joined forces with us. Then when the Report describing the survey was going to be published, the Civil Service tried to stop the publication of it. Knowing that there might be difficulty in having it published I went to the British Association meeting and I saw my friend Ritchie Calder, now Lord Ritchie Calder. At this meeting I gave the results of the survey, and then I went to see Harold MacMillan the publisher, who was the Member for Newcastle. He said 'I'll publish it for you'. So he did, under the name of *Food, Health and Income*. It went through several editions, and it was published in America.

Then I became involved with Lord Bruce of Melbourne, who was invited to be a guest speaker at an endowed meeting. I suggested that he should talk about the world food position and the effect that it would have if the food supplies were doubled so that there was sufficient for all the people in the world. He made a magnificent speech which was reported in all the Press. Lord Bruce then went to the League of Nations in Geneva as Economic Adviser. Sir Walter Elliott and his Undersecretary, Lord Delaware, went too and at first heard that nobody would be interested in our ideas. However, when the meeting started, ten minutes late, everybody was so interested in what Lord Bruce had to say that the meeting went on for three days. The League then decided that a Board should be set up to consider, first, what food was required by people for health. I was on that committee and then another Board was set up of farmers, economists and scientists to consider what policy was needed to carry this through. It was intended to provide additional work and to reduce unemployment. The League of Nations set up another Committee, the Mixed Committee, which produced a Report which the *New York Times* said was a best-seller. It is interesting to note that in this Mixed Committee Russian scientists, American scientists, British and French scientists, and scientists from many different countries were unanimous – they were all working together.

When the war broke out I was made a member of the Cabinet Committee

on Food. I went to America first, and made contact with American scientists who had been at Geneva. President Roosevelt called a conference, the Hot Springs Conference, and Lord Halifax, the British Ambassador, was asked to send me to it; but our Government said they would not do that because I would give different evidence from what they had told their delegates to say, and it would only cause confusion. I quite agreed! At the Hot Springs Conference it was said that an Agriculture and Food Committee should be set up by the League of Nations. I had been getting reports of the Hot Springs Conference through the diplomatic bag, and was horrified to find that they had recommended that a new branch of FAO should be set up to deal with research, with 200 research programmes to carry out.

Then a Member of Parliament and a Nobel Prize winner was to lead the British delegation at Quebec, and he wanted me to go with him. I wouldn't go as a member of the delegation if I was not allowed to speak. My wife told me I ought to go and meet some of the scientists from the British Empire. So I said I would go for three days. The Canadian Prime Minister was Chairman, and he said the Conference would like to hear a speech by me. I was very angry, because they were side-stepping the big problem, and I made a fiery speech. At the end of it they said, 'You must be the Director of the new Board of FAO', and I refused. But Dr Boudreau said, 'You have been fighting for this all your life. Now you have a chance to carry it through and you refuse!' So I had to accept being Director-General of FAO.

When I went to the office of the Hot Springs Conference I looked at the scheme for research and put it into the waste paper basket. I set all the staff to find out what the food position was in all the countries of the world, and at what rate the population was increasing to try to find out the quantities of food that would be needed. That showed that there was a food shortage. This was because during the war food production in Europe had decreased. Before the war Europe had produced more wheat than America and Australia put together.

I then wrote to the Government and asked them to send representatives to a Conference in Washington to consider the food position in the world, and what action should be taken by the nations of the world to avoid a world famine. That conference was held and Fuller, the great food expert of America, who arranged for food to be sent to Russia after the First World War, came and spoke. The Ministers for Agriculture of America, Canada and Australia all came to the Conference, Britain did not send anybody to it. So I had to fly the Atlantic and see Clem Attlee the Prime Minister, and he said he had never heard of FAO. Nobody had told him anything about it. So I explained what the position was and he said, 'I shall certainly send a representative'. So I flew back, and the Conference was held. It was decided that the problem was so urgent that another temporary Food Board should be set up to control all the food coming into the world market and direct it to the countries most in need of it, irrespective of their purchasing power. A representative from Britain did

come to the Conference, but by the time she came the Conference was finished, and when she did come she said she knew nothing about it! So for three years that Board handled all the food; for example America bought all the sugar that came on the market in the name of the Board, and the sugar was distributed in the same way as surplus wheat and milk. The action of that Food Board saved millions from dying of starvation after the Second World War.

The report of the first Conference in Washington was sent to Governments in different countries connected with FAO, and they were asked to send delegates to a conference in Copenhagen. This was six months after FAO was established, and there I put before them the proposition of a World Food Board which would persuade countries to double world food production. This would call for enormous quantities of industrial products, fertilizers, equipment and so on, and provide plenty of work to make good the unemployment that existed now the war had stopped. At that Conference in Copenhagen every country, including Russia, was in favour of it except Britain. Britain said food was a matter for trade, and this proposal should be the responsibility of the International Trade Organization. But food is different from trade, and food should be provided for people at a price they can afford to pay. So all except Britain were in favour, and we agreed to set up a World Food Board. Then I asked the Governments to come to a Conference in Washington to work out the details, and I got Lord Bruce of Melbourne to act as Chairman. But when we met in Washington the first speaker to get up was the American representative, and he said that Britain and America were not prepared to give any authority for funding an international organization for any such project. So that was the end of FAO for the time being. I immediately resigned but I couldn't get away for two years. It took a long time to appoint a new Director. Before giving up the Directorship I made friends with the Italian representative and got him to offer a big building in Rome that Mussolini had built to administer his colonial empire. We got this building for nothing, and all the help the Italian government could give. So I got FAO moved from the United States, where it was very much under the control of the Americans, to new headquarters in Rome. I resigned during the first week in Rome.

When I had been in America a long time before, I had visited the Agriculture Department and I found they had a wealth of information that was not available in London. They could tell me more about the Milk Marketing Board than I could find out in London. I am very much indebted to the Americans for the information I used in drawing up 'The Food Position of the World'. I received a cablegram from Whitehall asking if the Americans had any information about poisonous plants. So I went to the Department of Agriculture and asked for it, and they pulled out a file and said, 'There is a Report and that contains everything that has been published on the subject'. So I asked them if they had reports on other things, so they pulled out more files and said, 'Here are reports on every subject connected with agriculture and food'. I saw

what an excellent thing this was, and I thought that every worker that was studying the subject should take six months off and go to the library and read through all the papers he could find dealing with a subject, and write a short review of it. These should be typed and put in the library so that there was a summary of everything that had been done beforehand. Sir John Julius of Australia, who was visiting us, pulled out one of these files and asked what it was. I explained, and he said, 'That is what we need in Australia. Why don't we make this available for all countries?' So he took a copy back with him to a Commonwealth Agricultural Conference in London and said, 'This is what the Agricultural Bureau should be doing'. They said they would do it but they needed money. I thought it would be a good thing to do this at the Rowett Institute. I knew that a friend of mine, a wealthy man, was offering £5000 for a project at the Institute, and we had been talking about starting an international journal of nutrition. We could change this and make it an abstracting journal. I decided that £5000 was not enough, so I went to Morley Fletcher at the Medical Research Council and told him all this was being done for animal nutrition; it was up to him to give enough money to do it for human nutrition as well. So he agreed to make a contribution of £300 a year, but said he wanted someone to represent the Medical Research Council on the governing body. I then went to see Sir David Chadwick at the Imperial Bureau. I told him this journal was coming out and that the Medical Research Council wanted to be part of it. He also agreed to give £300 a year from the Imperial Bureau, and to appoint a member on the governing body. So we had £600 a year in addition to the original £5000, and the first number of *Nutrition Abstracts and Reviews* came out in October 1931.

Another journal I had some connection with was the journal of the Nutrition Society. Dr S.K. Kon of Reading wrote to me and asked me if I would give him a hand in starting a Nutrition Society and a nutrition journal. I went down, and we invited a number of the leading nutrition research workers for a meeting at the Royal Institution. We decided to set up a Society, and I was appointed to be the first Chairman, and Dr Cruickshank was to be the Secretary. The Society was founded, but it had no money. When I was in America I was invited to go and speak at the Mayo Clinic, the biggest Medical Research Institute in the world, and they gave me \$200. So I passed this on to the new Society to give them a start. The Society became a great success, and it is now one of the leading scientific societies in Britain, and the journal has an important place in scientific literature.

Since then I have been writing books, trying to promote my idea of co-operation of all the nations of the world to double food production so as to abolish hunger, and saying that the best way to do it would be to take 10% of the money that each nation was spending in preparation for war, and devote it to co-operation of the nations to save the world from hunger and malnutrition. That would have meant a change from conflict and war and destruction

to promotion of the welfare of the human race. I went on writing books and making broadcasts, and then I retired. Here endeth the first lesson.

Recorded with Ethel Cruickshank, 1970

Sir Joseph Barcroft

Professor of
Physiology,
University of
Cambridge
Chairman 1945–1947



Joseph Barcroft was born in Newry, Co. Down in 1872. His parents were Quakers, as was Joseph himself and his life was guided to a great extent by their principles. His formal education did not begin till he was seven years old, when he was sent to the Friend's School, Botham, York, and later to the Leys School, Cambridge. He did well, and he went up to King's College, Cambridge, where he graduated in 1896, having won a first class in both Parts I and II of the

Natural Sciences Tripos. As an undergraduate he had a keen sense of fun and a very old friend recalled one example. A very dull lecture was given every Monday, and the lecturer was often heckled a bit. On one occasion an organ-grinder came to play in the street under the window and all the undergraduates at once began keeping time with their feet. The lecture broke up in confusion when Barcroft slipped out to give the musician half a crown for his services! After teaching for a time at the Leys School, he returned to the Department of Physiology, and was Reader there under Professor J.N. Langley, until he succeeded him as Professor in 1926. After his retirement from this post in 1937, he became the first Director of the new Unit of Animal Physiology, also at Cambridge, under the Agricultural Research Council.

Barcroft was associated with a group of men, all of whom each in his own way made their mark on science and society. Among them were J.N. Langley, H.H. Dale and W.H. Mills, who had themselves been inspired by Michael Foster, the patron saint as it were of Cambridge physiology. Through Langley, Barcroft became interested in the oxygen consumption of the salivary gland. Studying this involved studying the oxygen dissociation curves of haemoglobin under various conditions – different concentrations of a variety of salts, varying pH, oxygen tension and atmospheric pressure. For all this a blood-gas differential manometer was used, which had been brought to perfection over a number of years by Barcroft himself. The Barcroft manometer became a standard piece of equipment in physiological laboratories.

Arising out of this work, Barcroft realized that there must be some organ in the body that was capable of storing large amounts of blood. He had used dogs and he found that in this species at any rate the storage organ was the spleen.

Once committed to anything, Barcroft was indefatigable in his efforts to make it go right. He was Chairman of one Government Committee which involved much travelling. While the younger members of the Committee were comfortably breakfasting in the hotel Barcroft, the septuagenarian, would be at the railway station checking the train connections and reserving the seats. On the occasion of the Post-war Conference of European Nutritionists in 1946 it was Barcroft who, with unflagging interest and zest, received the foreign delegates at the inaugural *convesazione*, who took the Chair at the first scientific session, and accompanied the visitors on their tour of the principal research centres of the British Isles. He was the guest of honour at the farewell banquet, and took his full share in all the preliminary negotiations and planning – and this was all done with an unvarying grace, good humour, and evident relish and enjoyment. Many men of half his age would have found much that the Chairman was able to accomplish, and in his stride, almost beyond their physical endurance, and sometimes beyond their forebearance.

Two things characterized Barcroft. The first was his easy relationship with students and colleagues. He came in to give a lecture one day and began in all

seriousness, ‘The title of my lecture is, “Where do flies go in the wintertime?”’ (a popular song of the period). What he intended to talk about was ‘Where do physiologists go on their sabbaticals?’ He went on to give a brilliant lecture on high altitude physiology, illustrating it by his own experiences on a high peak in Tenerife in 1910 and on Monte Rosa in 1911.

Barcroft’s second characteristic was his courage. Towards the end of the First World War the Government proposed to build a factory costing £1 million to make shells filled with hydrocyanic acid (prussic acid). The toxicity of this gas by inhalation had been tested on guinea pigs, goats, monkeys and dogs; and there was a considerable difference in susceptibility in different species, the dog being particularly sensitive and the guinea pig, goat and monkey less so. Nothing was known about the susceptibility of man to the gas, the very species on which it was proposed to use it. So Barcroft shut himself in a gas-tight chamber with a 12 kg dog and gradually raised the concentration of the gas in the chamber to 1 part in 2000. In just over one minute the dog was unconscious, and at the end of one and a half minutes was having convulsions and appeared to be *in extremis*. Barcroft then left the chamber carrying the unconscious dog. He had felt neither breathlessness nor in fact any symptoms at all.

Early in July 1941 Barcroft had been to the north of England for the Ministry of Food, talking to and advising small groups about current troubles they were having in feeding their children and farm animals. Later in the month he was one of 11 signatories who issued an invitation to scientists who might be interested in forming a Nutrition Society. The other signatories were Drs H. Chick, J.C. Drummond, J. Hammond, L.J. Harris, Sir Frederick Gowland Hopkins, Professor H.D. Kay, Sir Charles Martin, Sir Edward Mellanby, Sir John Boyd Orr and Professor R.A. Peters. The proposal was ratified and the Society was founded at a meeting at the Royal Institution on 23rd July 1941. The inaugural meeting was held in Cambridge on 18th October 1941, with Barcroft in the Chair at the first discussion. He succeeded Sir John Boyd Orr as Chairman of the Society in 1945, and the Council was to have nominated him as President under the new Constitution for the Session 1947–1948. Sadly he died on 21st March 1947.

No effort was too much for Barcroft in furthering the Society’s interests. At the age of 74 he would take it as a matter of course to be expected, say, to travel north to attend a meeting of the Scottish Group, or, if the need arose, to be asked at a moment’s notice to write half a dozen letters ‘at a high level’, or to make some necessary personal contacts with people in exalted places.

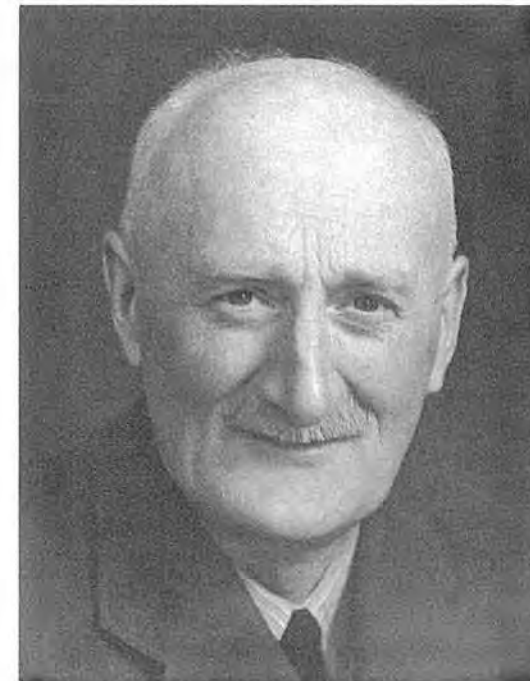
Barcroft’s distinctions included the Fellowship of the Royal Society (1910), the Royal Medal (1922) and the Baly Medal (1920). He received various honorary degrees, was a corresponding member of numerous foreign academies, and had been Fullerian Professor at the Royal Institution (1923–1926), Dunham lecturer at Harvard (1929), and President of the Physiological Section

of the British Association (1920). In both the First and Second World Wars he worked on problems of gas warfare at Porton, and was a member of the Army Medical Directorate Consultative Committee. Not himself medically qualified, he was proud to have been elected an honorary fellow of the Royal College of Obstetricians and Gynaecologists – a tribute to his researches on fetal physiology.

Contributed by R.A. McCance, 1989

Sir John Hammond

Reader in
Agricultural
Physiology and
Head of Animal
Research Station,
University of
Cambridge
President 1947–1950



Sir John Hammond was born in 1889, the eldest son of a farmer, and nephew, grandson and great-grandson of veterinary surgeons. He attended several different schools in Norfolk, including Gresham's School, Holt, which he left at the age of 12 when his family moved to a farm near Aylsham, and he spent the last part of his schooldays at Edward VI Middle School, Norwich. He won a Norfolk County Agricultural Scholarship, which took him to Cambridge to Downing College in 1907. He took first the Natural Sciences Tripos in 1909, and then the newly established Agricultural Diploma in 1911. This completed, he was awarded a three-year Ministry of Agriculture Research Scholarship. His great interest was in animals, and with the scholarship he worked with F.H.A. Marshall on fertility and milk secretion. There is no doubt that Marshall had a great influence on Hammond, particularly in arousing his enthusiasm for studying reproduction and development, and the effect of nutrition on them. As a sideline during this period (1911–1914) he collaborated with a geneticist, P.G. Bailey, in the application of Mendelian principles to sheep breeding.

At the outbreak of war in 1914 Hammond joined the 7th Norfolk Regiment, went to France, was in the battle of Loos and was invalided home with rheumatic fever. To pass the time while he was convalescing he got copies of past Smithfield catalogues, and from these he worked out and published papers on the growth rates of cattle, sheep and pigs. He realized that gain in weight was only the beginning. He wanted to know about the change in shape and composition of the animal as it grows and the effect of nutrition on them. The idea came for his well-known studies on the effect of the plane of nutrition on growth and development.

Meantime the war was still going on. Hammond did not go abroad again. He was sent to the Harwich Naval Base where, among other things, he arranged that some pigs should be obtained and fed on scraps from the canteen. This made such a handsome profit that the authorities suspected that something was wrong. In the end Hammond got off with a reprimand!

When the war ended he spent a year working for the Ministry of Agriculture as Inspector in the Livestock Branch. His task was to suggest how milk production might be improved. He found that the two main troubles were low milk yield and sterility.

He then returned to Cambridge to the Animal Nutrition Institute of the School of Agriculture. Marshall was the Director, and his interests and those of Hammond in reproduction and development ensured that these were their main lines of research. The supply of animals and facilities for keeping them were minimal, and it was not until 1930, when the Animal Research Station was set up, that Hammond had the possibility of keeping experimental farm animals. In the meantime he began work with a few rabbits kept in a tin shed. He bought them from a dealer, but this was so unsatisfactory that he began breeding them himself. He selected strains for large and small body size and

litter size at birth. He found that after the same length of gestation the average weight of the young was less in large than in small litters. It was his work on birth weight of young resulting from reciprocal crossing of rabbits bred for large and small birth weight that gave him the idea that the prenatal environment was of great importance for the growth of the fetus, and that the size at birth depended on the size of the placenta and blood supply, and hence the nutrition the fetus received. This was the reasoning behind Hammond's most spectacular experiment, carried out many years later with Arthur Walton. This involved the reciprocal crossing of a Shire horse and Shetland pony, using the new technique of artificial insemination. Some of his colleagues regarded the experiment as cruel for the Shetland mare – it would never be able to deliver its large foal – and farcical for the Shire mare, for its tiny foal would not be able to reach its teats. Hammond knew better. Both foals were carried to term and delivered without difficulty. The mean birth weight when the Shetland was the dam was 17.8 kg (mother's weight 214 kg). The birth weight of the cross when the Shire was the dam was 49.4 kg (mother's weight 797 kg), so all was well. At a meeting of the Society of Experimental Biology in Cambridge which was held about the time of his experiments in the 1930s Hammond gave a vivid demonstration outside the Physiology Department. He had Shire and Shetland stallions and Shire and Shetland mares as well as their foals. This must have been one of the most astonishing demonstrations ever seen on the Downing Site! As a postscript to this, when the war started in 1939 Hammond was obliged to dispose of his horses. But he wasn't going to lose the foals of the reciprocal crosses, so he got a cart for them and used it for his transport, so that he was able to keep them till they were 14 years old. They were still different in size.

More than 20 years later Hammond and Joubert followed this up with a reciprocal cross between Dexter and South Devon breeds of cattle. The result was the same. The small Dexter cow, with her smaller uterus, placenta and fetal blood supply had a much smaller calf than the large South Devon cow in spite of the difference in size of the bulls which provided the semen for insemination.

In the late 1930s Hammond and his colleagues began their well-known studies on the effect of the plane of nutrition after birth on growth and development. Between 1936 and 1938 McMeekan, from New Zealand, made comparisons between pigs reared on high and low planes of nutrition and consequently growing rapidly and slowly. He killed and examined the animals at the same body weights but different ages. The growth of all tissues was accelerated during rapid gain in weight, including the fat. Then a few years later, with Pálsson and Verges, the work was repeated with sheep with the same results. This was followed in the early 1940s by a study with Wallace, another New Zealander, on the effect of a low plane of nutrition of ewes at different stages of pregnancy on the growth of the fetal lamb. During the early part of pregnancy the growth of the fetus was not affected by the plane of nutrition

of the ewe, but during the last eight weeks nutrition was profoundly important.

On Marshall's official retirement in 1943 Hammond was appointed to succeed him as Reader in Agricultural Physiology and as Head of the Animal Research Station, and he held the appointment till he retired in 1954. Throughout his career Hammond's outlook was always practical. 'Science is not science until it works'. His great interest in reproduction and growth was due to his concern about the breeding and feeding of livestock for meat, milk, and wool. He was a great traveller, advising about milk and meat production, for example in the Argentine, the Sudan, India and the West Indies, and in post-war European agriculture. He was also a gifted speaker, especially at a practical level, and he was in great demand, not only in scientific circles, but also among gatherings of farmers and butchers.

When Hammond was asked what he believed his greatest contribution to practical agriculture had been, his answer was the use and promotion of artificial insemination. He used the technique on horses and rabbits first in the early 1920s and for these he used freshly collected semen. At the Research Station Walton explored ways of preserving spermatozoa, keeping the semen cool and under liquid paraffin; semen sent from Cambridge in 1935 'fathered' a lamb in Poland. The technique was applied with cattle on a large scale in Denmark and the USA. In England there was opposition, first from the Ministry of Agriculture, and when they had been won round the Church came in on the act, saying it was artificial and nothing artificial would be of any use, it was going against nature. Hammond replied, 'Do you drink milk?' They of course said 'yes', so he told them that was artificial – milk was meant for calves, not for them. They then questioned insemination on moral grounds. Hammond records his reply. 'In lots of villages throughout the country there is a long village street and a man at one end keeps a bull and a man at the other end keeps some cows. When a cow comes on heat the owner leads it along the village street to the bull at the other end. Everyone knows what is happening and all the small boys come and watch. That is what happens now. Under my conditions the owner would phone up, a car would appear and a man with a little black bag would do an insemination before any boy in the village knew anything about it. Which is best for morals?' That settled them. Incidentally, Scotland did not allow artificial insemination till years later. Artificial insemination made possible the exploitation of superior males. Hammond next turned his attention to the exploitation of superior females. This became possible when gonadotrophic hormones became available. A suitably treated female animal produced more eggs than could possibly develop in its own uterus; the eggs could be transferred to several other animals. This was not all plain sailing, for there were troubles with infections and rejections. These difficulties were overcome, a method for preserving the eggs devised, and in Hammond's words, 'In a little tube in a Thermos flask you could send a whole herd of cattle to Australia for a matter of shillings'. Hammond did not live to

see what the outcome of his work was going to be – the development of the 'test-tube baby'.

Hammond's bicycling figure, shrouded in an old macintosh, was often seen in Cambridge. He was always ready for a chat, and he talked to farmers large and small as he did to his youngest assistant, in terms of complete equality. He was a great teacher, and all who had the privilege of working with him know how much they owe to him. He was an enthusiastic founder member of the Nutrition Society and became its first President in 1947 under the new Constitution when Sir Joseph Barcroft died in March of that year while he was Chairman. He presented several papers at meetings of the Society in its early days, including one at a Symposium on Nutrition and Fertility in March 1949 entitled 'Physiology of reproduction in relation to nutrition'.

Hammond received many honours. He was appointed CBE in 1949 and received his Knighthood in 1960. He was made a Commander of the Order of Orange Nassau in 1946, and of the Italian Order of Merit in 1954. He was elected FRS in 1933, and he held Doctorates of two British and four foreign universities. He died in Cambridge in 1964. He was a truly great man.

I should like to thank John Hammond, son of Sir John Hammond, for providing the photograph of his father, and for his help in preparing this biography.

Contributed by Elsie M. Widdowson, 1989

Professor R.C. Garry

Professor of
Physiology,
University of
Glasgow
President 1950–1953



I was born on 21st April 1900 in Langside, a suburb on the south side of Glasgow. At that time Glasgow was a busy industrial city. Burning of raw coal and coal gas supplied energy and lighting industrially and domestically: a pall of smoke hung over the city: the skies cleared only during the Glasgow Fair when industry closed down and the citizens went on holiday. The Fair took place in the second half of July, and undergraduates present in the University at that time sought permission to climb up the steps inside the University Tower

to the top in order to see the surrounding countryside, which was quite invisible at other times of the year.

I went to a State School – Queen’s Park. I was a well set-up youth, verging on six feet in height, but the majority of my school companions were, by modern standards, undersized. They had fun asking of me, ‘Is it cold up there?’. Many showed signs of rickets, knock knees or bow-legs.

I matriculated in the University of Glasgow in the Autumn of 1917 to study Medicine: I graduated in 1922. As undergraduate I saw many of the later effects of rickets. Sir William Macewen, Regius Professor of Surgery, developed his surgical technique of osteotomy to treat knock knees. Murdoch Cameron, Regius Professor of Midwifery, was the first surgeon in modern times to use Caesarean section for childbirth. Severe rickets in young girls could cause such distortion of the pelvis that later in life normal childbirth was impossible.

Not all children developed rickets, and this was because cod liver oil was relatively cheap, readily available and had a popular reputation as a valuable protection against all winter ailments. The First World War forced the Government to take an interest in the quality and quantity of the Nation’s food supply. Such attempts were not always happy. The Government published a recipe for a ‘Standard Flour’ to bake a ‘Standard Bread’. In *John Bull*, a weekly magazine, appeared a ribald verse:

I thought I saw a paving stone
Defiled by many a tread.
I looked again and Lo! it was
a loaf of “Standard Bread”.
“Dear God!” I cried, “if I eat that
I’ll soon be with the dead.”

In the early years of the century the supply of vitamin C was scarce. My father’s annual autumnal remark was, ‘We won’t see oranges in the shops before Christmas’. During one winter the potato crop failed: I had to have a molar tooth removed, and the extraction wound stubbornly refused to heal. Twenty years were to pass before I realized what was amiss, I had had scurvy.

After graduation I spent one year in conventional house jobs in the Western Infirmary of Glasgow, but finally came to rest in the Institute of Physiology of Glasgow University. Here, in the 1920s, medical undergraduates spent three terms in pre-clinical work – physiology, physiological chemistry and histology. The Regius Professor of Physiology was Diarmid Noël Paton. His colleague, Professor of Physiological Chemistry, was Edward Provan Cathcart: the term biochemistry was not yet in general use.

Nutrition was dealt with in a rather bald, chemical fashion – proteins, carbohydrates, fats and ‘accessory food factors’, whatever they might be.

Cathcart’s main interest was in intake and expenditure of energy. He was an expert in indirect calorimetry, and human subjects, shouldering ‘Douglas Bags’, were everywhere; Haldane’s gas analysis equipment was on most laboratory benches. The energy cost, in terms of calories, was measured in all sorts of activities from marching to scrubbing floors.

The Glasgow department was also becoming concerned about minerals. Another strong advocate of the importance of minerals was John Boyd Orr, Director of the Rowett Institute near Aberdeen: John Orr was a Glasgow graduate. In the Rowett Institute, spending such time as he could spare from his Parliamentary duties, was still another graduate of Glasgow University. This was Walter Elliot Elliot. He received a DSc at the graduation in 1922 at which I graduated MB. The details of Elliot’s thesis are as follows:

Degree of DSc: Walter Elliot Elliot, BSc, MB, ChB, MP. ‘Studies in mineral metabolism with special reference to rickets in the pig and similar bone lesions in other animals and in the human.’

Much good solid work on mineral metabolism could not hide the growing recognition of the role of vitamins in nutrition. The Glaswegians had originally doubted the existence of these ‘accessory food factors’, but they were worsted and retired from the fray with a certain dignified resignation.

Around 1930 Cathcart and Mrs A.M.T. Murray were engaged on detailed and time-consuming studies on domestic food intakes by the inventory method. The results of these appeared in the Special Report Series of the Medical Research Council. In 1931 appeared Report No. 151 about 154 families in St. Andrews, in 1932 Report No. 165 about families in Cardiff and Reading, and later, in 1940, Report No. 242 about families in the Scottish Highlands and Islands. These were major contributions to the study of domestic food habits, with important social and nutritional implications.

This quite outstanding work was wholly in the hands of Cathcart and Nan Murray, and the staff of the Department did not know what was going on. More important, and quite regrettable, was the fact that the work and findings were not reported to meetings of the Physiological or Biochemical Societies: nor were they published in the corresponding journals. I did not understand why, and said so at the time. I still do not understand, and deeply regret that Cathcart and Nan Murray did not contribute actively to meetings of our traditional Societies.

In 1934 I went as Physiologist to the Rowett Institute. Dr John Orr (by this time Sir John) was Director. J.J.R. MacLeod, back from Toronto, was Professor of Physiology in Marischal College of the University of Aberdeen, Stanley Laybourn Davidson was Professor of Medicine and James Learmonth Professor of Surgery, Robert Boothby was Conservative Member of Parliament for East Aberdeenshire. All, in some way, had an interest in nutrition of farm animals and of man, and in agriculture in general. Nutrition

was the common factor. I was appointed Lecturer in Nutritional Physiology in MacLeod's Department of the University.

In the Rowett Institute I inherited a long-term study on rats. A large colony had a building to themselves with a carefully controlled environment: the genetic background was the same in all cases. The experiment lasted over two years, and four generations of rats were reared from the same stock. The care of the rats, and the recording of physiological and pathological states followed the technique deployed originally by McCarrison in India. All the rats were fed on the 'Peterhead Diet'. Peterhead and Fraserburgh were two fishing towns on the coast of Aberdeenshire, where herring were caught with drift nets. The general health of the inhabitants of these two towns was indifferent. Dietary surveys by the inventory method were made and the rats received a diet similar in all respects to the food eaten by the human families. Half of the rats, generation after generation, received only the human diet, the other half, the human diet with a generous supplement and green vegetables.

In spite of a similar environment and heredity the animals which did not have access to additional milk and green food grew more slowly, had a slightly impaired reproductive capacity and an increased death rate due to infection by micro-organisms, although exposure was the same for all. Moreover, the rats without the supplement of milk and green food had a lower concentration of haemoglobin in their blood, and did not seem to be so bright and alert. Publication of this work was in the *Journal of Hygiene* (Orr, J.B., Thomson, W. and Garry, R.C. (1936) *Journal of Hygiene* 35, 476–497). There was no mention of Peterhead in print and glib use in speech of 'Peterhead Diet' was discouraged. When I suggested that the work should continue and an attempt be made to identify in the 'milk and greens' supplement the factor or factors responsible for the benefit, Orr shied away. The supplemented diet was a good diet, the unsupplemented 'Peterhead' diet was not a good diet: that was all there was to it. From the point of view of immediate human welfare I suppose Orr was right but I still remember my feeling of disappointment at abandonment of this work.

In the course of this long experiment, lasting over two years, on a large colony of rats, all given a diet mimicking a human diet eaten by a working-class community in a Scottish seaside fishing town, we saw that some adult rats suffered from diverticulitis. The diverticula were first noted after an outbreak in the rat colony of infection with salmonella. The diverticula were in the distal ileum and colon: the diverticulosis gave place to diverticulitis. The incidence of diverticulitis was less in the rats whose diet was supplemented with milk and greens. It would be unjustifiable after this long interval to suggest a cause for the occurrence of the diverticula or for the different incidence. But the occurrence should go on record.

The original research of the Rowett Institute received practical and psychological support from the presence of the Imperial Bureau for Animal

Nutrition in the Institute. Here was published *Nutrition Abstracts and Reviews*, abstracts of papers on nutrition in journals all over the world. From time to time these abstracts were drawn upon by writers of reviews which were in their turn published in *Nutrition Abstracts and Reviews*. This smacked, as some said, of scientific journalism, but the coverage of the literature was comprehensive, and the distribution of the information world-wide. I tried my hand at one such review, on dietary requirements in pregnancy and lactation. I was most impressed by the size and distinction of the audience I thus reached.

The general administration of the Bureau, the culling of abstracts from world literature on nutrition, were in the able hands of Dr Isabella Leitch. Moreover, her encyclopaedic knowledge of the literature on nutrition was available to all. Workers on nutrition, far and near, were deeply in her debt, and articles and abstracts for publication owed much to her skilful editing.

I left the Rowett Institute in 1935 to go as Professor of Physiology to University College, Dundee, at that time one of the Colleges of the University of St. Andrews. I went with much regret. At the outset in Dundee I had to cope with a hangover from my time in the Rowett Institute. A dietary survey of families in the Dundee district revealed that many lacked an adequate intake of vitamins of the B group. From the Physiology Department of University College we distributed supplements to make good such deficiencies. In many families these supplements did not find favour on the grounds of taste and flavour. It was no use to say 'They're good for you'. This rebuff was a salutary lesson for the nutritionist in the laboratory.

Connective tissue plays a part in the healing of wounds. Gordon Campbell, Professor of Dental Surgery in the University of St. Andrews, thought that many of his elderly patients in the Dundee Dental Hospital had an inadequate intake of vitamin C and would benefit from administration of ascorbic acid before removal of teeth. To his delight he found that giving several grams of ascorbic acid some days before the operation greatly accelerated healing and reduced pain and inflammatory reaction. Here was stark evidence that many patients attending the Dental Hospital were suffering from some degree of scurvy. This was when I realised that I, too, had suffered from scurvy 20 years previously, towards the end of the First World War when the potato crop failed.

I left University College, Dundee in 1947 with very considerable regret. I was much at home in the Medical School, although I was made to realize that the Medical School 'belonged' to the University of St. Andrews and was not part of University College. So it was back to Glasgow University, to Glasgow College on the point of taking over the extra-mural medical Colleges, St. Mungo's and Anderson's and of admitting for the first time undergraduates in Dentistry.

Once more there was a hangover from previous work: since healing of tooth-extraction wounds benefited from the presence of ample ascorbic acid,

would wounds of the cornea also benefit? An ophthalmologist, Fergus Campbell, found that this was so if the injury was deep enough to involve the connective tissue of the cornea. Healing of superficial ulcers of the corneal epithelium alone derived no benefit from administration of ascorbic acid.

I had for long been irked by those who claimed that many women were anaemic due to a poor diet, to loss of blood at menstruation, or to an unhealthy life-style. In the chaos of the immediate post-war years there was neither time nor facilities to carry out conventional physiological laboratory research. Then I looked at the couple of hundred healthy youngsters, male and female, in their late teens, in the seats of my Lecture Theatre. I thought, 'What a wonderful opportunity to have a go at anaemia in young women'. I was already critical of the sloppy use of the word anaemia. I thought that experiments with supplements of iron salts lasted for far too short a time. We started our experiments in a crusading spirit and with tolerant interest on the part of the undergraduate experimental subjects. Work was carried out with the greatest care to ensure secrecy about dosage, and we used sophisticated methods to find the concentration of haemoglobin in venous blood. The subjects, men and women alike, received tablets daily with 28 mg of iron as ferrous sulphate; there was a small addition of copper and manganese as sulphates. A.L. Bacharach, of Glaxo Laboratories, arranged for the supply of tablets and of indistinguishable placebos.

At the outset 54 healthy men undergraduates had a mean concentration of 16.21 g haemoglobin in 100 ml of blood: 55 women undergraduates had 14.59 g haemoglobin in 100 ml of blood. One year later the difference in mean concentrations between men and women was still of the order of 1.5 g haemoglobin in 100 ml of blood. As a result of giving a supplement of iron for one year there was a slight comparable increase in the concentration of haemoglobin in the blood of both sexes. There may have been, therefore, in men and women alike, in the past, a sub-optimal intake of iron in their daily food. But there was no evidence to justify the belief that women were selectively disadvantaged (Garry, R.C., Sloan, A.W., Weir, J.B. de V. and Wishart, Mary (1953). The concentration of haemoglobin in the blood of young adult men and women: the effect of administering small doses of iron for prolonged periods. *British Journal of Nutrition* 8, 253–268).

The Second World War revived the interest in intake and expenditure of energy. The Medical Research Council appointed a Diet and Energy Committee with myself Chairman and R. Passmore, of the Edinburgh Physiology Department of the University, Secretary. We considered where we could make the most useful observations. Coal miners have to work in a difficult environment and there was a feeling abroad that they have to spend a tremendous amount of energy underground. In a mining community, however, not all workers go underground. There are many clerks on the surface engaged in sedentary work. Here was a wonderful opportunity to study intake and

expenditure of energy by men with two very different life-styles. We used the Kofranyi-Michaelis respirometer to measure the volume of expired air and to obtain samples for analysis. Grace Warnock, of the Scientific Adviser's Division of the Ministry of Food, organized dietary surveys of the families by the inventory method. The University of St. Andrews allowed us to set up our equipment in the Physiology Department and we had comfortable accommodation in student residences. The miners and their wives gave us every assistance. We were made temporary members of the Miners' Canteen and ate very well, better than the general public: in 1952 rationing was still in operation.

The daily energy expenditure by the miners was, on the average, 3660 calories: they kept physical exertion within bounds by use of rest pauses. The clerks spent, on the average, 2800 calories in 24 hours. The dietary surveys showed that these expenditures of energy were comfortably met by the intake of food. Full details of this investigation appeared in 1955 in No. 289 of the Special Report Series of the Medical Research Council.

The formation of the Nutrition Society was surprisingly hesitant. In the early years of this century physiologists and physiological chemists worked on what we would now call nutrition, and they did not find publication difficult. There was the *Journal of Physiology*, the *Biochemical Journal* and medical publications in general. Did we really need another society of professional scientists wholly engaged in the study of nutrition?

It was John Orr who realized that nutrition requires something more than another scientific society. I think he had a vision of a much larger group, many with knowledge and skills in a wide variety of disciplines. Orr, who trained in medicine, had entry to the medical profession, and he attracted physiologists, sociologists and politicians. One aspect of this wide appeal was Orr's slogan 'The marriage of health and agriculture'.

The publications of the Nutrition Society also had a hesitant start. First came a very modest *Proceedings* which, from the outset, revealed the many different primary interests of those contributing to it. The Symposia, published in the *Proceedings*, were a new venture in scientific journalism and brought together many and varied disciplines. Then came the *British Journal of Nutrition*, a conventional scientific journal on the lines of the *Journal of Physiology*.

This was a time of great activity, of much progress and development. The Society is indebted to three men above all, A.L. Bacharach, S.K. Kon and L.J. Harris. Bacharach was Honorary Treasurer. He had knowledge of commercial practices and swept away with good humoured impatience unnecessary timidity in the conduct of our early financial affairs. Kon was Chairman of the Editorial Board and a brilliant editor. He had delight in, and enthusiasm for, the English language. With infinite pains, he moulded the work of less gifted writers into acceptable texts.

Harris, Honorary Secretary in the early days, was a born administrator. He

planned, he organized until every aspect of the work of the Society occupied its proper niche. And then, in 1946, he played a major role in the formation of the International Union of Nutritional Sciences. During its formative years (1946–1960) Harris was Secretary-General of IUNS. Harris also found time to write a history of our Nutrition Society and also of the International Union.

How did Cathcart, a pioneer in metabolic studies, react to this acceptance of nutrition as a separate discipline? Cathcart had used indirect calorimetry to study expenditure of energy by man at work and at play: by family dietary surveys he revealed in intimate details the practices of domestic catering. Yet he never became a member of our Nutrition Society. However, after one meeting of the Society in Glasgow he said to me, ‘If you go on having meetings like this, I think I’ll have to join you’. And this he did do in a way. In 1947 he published his second last paper on pages 45–52 of Volume 5 of the *Proceedings of the Nutrition Society*. The title was *The rôle of nutrition in social medicine*.

Recorded with Elsie M. Widdowson, 1986

Dr Leslie J. Harris

Director,
Dunn Nutrition
Laboratory,
Cambridge
President 1953–1956



I was born in 1898 in Liverpool where my father was a Jewish clergyman. The spectre of destitution, poverty and malnutrition in the streets near where we lived was there for all to see, and when today I hear people talking glibly about the ‘good old days’ I feel that they really know very little about what the early years of the present century were like.

I went to school at Liverpool College, a so-called British public school.

One of the masters there, who must have made a specially strong impression on me, was fond of stressing that one had to keep on working hard all the time, never to let up. Another master who influenced me a great deal at school was the late R.W. Slowly, later to become a distinguished Egyptologist and Head of the Research Department of the British Air Force. He it was who excited my enthusiasm for science and for things scientific.

I did sufficiently well in my school-leaving examinations to be awarded a senior scholarship to study Chemistry at the University of Manchester, so my future career was now assured for the time being. My course in Honours Chemistry included in the first year Mathematics and Physics and I had the good fortune to be taught by Sir Horace Lamb and Physics by no less a master than Sir Ernest Rutherford, later Lord Rutherford. Rutherford's lectures were delivered in a very matter-of-fact way. He always adopted the historical approach, showing us how one discovery led up to another, almost as a matter of course and as a natural sequel. He used to explain, for example, that Faraday, having first found how an electric current affected a magnet, then naturally proceeded to investigate the effect of a magnet on an electric current – hence the dynamo.

In my advanced Chemistry classes I specialized in the History of Chemistry and Biochemistry, then called Bacteriological and Enzyme Chemistry. I was fortunate enough to come out top in the class list of both these subjects, and obtained a first class Honours degree in Chemistry.

Upon graduation I went to see Professor A.V. Hill, Professor of Physiology at Manchester University. He offered me two alternatives; the first was that I could join his staff and he would offer me an appointment as a demonstrator in Physiology with opportunities for research; secondly, the alternative, he recommended that I should write to Professor Gowland Hopkins in the new Biochemistry Department at Cambridge. I accepted the second piece of advice and wrote to Hopkins. By return of post I received an invitation to come to Cambridge and work under Hopkins with a research grant from the Department of Scientific and Industrial Research, and with additional part time employment as junior demonstrator in Biochemistry. This was in 1922 and marks the beginning of my research career.

When I joined the Department of Biochemistry at Cambridge, Professor F.G. Hopkins already enjoyed the reputation of being the outstanding biochemist of his time. He was best known for the isolation of the amino-acid tryptophan and for his early experiments on the growth of rats, kept on purified basal diets with and without the addition of small quantities of milk, supplying what were then termed the accessory food factors and now known as vitamins. At that particular time, however, he was concerned in the isolation of glutathione and examining its properties. I subsequently collaborated with him in revising its constitution. A paper on this was published later in the *Journal of Biological Chemistry*. Hoppy, as he was affectionately called, was

surrounded by groups of devoted and admiring disciples. Just across the way was Joseph Barcroft, whom I was to see a lot of later on in The Nutrition Society and in the International Union of Nutritional Sciences. Research flourished in such a distinguished atmosphere and people would come to Cambridge from far and wide. Hopkins himself was to receive many signs of recognition and honour, including a knighthood, the Presidentship of the Royal Society, the Order of Merit and he shared a Nobel Prize with Eijkmann for his work on the accessory food factors.

I turned my attention to the titration curves of amino acids. By titrating them successively in water and then in formaldehyde I was able to give the first positive proof of their so-called Zwitterion constitution as well as establishing the identity of the various titrating groups.

The next event in my career was the establishment of the Dunn Nutrition Laboratory at Cambridge. Sir William Dunn was a financier and company director in the City of London who left a most singular Will and Testament, stating that 'he bequeathed his estate for the alleviation of human suffering' and he appointed the Commercial Union Assurance Company as his executors. This phrase 'the alleviation of human suffering' seems vague in the extreme but it happened that the Commercial Union Assurance Company was wise enough to approach the then Secretary of the Medical Research Council, Sir Walter Raleigh Fletcher, to enquire how best they could alleviate human suffering. Fletcher recommended that part of the Sir William Dunn bequest should be used for setting up a new building for his close friend and benefactor Gowland Hopkins and for endowing the Chair of Biochemistry. Later on, funds were made available from Sir William Dunn's bequest for the building of our own Institute, the Dunn Nutrition Laboratory at Cambridge. Up to 1927 Britain had no institute for nutrition studies, and at the time almost the only work on nutrition outside Cambridge was that of Martin and Chick and their group at the Lister Institute in London, and that of Boyd Orr and his associates at the Rowett Institute at Aberdeen.

When I was put in charge of the Dunn, it was natural that we should turn primarily to investigations on vitamins. For many years it happened that there was also another academic in Cambridge with the same surname as myself. He was the Professor of Anatomy, and so while he was popularly known as Anatomy Harris, I was called Vitamin Harris.

In 1933 I devised a test for assessing the vitamin C status of human subjects. This is the so-called saturation or loading test; the basis of it is that the lower the subject's recent intake of vitamin C has been, the greater is the load of vitamin C that has to be given before he eventually becomes saturated, when the excess of vitamin C begins to overflow into his urine. This test was the first to be devised to assess chemically a human subject's status in a particular vitamin.

When I first began my research career in the 1920s only three or four

vitamins were recognized, namely A, B and C and later D. At that time no chemical method existed for estimating vitamins in foods or tissues. Then came the isolation of vitamin C by Szent Györgyi, and its chemical estimation soon followed. My colleagues, Mamie Olliver, T.W. Birch, S.M. Ray and others were actively associated with me in this and similar investigations.

Another of our main interests was vitamin B₁. I was struck by the fact that, although the deficiency of vitamin B₁ in the human subject was known to result in characteristic changes in the heart, no such effect had ever been recorded in animals. I therefore sought out the collaboration of a neighbouring cardiologist, A.N. Drury, and we soon discovered that in experimental rats vitamin B₁ deficiency results in a remarkable and characteristic slowing of the heart beat. The severity of this bradycardia depended on the degree of deprivation of the vitamin; it was reversible in the sense that it was rapidly cured by the administration of the vitamin. This fact immediately suggested to me a new method of assessing vitamin B₁. This had the advantage of being much more rapid than the standard growth rate test, and the method came into wide use. My colleagues and I did a good deal of work also in developing a reliable chemical method of estimation of vitamin B₁ by means of a thiochrome reaction. As with vitamin C, not all nutrition workers were prepared to accept the validity of chemical methods of vitamin assay, so we devoted a lot of time comparing the results of chemical and biological methods. I have been especially interested in the metabolic effects of overdoses of vitamin D. My conclusions were that these could be regarded as an intensification of its normal mode of action. We found that overdoses of vitamin D caused an increased retention of calcium and phosphate in the body, a raised level of calcium phosphate in the bloodstream and in consequence, excessive calcification in the growing ends of the bone as well as in certain other sites in the body. These effects are the exact opposite of those seen in deficiency of the vitamin. Another point was that the effect of vitamin D could be contrasted with that of parathyroid hormone which, although it raised the blood calcium, actually caused a loss of calcium from the body instead of an increased retention. R.M. Innes shared in much of the experimental work in this field.

I have already mentioned various colleagues and collaborators. One of the most distinguished has been A.J.P. Martin. He was awarded a Nobel prize for his pioneer work in introducing paper chromatography. His investigations had their origin in work that he began with us when he was engaged in fractionating different forms of vitamin E. I invited Martin to come and work with us when he was setting out on his research career and had already been refused admission to several other research institutes, largely, I suspect, because of his somewhat unconventional ways.

Throughout the whole course of my appointment our deputy director has been Thomas Moore, and how happy and cordial my connection with him has always been. The friendship has extended also to our wives and children.

I think of Moore's chief achievement in research to have been his early demonstration of the relationship of carotene as pro-vitamin of vitamin A. His monumental and authoritative book on vitamin A is a model in itself and confirms his position as a leader in this field, as it is in other neighbouring fields. Looking back I can recall how, before any building operations had begun on the erection of our new laboratory, there were just the two of us, Thomas Moore and myself, accompanied by the 'lab boys' as they were then called. We worked together in what was no more than a small tin hut, literally a hut, with no proper accommodation yet available. Since then, during the course of the years, several extensions and enlargements to the building have been added; partly I think in recognition of a great deal of routine testing work that we were called upon to do on behalf of various government departments and official committees, especially during the Second World War.

My successor as director at the Dunn was Dr Egon Kodicek. I was able to help him to leave Prague and to join us here when his own safety was threatened by the arrival of the Nazis. A little later on I took part in a kind of secret service conspiracy in order to smuggle his wife and their small child out of the country from under the eyes of the occupying forces. Kodicek's own distinguished record obviously justifies his steadily continuing advancement since those early days.

I was the first Secretary of the Nutrition Society, and remained Secretary for a good many years; later I became its President. An immense amount of organizing work was involved in getting our new Society on its feet and directing it, as I hope, in which way it should go. From the very beginning we had crowded and enthusiastic meetings and conferences and we maintained, as I think, a high scientific standard, both at the meetings and in our journals. I have previously published an account of the history of the Nutrition Society and I need not go into much detail, but should like to add a few personal notes.

Our first President was Lord Boyd Orr and he was one of the prime movers in its inception, a man of considerable influence in political and international circles and later at the Food and Agricultural Organization of the United Nations. Sir Joseph Barcroft was also a very vigorous helper for us, rather surprising perhaps as he had no previous ties with work on nutrition, apart perhaps from the fact that he was at the time Chairman of the Food Investigation Board, whose principal concern was low temperature research.

Another active worker with whom I was brought into close contact was A.L. Bacharach, a man of extraordinary ability and extremely wide interests, scientific, musical, literary and political. When he died, the obituary tributes paid in *The Times* were witness to his remarkable range of activities. In the Nutrition Society he shone in his contributions to scientific discussion, always pertinent, in impromptu speeches on any occasion, as well as in solid committee work.

It was the Nutrition Society which took the lead in establishing the

International Union of Nutritional Sciences when we held our first International Congress in London at the end of the Second World War – the principal object then being to bring together scientists who had become separated by the war, so as to pool their experience and knowledge.

Being the First Secretary General of the International Union of Nutritional Sciences threatened to become pretty well a full time occupation and I had the responsibility of maintaining touch with the various member countries and helping them to organize successive World Congresses in their respective capital cities. As far as I was concerned, a particularly happy feature of this post was the close contact it gave me with eminent nutritionists in different countries. As Secretary General I also had the privilege of getting around the world and meeting various political leaders and Heads of State. I recall when I was asked to confer with President Nehru about problems of nutrition in India, I found him to be even more concerned about over-population than about undernutrition. Unfortunately I was unable to answer his question and to recommend to him a nutritional message of birth control.

Recorded with Alice M. Copping, 1970

Dame Harriette Chick

Head of Division
of Nutrition,
Lister Institute,
London
President 1956–1959



Harriette Chick, the third daughter of Samuel and Emma Chick, was born in London on 6th January 1875. Samuel Chick, son of a lacemaker of Sidmouth in Devon came to London as agent for the family lace business. He took up property in Newman Street, W1 where most of the family were born. They later moved to Chestergate, 30, Park Hill, Ealing, W5 where the large house and garden allowed a family of seven sisters and three brothers to grow happily. There was a second home at Hazelwood, Branscombe, Devon for holidays. At

Chestergate and Hazelwood there were large orchards and gardens where Harriette was always concerned with cultivation. Her interest in gardens continued throughout her life.

All the sisters attended Notting Hill High School where the excellent Headmistress taught much science. At its centenary in 1973 Dame Harriette was present as the oldest of old pupils.

From Notting Hill High School Harriette went on to University College London to take a BSc in science. She took a distinguished degree and was awarded an 1851 Exhibition award with which she travelled to study bacteriology at the University of Vienna Institute of Hygiene, and in Munich under Professor Max Gruber, who headed one of the earliest European Schools of Bacteriology. On her return to England she took up further study under Professor Rupert Boyce at Liverpool. In 1905 she was awarded a Jenner Memorial Studentship to work at the Lister Institute where began the fruitful years of collaboration with Charles Martin. From 1905 until the outbreak of war in 1914 most of the work of Chick and Martin was concerned with disinfection studies and with coagulation of proteins. Martin went into the army medical corps and met problems of nutritional disorders in survivors from Gallipoli and in the Middle East. He sent back to the Lister Institute for help. Harriette Chick's transfer of interest from bacteriology to nutrition came in the war years when Charles Martin, then Director of the Institute, was on active service with the Australian Army Medical Corps. He found troops in Egypt and Palestine suffering from a nutritional disorder that resembled beriberi and sent back to the Institute for work to discover protective foods suitable for provisioning the army in the field. The skeleton staff of the Department of Experimental Pathology, led by Harriette Chick, attacked the problem and suggested the use of dried eggs and dried yeast to provide an anti-beriberi ration. This work expanded and by the end of the war much work in the field of nutrition, including studies of scurvy, was in progress. There was at this time a severe shortage of food in some parts of Europe, and Austria was suffering acutely.

Reports of the occurrence of many cases of rickets in children and of a condition called bone-softening, or adult rickets, caused a joint mission from the Lister Institute and the Medical Research Committee to go to Vienna to investigate the opportunities for studying the relation of nutrition to bone diseases. In 1919 this preliminary mission agreed that the time was ripe for such research and a small team of women led by Dr Chick and Dr Elsie Dalyell with Miss E.M. Hume, Dr H.M.M. Mackay and Miss H. Henderson-Smith went to Vienna and worked for more than two years in close collaboration with the staff of the University Kinderklinik in Vienna and the Children's Hospital at Meidling. The success of this team in overcoming scepticism of the clinicians about the nutritional factor causing rickets and proving that a fat-soluble vitamin present in cod liver oil or exposure to ultraviolet light could cure and

prevent rickets in children was a measure of the personal powers of persuasion of their leader. The full report of the study is given in the Medical Research Council Special Report Series No. 77 of 1923. In his preface, Professor Clemens von Pirquet, Director of the University Kinderklinik, states that he had little expectation that the study would lead to results of much practical value, since he was convinced that rickets was an infectious disease, widely prevalent in Europe and severely affecting children who had special susceptibility as the result of an inherited tendency, or a faulty diet, or of defective general hygiene. In fact he imagined that rickets was comparable to some extent with tuberculosis. However, the minute and conscientious observations carried out by the team finally produced results that dispelled his doubt about the nutritional nature of the disease. Moreover he congratulated the team on the management of the hospital wards where their studies were made and he and the rest of the staff of both hospitals became firm friends of the team, and a longstanding connection between the Lister Institute and the Kinderklinik grew around the rickets study.

On her return to London in 1922 Dr Chick began her nutritional studies on proteins from the point of view of their biological value, and built up a team to carry out this very demanding investigation. The problems of planning the diets for the protein studies necessitated balancing all known accessory food factors and particularly the vitamin B content. At this time the complex nature of vitamin B was only beginning to be recognized and soon further work developed on the nature of the various fractions of vitamin B preparations so that a new team took on this problem with biologists, biochemists and clinicians under the direction of Dr Chick. The detailed work on B vitamins led onto a study of pellagra with both experimental and clinical observations that contributed to the elucidation of the nutritional background of another disease which had long been known but not recognized as a deficiency condition. The study of B vitamins was also closely related to the nutritional value of cereals and specially to the various fractions of the wheat grain obtained in modern milling procedures. A very extensive study was made of the B vitamin content of brown and white bread and of flours of different extraction. At the same time the protein value of the flours was measured and this work was a large part of the background of the wartime national loaf in 1940.

In 1939 Dr Chick took the Division of Nutrition to their wartime home in Sir Charles Martin's house in Cambridge since it was deemed unsuitable to continue to work with experimental animals in Chelsea in case of bombing, and as the Division had to have its animals for biological testing of foods a safe retreat was needed. The transport of experimental animals in the midst of tests, of the breeding colony of rats and of the very precious small monkeys, was safely achieved in her family car. Much help was given to the Nutrition group by other laboratories in Cambridge, but the main part of their work on cereals continued and studies of the nutritive value of potatoes, of air force diets and

of other wartime problems were undertaken. Work was carried out at Roebuck House for the next six years. Only when Dr Chick herself retired five years beyond her full retiring age did the Division return to Chelsea.

Dr Chick, who was Head of the Division of Nutrition of the Lister Institute, was one of the 11 distinguished nutritionists who signed the original circular letter in June 1941 inviting interested people to inaugurate The Nutrition Society. She attended all the preliminary discussions and was a member of the original committee set up to arrange the plans for meetings of the Society, and called in the help of her colleagues in the work of preparing for the first meeting of the new society in Cambridge, on 18th October 1941. For her early promotion of the society Dame Harriette was honoured, together with two other founders, Lord Boyd Orr and Sir Charles Martin by being made an Honorary Member in 1949. Her interest and activity in the affairs and meetings in no way decreased in spite of her age and from 1956–1959 she was President with no apparent limitations, though already over 80. Her contact with the Society and with nutrition in general continued to the end of her long life.

In 1931 the idea of an abstracting journal giving rather full abstracts of nutritional papers relating to animals and man was brought to fruition and Dr Chick was one of the first three editors of *Nutrition Abstracts and Reviews*, together with Sir John Boyd Orr and Professor J.J.R. MacLeod. She continued as an active editor until 1947 and then as consulting editor until many years later. Her achievements in nutrition were honoured by the award of the CBE in 1932 and in 1949 she was made DBE. After she retired as head of the Division of Nutrition Dr Chick kept in close touch with the Institute and with the Division until it was disbanded in 1949. By that time she had made her home in Cambridge, but was always accessible to old colleagues and ready and anxious to discuss the latest work in the field of nutrition. She attended meetings of the Nutrition Society frequently into her 99th year, always taking a keen interest in the proceedings and making herself available for discussion.

In 1932 Dr Chick gave a series of lectures in the United States on the relation of pellagra to maize diets. At this time she went on to Australia, where she visited Sir Charles Martin who was working in Adelaide, and continued to travel round the world visiting friends and colleagues. In addition to her work at the Lister Institute Dr Chick was secretary of the Accessory Food Factors Committee of the Medical Research Council from 1925 to 1945, and of the League of Nations Health Section Committee on the Physiological Bases of Nutrition from 1934–1937. She was also closely involved with committees on international standardization of vitamins and other factors. Her Division attracted scientists from all over the world to come for brief visits or to work for longer periods under her aegis. So it was a marvellous meeting place for international nutritionists, even before The Nutrition Society set out to keep the subject in the forefront of world interest. In 1974, shortly before her 100th birthday the British Nutrition Foundation awarded her their annual

prize. For this she prepared a lecture on her work on rickets in Vienna and gave a lively introduction before a colleague read the rest of the discourse.

All her life Harriette was lively and energetic. As long as there was a tennis court at the Lister Institute she was happy to play. She was always keen on mountain holidays and went back to Austria with a regular group of friends. Her interest in gardens and flowers never decreased, as anyone who visited her in her last years in Cambridge would remember. She died at her nephew's house, Uppercross, on 9th July 1977 aged 102 years.

Contributed by Alice M. Copping, 1990

Note by Elsie M. Widdowson

Dr Chick made several visits to the MRC team studying the effects of under-nutrition in Wuppertal, Germany, between 1946 and 1948, each time accompanied by Dr E.M. Hume. The problem was not how to entertain her, but how to keep up with her, for she wanted to go everywhere and see everything that was going on. It was her idea that a study should be made on the nutritive value of soya proteins for infant feeding. This was conducted by R.F.A. Dean, and this experience led to his going to Uganda in the early 1950s to investigate protein deficiency among young children there, and whether plant proteins might be used in prevention and treatment. This in turn led to the setting up of the MRC Infantile Malnutrition Research Unit, Kampala, Uganda.

Dr A.L. Bacharach

Head of Nutrition
Department,
Glaxo Laboratories
President 1959–1962



Alfred Louis Bacharach was born in 1891. His parents had come from Stuttgart to live a comfortable middle class existence in Hampstead, in the same area that Alfred lived and brought up his own family in a house overlooking the Heath.

As a boy, Alfred attended a small private school and then, at the age of 12, was a foundation scholar at St. Paul's School. He then won a scholarship to Clare College, Cambridge where he read chemistry, obtaining a BA and then an MA in 1914. In Cambridge he made friends in a wide variety of fields – scientific (chemistry, biology, mathematics), artistic (music and writing) and political. He was deeply involved in the Union Society and the Gownsmen Journal. At this time he developed his enthusiasm for mountain climbing, for photography, and an enduring addiction to coarsely cut tobacco smoked in a pipe of characteristic shape. He always felt keenly the need to overcome poverty and to help the disadvantaged (among his many acts of kindness, he would not throw away his old pipes but would leave them on a seat he knew to be frequented by tramps!).

Though his interest in politics began at school it was at Cambridge that he became a prominent member of the Fabian Society, following Rupert Brooke and Hugh Dalton. He served the Independent Labour Research department on its executive committee and as a member for about 40 years. He frequently wrote for socialist periodicals, especially on food and education. He was always ready to discuss labour history and labour politics but never attempted to proselytize. He had a life-long association with the Working Men's College in North London where he taught English and organized Sunday evening concerts for many years. He also became a Governor of the Borough Polytechnic and of Kynaston School.

So far as is known Alfred never kept a diary but he did take records of his hobbies and pastimes, whether of bird sightings on the Heath, stamps exchanged, books read, menus his wife Lillie offered to their dinner guests, the type of wine served (and whether it lived up to expectations!) or the weight of incombustible matter present in the St. Bruno Rough Cut which he persisted in smoking. It is hard to determine if this precise recording and tabulation resulted from, or caused, his interest in science but it does show his keen appreciation of the numerical assessment of biological phenomena which was to be a feature of his scientific career.

After leaving Cambridge Alfred found employment in the Wellcome Research Laboratories, working on alkaloids, then in the analytical Department of the Wellcome Chemical Works until in 1920 he joined Harry Jephcott in the Glaxo Department of Joseph Nathan & Co. Ltd., which was later to become the Glaxo Laboratories. Alfred was recruited as an analyst, later to become Chief Chemist and then Head of the Nutrition Department.

In the 19th and early 20th centuries 'summer diarrhoea', transmitted by contaminated, unpasteurized milk, was the major cause of infantile mortality, being responsible for over 70 out of 130 deaths per 1000 live births. About

1910–1912 it was shown that this hazard could be overcome by the use of roller-dried milk in baby foods such as Glaxo. This led to a rapid decrease in mortality (and equally rapid rise in the use of dried milk). In 1919 infantile scurvy and rickets were serious problems, only avoidable by dietary supplements providing the essential food factors now known as vitamin C and vitamin D respectively. At that time little was known of the chemistry of the vitamins and to add to the confusion it was found that rickets could be cured by sunshine as well as by cod liver oil. In 1923 the anti-rachitic vitamin D was clearly differentiated from the fat-soluble A and Glaxo speedily obtained a licence to purify this vitamin from cod liver oil. In this way a stable odourless fraction was produced which could be used to fortify infant milk foods and also to manufacture bland pharmaceutical preparations to replace unpalatable cod liver oil. (Alfred Bacharach coined the term 'OSTELIN' for the pharmaceutical preparation from the classic root OS – bone, TEL – teleost (bony fishes) and IN – for easy termination of this trade name!) Shortly thereafter Glaxo obtained the rights to the Steenbock patent for the production of vitamin D by ultraviolet irradiation of sterols, an outcome which owed much to Alfred's ability to measure the vitamin.

As Chief Chemist, Bacharach was responsible not only for the standardization and quality of dried milk, but also for the detection and measurement of vitamins and other components essential for health. From 1921 onwards he published numerous papers on these topics in refereed scientific journals, in reviews, reports to symposia and numerous articles in magazines or broadcasts to the lay public. In the 1920s the vitamins could only be assessed by biological techniques using animals. Alfred was one of the first to realize the need for animals specially bred and reared for uniformity of response. To meet this objective he obtained, in 1924, a nucleus of albino rats which were then maintained by a strict system of brother x sister mating to maximize genetic uniformity. This stock, known as the WAG (Wistar Albino Glaxo) strain was still being bred in this way 90 generations and 40 years later.

Bacharach recognized early the problems raised by the inescapable variability inherent in bioassay systems and was one of the first to apply statistical techniques to determine the validity of the results obtained. In this he was greatly helped by his acquaintance with statisticians Irwin, Bliss, Fisher and Yates. He made his own useful contributions, devising the term 'metameter' to define a parameter which can be measured. He was one of the first to use, in vitamin research, statistical tests of significance and analysis of variance. The bioassay he developed for the quantitative measurement of vitamin D (*Biochemical Journal* (1928) 22 60) was in use, substantially unsurpassed, for over 40 years, a record that few other tests can rival. Though Alfred maintained his interest in the use of animals for bioassay throughout his career, he was quick to appreciate and employ novel methods as soon as they became available. Colorimetry in the 1920s, spectrophotometry in the 1930s followed

by adsorption chromatography in the 1940s and then partition chromatography and paper electrophoresis until his retirement at the age of 65 in 1956.

Bacharach's long concern with food analysis is demonstrated by his work as chairman of several groups set up to investigate pressing analytical problems of a topical, and often controversial, nature; for instance, the Milk Products Committee (1932–1936), the Biological Methods Group (1943–1947) and the Vitamin E Panel (1953–1959). He also chaired the multi-centre trial for the Assay of Vitamins A and D in herring (1942). Though quick to employ and develop new chemical and microbiological procedures his continued interest in the use of animals essential for research was shown by his publications on their breeding and maintenance, his membership of the Research Defence Society, his support as Secretary to the Conference on the Supply of Experimental Animals and his membership of the MRC Advisory Committee on Experimental Animals.

It is difficult now to understand the background to research on food when Alfred began his scientific career. Though the Biochemical Society began in 1913 there was no university department of biochemistry separate from those of chemistry and physiology. Nutritional investigations were showing great promise but there was, in the UK, not a single university department or academic organization devoted to nutritional research. There were other problems too – few industrialists recognized the value of original research. Those engaged in commerce believed it to be a hobby pursued by long-haired intellectuals while, conversely, industrialists were thought by academics to be ignorant money-grabbers, totally unable to appreciate the importance of academic investigations. Bacharach realized the need to overcome these obstacles to communication and, through his wide circle of industrial and academic acquaintances, was able to show how scientists from these different backgrounds could effectively co-operate to their mutual advantage.

From first to last Alfred was proud to refer to himself as a food chemist, and did all he could to promote the science of food and nutrition. He was an active member of several scientific societies and a founder member of the Food Group of the SCI. In 1940 an Informal Conference of Nutrition Workers considered ways in which scientists from different disciplines could help meet the many problems foreseen for the forces and the civilian population during the war. At this Conference and the subsequent discussions Alfred Bacharach was responsible for many of the suggestions, developments and appointments which led to the formation of the Nutrition Society in 1941. Its meetings during those wartime years were of inestimable value in defining problems, indicating work needed to solve them and indicating the steps that needed to be taken to avoid nutritional deficiencies. The rules and by-laws of the Nutrition Society were devised by Bacharach in collaboration with Professor R.C. Garry. While Bacharach served as Honorary Treasurer for ten years from 1942 he also played an important role on the Programmes Committee as well as representing

the Society on the Parliamentary and Scientific Committee. He was President from 1959 to 1962.

Alfred had a great passion for, and mastery of, the English language, spoken or written. He once said 'language is the most important tool that the chemist possesses', and often demonstrated this view in his numerous contributions to meetings and publications of scientific societies. S.K. Kon, in writing about Bacharach, noted '... I will say unashamedly and with pride that he taught me much, often the hard way, in my long years as editor of the Society's publications ... he was repelled by slovenly thinking and its sloppy presentation, and in his editorial work said so. Yet such was the guileless gusto of his remarks and their elegant force that seldom did the castigated take offence'. Others have remarked on his 'flair for posing key questions' and 'his knack in setting the atmosphere for a lively exchange between experts'. It is thus not surprising that Alfred's ability as an editor and chairman were in great demand. He not only served on the Editorial Board of the British Journal of Nutrition for 16 years, but also from time to time on the publications and library committees of other bodies including the Society for Analytical Chemistry, the Chemical Society, the Society of Chemical Industry and the Royal Institute of Chemistry.

There was no doubt of Alfred's command of grammar, syntax and English usage. He was often asked, both here and abroad, to present his very amusing but exceedingly helpful lectures entitled 'Writing Wrongs', which unfortunately have never been published. His more formal 'Notes on the Written Word' were distributed to his colleagues at Glaxo Laboratories, but these very useful suggestions as to how scientific information may be presented in an unambiguous and readable form regrettably have not reached a wider audience. He did, however, write a number of books and edited many more. His *Science and Nutrition* (1938), with its touching dedication, in Latin, to the white Norwegian rat, is still a pleasure to read. He excelled in persuading leading research workers to contribute to joint volumes, typical of which were *Principles and Practice of Chromatography* (1943), *The Nation's Food* (1946), *Hormones in Blood* (1961), *Drug Activities* (1964) and *Exploration Medicine* (1965).

Alfred's great interest in music led to the publication of several books by Pelican Press. He wrote *British Music of our Time* (1946) and edited *The Musical Companion* (1934), *Lives of the Great Composers* (1935) and the six volumes of the *Music Masters* published between 1946 and 1954. He was a skilled pianist and, though he refused to act as a soloist, he invariably played the piano in the intimate chamber music concerts, trios and quartets which he arranged, first in his parents' house and later with his wife, Lillie. These evenings were attended by many who became celebrities in the world of music, amongst them Harriet Cohen and Sir Arnold Bax, who was godfather to one of their two sons.

Alfred was always impeccably dressed in a well-cut suit with silk handkerchief and tie to match. He invariably wore a rose from his own garden in his buttonhole. He could, on occasion, be seen striding across Hampstead Heath in a long black cloak. He was also notable for his luxuriant white hair, although F. Le Gros Clark (who lost his sight in the early 1920s) remembers him as being remarkable for his flowing dark locks! And there was always the pipe, of curious shape and strong smell.

Though considered by many to be a leading expert on healthy eating, Alfred never let the prevailing nutritional beliefs, prejudice or consensus interfere with his own preferences and antipathies. 'Nutritional advice', he averred, 'should be given to others, not to one's self!' He firmly believed that, given freedom of choice, the senses and the appetite offered to the normal person a far better guide to health than any fashionable nutritional dogma. Looking, with hindsight, at the rise and fall of many nutritional beliefs over the last 70 years one cannot but agree with his foresight and wonder if he would change his mind today. He had his aversions – he would not eat any pudding if it 'wobbled' – but he had a gourmet's appreciation of food and wine. The Grand Chateau Latour (vintage 1925) was found 'quite perfect' at 'Escargot' in 1931.

In the laboratory, or indeed elsewhere, he did not suffer fools gladly. He was as fastidious with his apparatus as with his appearance. He disliked others using his personal wash bottle so this was labelled 'Bisnorethanol' or 'Normethanol', a nomenclature that deceived some! He was readily approachable and always willing to discuss problems, except when commuting to and from work on the train. This period was the time for concentration on *The Times* – its leaders, letters and crossword were priorities, followed by other demanding reading.

He was meticulously careful not to claim credit for more than his share of work, and often his contribution was not fully appreciated. It was remarked of Alfred that 'he was brilliant, and had he been willing to sacrifice the wide diversity of his interests he could have made an international reputation in any of several facets of science'. In fact he was highly regarded by nutritionists and food chemists throughout the world. He could almost certainly have achieved greater recognition had he restricted his activities to a single area but, had he done so, we working in the field of food and nutrition could well have found our progress delayed. In fact, his breadth of knowledge, together with his involvement in other fields, was a major factor in facilitating the advance of nutritional science. Fortunately for all of us he was not interested in personal aggrandisement. Alfred Bacharach died on 16th July 1966.

Contributed by W.F. Cuthbertson and Marie E. Coates, 1990

Sir David Cuthbertson

Director,
Rowett Research
Institute, Aberdeen
President 1962–1965



*Sir David Cuthbertson CBE, DSc, MD, LLD, FRSE, FRCPE
First biochemist to this infirmary 1926 to 1934 whose work laid the foundations
of nutritional care of the ill patient. Erected by past and present members of
staff and friends 1987*

I had just graduated in medicine when Professor Cathcart, who was my great friend and guide through most of my career, said to me, 'There is a post at the Royal Infirmary, of Lecturer in Pathological Biochemistry and Clinical Biochemist to the Hospital. I think this might suit you very well, and your earlier training as a pure chemist will undoubtedly help. I'll arrange with the Principal of the University to interview you.' At that time the Principal, Sir Donald MacAllister, who was slightly lame, preferred to interview people in his study in his house within the precincts of the University and there he told me about the post and if I wanted it he thought that, providing the Court agreed, this could be arranged. So I was greatly delighted; then he mentioned the salary and I was even more delighted and when I told Professor Cathcart the good news he said, 'Now what did he offer you?' I told him, and he said, 'But you can't accept that; that's as high as my senior lecturers are getting and you're just beginning. Go and tell him that it's too much and ask him to lower the salary.' Before doing that, I consulted my father, who was Secretary of the West of Scotland College of Agriculture in Glasgow. He said, 'Cathcart has been a good friend to you and I think you should do as he tells you.' So I made another appointment with the Principal and went to see him. He misheard me and thought at first that I was asking for more salary, instead of which I was asking for less. He said, 'You silly fool, you'll need it all', and that was the end of that episode.

So I joined the staff of the Royal Infirmary. There was no laboratory. However, the pathologists gave me a little room and I started, but of course I had only just graduated and had no clinical experience. While I was gathering apparatus together, I decided that I would go round the hospital, as a kind of itinerant outdoor resident, doing some service in the various units that I might be in contact with later and finding out what their problems were. I was very glad that I did this, because in one of the surgical units a Mr George Stevenson said to me, 'If you could tackle the problem of the slow union of fractures of the lower third of the tibia, this would be a substantial piece of work'. Thinking that balances of calcium and phosphorus would be appropriate, I set up a metabolic unit in the wards to begin with, but later I found that I had to get a place of my own where I could be certain that the diet fed was the diet as calculated and that all the faeces and urine were collected. I found that while the calcium input and output roughly balanced, there was a considerable loss of phosphorus, reaching a peak at seven or eight days after fracture. I could not explain this, but I had earlier done a little piece of research on the effect of fatigue on phosphorus compounds in the muscle of the cat. So, I thought to myself, could the phosphorus be coming from muscle and, if so, what about nitrogen? I then measured the nitrogen balance, and the negative nitrogen balance was roughly in parallel with the phosphorus. Then I measured sulphur balance and it also was parallel with the nitrogen. We also measured the creatinine (later also creatine) and potassium excretion following injury. This

occurred in 1926, and by 1927 I had persuaded the management of the hospital to give me sufficient space to have a metabolic unit for six beds, plus a little dietetic kitchen, because there was no general dietetic kitchen in the hospital at that time. I also had a Sister to look after the cases. I did not take clinical care of the patients, but I arranged with the Clinical Department from which they came that their resident would visit daily and report to the chief concerning the progress of the patient. They, in turn, wouldn't do anything untoward to disturb my balances unless it was really urgent and preferably after consultation with me. I decided that first I must find out how far the negative balances were due to disuse atrophy so I got some students and persuaded the management to give me a little money, £2 a week, to pay the students to lie in bed encased in a long osteotomy splint, with the unsplinted leg tied to the bound leg. We found that there was a response to disuse, but not on the scale that was appearing as a result of fracture. Admittedly, we could not give the degree of immobilization that resulted from splinting the real fracture, but the experiment gave us a great deal of information. Then I decided that we needed to know whether there was increased metabolism in general in patients with fractures. There was a rise in oxygen consumption that paralleled the urinary nitrogen excretion, mainly as urea, not always exactly, but in general there was a relationship between nitrogen, phosphorus (mainly as phosphate) and sulphur (mainly as sulphate excretion), also creatine, and potassium excretions and oxygen consumption. The rise in oxygen consumption was of the order of 10 to 25%, and a slight fever. I also tried the effect of increasing the energy intake considerably above requirements and also the protein intake on the nitrogen imbalance. These reduced the degree of protein loss, but at the height of the protein catabolism in the severely injured, which I called the flow period, it was not always possible to achieve a positive balance during the first ten post-injury days. The early post-injury period was a period of depressed vitality, which I termed the ebb period, and corresponded to the shock period.

In 1934 Professor Cathcart invited me to come back to the department where I had done my earlier training and be the Grieve Lecturer in Physiological Chemistry. In his earlier days he had been a Grieve Lecturer. This post gave me ample lecturing experience, and I also had to teach physiology to non-medical students, for example those studying for the degree of Bachelor of Education.

At that time Cathcart had received some results from the College of Domestic Science, where Miss Mary Andross taught physiology, which were interesting but difficult to explain. She had been trying to differentiate between the value of the protein of boiled milk as against pasteurized and raw milk. There was a student in our department, Hamish Munro, who was available to help me and he and I set about trying to find out why these strange results being reported were occurring. We experimented on ourselves, and came to the conclusion that when one added a litre of milk, whether boiled or pasteurized or

raw, to the normal diet on which one was balanced in respect of energy and nitrogen, the increased nitrogen excretion was less than we would expect from the increased intake of protein in the extra milk supplied. This extra intake of energy caused less nitrogen to be excreted than before. We found that an increased intake of fat but particularly of carbohydrate had the same kind of effect – a protein saving effect, and that the results found by the domestic science students were simply the effect of surfeit in their intake of energy above that of their normal base level.

One of our Heads of Department was Professor George Wishart, and he said he couldn't possibly gain weight taking an extra litre of milk a day. I said to him, 'I am sure you would, George'. George tried for a time, and then said, 'Do you mind if I take half a litre because I tend to fall asleep on a litre?' I said, 'No, if you take a litre of milk consistently every day you will put on weight'. After a couple of months, there was no change in his weight. He led a very orderly life, always the same pattern, and then one day he said to me, 'Cuthbertson, my wife said to me, "George, why are you eating fewer potatoes now than you used to?"' He unconsciously had cut down on his potatoes to avoid an imbalance of energy intake over expenditure.

When the war started I got involved in the Home Guard, and eventually I had medical charge of the eight to nine battalions in Glasgow. At that time the War Office had asked the Medical Research Council if they could get someone to go into the question of the effect of the analeptic substances the Germans were reported to employ to make tired men less tired and allow them to go on fighting. It had been found that the substance the Germans were using was methedrine, a methyl benzedrine. The War Office was anxious to find out about this effect for the North Africa campaign. I got Dr Alan Knox, who was a member of our physiology staff in Glasgow, to collaborate with me, and within some six weeks we managed to get an answer which was of value to the War Office. We weren't able to publish this until considerably after the war.

About 1943 Sir Edward Mellanby, the Secretary of the Medical Research Council, asked Cathcart if they could possibly get me loaned to go out to one of the fronts and carry on observations of the metabolic response in battle casualties, along with a professor of surgery interested in this. But, before that actually happened, Dr Alan Drury of the MRC staff, who was also interested in the effects of injury, became Director of the Lister Institute of Preventive Medicine, and Mellanby then decided that I should work at the MRC's head office, then housed at the London School of Hygiene, where I worked for two and a half years, until the end of the war. I acted as a medical liaison officer with the Armed Forces and US Medical Research.

Towards the end of that period the Colonial Office asked if I would go out to Newfoundland and advise the people there how to better their nutritional state. So I went to Newfoundland in 1945 for seven weeks. I carried my office in a black bag and travelled about all over the place. I was helped greatly by

the Medical Officer of Health, and I wrote a report and sent it to the Colonial Office when I got home, but I never heard what happened to that report after it was published until recently, in fact a month ago. A friend said to me, 'It would be interesting to know what has happened to that report', so I wrote to the Dean of the Faculty of Science in St. John's, Newfoundland, whom I knew well, and learned that this report had been very favourably received by the Commissioner of Health and Welfare in St. John's, and all the suggestions that I put forward had been implemented in large measure. I had hoped to have been able to get the population to take a greater interest in growing their own fresh green vegetables, but apparently this had still not been successful. The short summer and rather miserable damp weather with misty fogs were not favourable for reasonably rapid growth of plants. In more recent years the Newfoundland cloud banks have been less dense and as a result the place is more sunny. However, my report apparently had been largely successful, and for 30 years, according to this Newfoundland doctor, they had acted on it and it had also been used quite a bit in Canada. It was called the *The Cuthbertson Report*, but apparently no one had thought to tell me about it. After the lapse of some 30 years the economic circumstances had improved so greatly that there was gradually no need for all the precautions and enrichments that I had suggested, because the population were, in general, now able to buy a very considerable variety of food, and transport to the outports, formerly isolated, and mainly by boat, had been greatly improved.

Before I had gone to Newfoundland an advertisement for the Directorship of the Rowett Research Institute in Aberdeen had appeared and, after some thought and consultation with other people, I decided to apply. Sir John Boyd Orr, the Institute's first Director, was retiring and shortly afterwards was appointed Director-General of FAO. When I went to see Professor Cathcart after I got back from Newfoundland I told him what had happened, and he said, 'Oh, I am sorry you've done that, because I had planned that Norman Wright should go to the Rowett and that you should go to the Hannah as Director'. Anyway, I got the Rowett job and I was very delighted. It was a tremendous challenge.

Prior to the war John Orr had established nutrition surveys, mainly with families with large numbers of children, and his classic *Food, Health and Income* had appeared. Then he had set in motion a UK Carnegie Survey and I found, when I got to the Rowett in the autumn of 1945, that this had still not been published. With some difficulty, but to help me over this, I got assistance from Dr Douglas Harvey, who had retired from the Commonwealth Bureau of Nutrition as its Director. I was charged, when I went to the Rowett, to try and improve the nutrition of animals of agricultural importance and to forget about man, for the time being at least. If anything came along of direct human interest, I should report that to the

Medical Research Council, or the nearest Medical Unit, and they would then take that in hand if they thought it suitable.

I found that there was so much of interest in the nutrition of ruminants in particular, and their microbiological processes, that no matter where you started you were able to discover things of considerable importance. It was tremendous how much could be done, and I went on being interested myself in some areas of this. However, to balance matters I had succeeded Cathcart in 1946 as consultant adviser on Nutrition and Physiology to the Army so that I did keep in touch with human affairs. Later I became a member of the Council of the British Nutrition Foundation, and subsequently became its Honorary President.

In 1965 I retired from the Rowett, and a week after that I started working again at the Royal Infirmary in Glasgow in my old laboratory, now greatly enlarged. I was fortunate to get Medical Research Council support for this for five years, and thereafter part-time from Greater Glasgow Health Board. I have remained here for 21 years, that is up to date, and I have been here in semi-retirement longer than I was in any other job. The Medical Research Council was very generous in giving me a grant to enable me to continue working and to have an assistant, and I was interested in the problem of the environmental temperature in relation to the metabolic response to injury. At temperatures of 28–32°C there was less loss of nitrogen than at the normal British hospital temperature which was usually about 19–21°C. In America the hospitals are run at about 25°C, and they did not get quite the same effect of raised temperature that we were getting here in Scotland.

In concluding this part of my reminiscences, I should like to mention that a bas-relief of my features has been cast in bronze. It was unveiled on 10th January 1987, and is installed alongside similar bronzes of Lord Lister and Sir William Macewan in the entrance hall of the old part of the hospital. It was in recognition of my observations on the metabolic response to injury made between 1926 and 1934. It is of interest that in the period 1974–1986 there have been some 500 references to my earlier work of over 50 years ago.

I have always maintained close contact with the Nutrition Society throughout my career; now I am an Honorary Member of the Society, and I deem this a very great privilege indeed. I was President for a period, before getting the status of honorary member. I had one difficult time as President. Perhaps I shouldn't describe it as difficult: awkward is the better word. We had a very good journal edited by Dr S.K. Kon of Reading. He had been editor for quite a considerable number of years, and he was very devoted to the task of editing the journal. The journal had a high status in the world of literature of nutrition, but members of the Society began to feel reluctant to accept some of the criticisms that were levelled at them by the Editorial Board. It was a powerful Board, but still these criticisms were mounting, and it was thought perhaps a change might freshen the Society a little by having new members with

slightly less restrictive, or more tolerant views. At the same time there was considerable doubt as to the wisdom of such a change.

As President, I felt that Kon had done an extraordinarily good piece of work over a considerable time. However, I thought, having been on the Editorial Board myself since its beginning, I should perhaps step down and make room for someone else. I made that proposal at a meeting, and I also raised this question of fixed period of office as Editor. This we managed to effect. I am sure we all felt what a wonderful job Dr Kon had done but, nevertheless, we also felt that it was right and proper that other members should have the privilege of acting as Editors. It was a difficult, rather sticky time for me, but I think we did the right thing. I am still an Honorary President of the International Union of Nutritional Sciences, having been appointed to this post at the time of the Congress in Mexico City.

Recorded with Elsie M. Widdowson, 1986

Dr S.K. Kon

Head of Department
of Nutrition,
National Institute for
Research in Dairying,
Shinfield, Reading
President 1965–1968



Stanislas Kazimierz Kon, born in Russia on 1st January 1900, was of Polish nationality and grew up in Lodz. He attended the University of Warsaw, but his studies were interrupted by the Russo-Polish war, during which he served in the Polish cavalry and was awarded a military medal. He resumed his university course after the war and gained a PhD in Chemistry in 1923.

During the previous decade Professor Kazimierz Funk at the School of Hygiene in Warsaw had established a worldwide reputation for his researches into the causes of beriberi, pellagra, scurvy and rickets. He believed they were due to a deficiency in the diet of 'special substances which are of the nature of organic bases, which we will call *vitamines*'. Kon worked under Funk's direction from 1924 to 1930, and this association undoubtedly laid firm foundations for his future career in nutrition. With Funk's help he obtained a two-year Rockefeller Travelling Scholarship and used it to broaden his knowledge of nutrition in the laboratories of such eminent scientists as Drummond on London, Hopkins in Cambridge and Steenbock in Wisconsin, while his interest in hormones took him to study with Herbert Evans in California.

On his return to Poland he began to seek opportunities elsewhere and as a result was offered two posts, one in Canada and one in England. He chose the latter, with Drummond's backing, and so began his career at the National Institute for Research in Dairying in 1930, which continued until his retirement in 1965. As an assistant in the Chemistry department his remit was to study the nutritive value of milk and dairy products. He established a colony of Norwegian hooded rats from a nucleus provided by the Lister Institute, and used them in conjunction with the available chemical tests to study the vitamin, mineral and protein components of milk and the factors affecting their nutritional quality. These researches earned him a DSc from Reading University in 1936, and in the same year he became a British citizen.

In the early years of the NIRD the Chairman of its Board was Lord Iveagh, who managed an extensive dairy farm in the neighbourhood and often came to the Institute for advice. In 1937 he generously endowed a Department of Physiology and Biochemistry, with Kon as its head. Nutrition was at that time regarded as part of the wider subject of biochemistry, but a few years later its identity was acknowledged at the NIRD by separation of Kon's domain into a renamed Department of Nutrition.

The recognition of nutrition as a subject in its own right was not confined to Reading. Throughout the United Kingdom there was a movement to create a special forum for nutritional scientists to discuss their work and ideas, and in 1939 food and nutrition assumed paramount importance with the need to provide an adequate diet for a nation at war. Kon, along with many other nutritionists in the UK, was pressed into service for the Ministry of Food, and his department contributed important data on the nutritive value of available foods such as the National Loaf and processed milks. At the same time he was deeply involved in the plans and preparations that culminated in the formation of the Nutrition Society in 1941. His special contribution, with Margaret Hume, was the editing and publication of its *Proceedings*. These contained records of papers given at the Society's symposia, and abstracts of brief original communications. With the cessation of hostilities a great increase in

research activities created a need for a journal devoted entirely to accounts of original research. Thus in 1947 the *British Journal of Nutrition* was established with Kon as its honorary editor, an office he held until 1965.

At first the work was done mainly in his own office, with part-time secretarial help, but as the journal grew the paraphernalia of editing began to overflow and the need to house it separately became pressing. The NIRD had little space to spare but a solution was eventually found. The rat house, long ago converted from the vinery of Shinfield Manor, had a loft. It was reached via a retractable wooden staircase, and was the repository of stacks of superfluous rat cages and other animal house equipment. But at the far end a desk, a typewriter and some filing cabinets were installed and it served as the office of the *British Journal of Nutrition* for many years. Barbara Smyth was appointed as editorial assistant in 1949, and her patience and quiet efficiency provided exactly the support Kon needed for an onerous and time-consuming task.

Kon was a natural linguist and his European upbringing had made him fluent in several languages. His knowledge of English grammar and vocabulary was better than that of many of his colleagues. Few papers escaped scarification by the editorial pen, and his insistence on clarity and attention to detail, for example the correct use of 'which' and 'that', set the high standards of presentation on which the *British Journal of Nutrition* justly prides itself. The breadth of his research experience also contributed to Kon's success as an editor. In the course of his work he had been concerned with nutrition of farm animals, poultry, laboratory animals and human subjects, and his research on vitamins had even led to investigations in whales and other marine life. An ongoing interest in composition of milk gave rise to studies in exotic species, and his recognition of the nutritional importance of the gut microflora resulted in the installation at NIRD of the first facility for gnotobiotic animals in the UK.

In 1965 Kon was elected President of the Nutrition Society. Noteworthy events in the Society's history during his term of office were the beginnings of the travel fund established to assist members to attend international meetings, and the formation of the Royal Society's British National Committee on Nutrition, on which the Nutrition Society was invited to send representatives. The activities of IUNS were growing in number and effectiveness, and Kon was the representative for the UK. A Trust had been set up in memory of Sir Jack Drummond to provide an annual prize for outstanding research by a young nutritionist. The officers of the Nutrition Society, with Professor F.G. Young, were given responsibility for selecting the successful candidate, and the first award was made in 1966 to Dr Roger Whitehead.

In his later years Kon received many honours. For his work on national committees and commissions he was appointed a Commander of the Most Excellent Order of the British Empire. The year 1969, in which he was elected an Honorary Member of the Nutrition Society, also saw his election as

a member of the Polish Academy of Sciences and a fellow of the American Institute of Nutrition. After his retirement from NIRD he retained his interest in nutrition as a member of the staff of the Commonwealth Bureau of Dairy Science, where his linguistic abilities, especially his acquaintance with the lesser known languages of Eastern Europe, were invaluable to the production of Dairy Science Abstracts and, later, Food Science and Technology Abstracts. He ceased to attend meetings of the Nutrition Society when he moved to the West Country, but continued to prepare his abstracts until shortly before he died. Stas Kon died in September 1986.

Contributed by Marie E. Coates, 1990

Dr J.A.B. Smith

Director,
Hannah Research
Institute, Ayr
President 1968–1971



In the early 1930s probably relatively few people set out to study the subject of nutrition; many tended rather to come to it from other subjects, and certainly I entered the field of nutrition in that way. In the late 1920s I did research on carbohydrates in Birmingham University under the direction of Sir Norman Haworth and Sir Edmund Hearst, who later synthesized ascorbic acid. That was followed by three years as a junior assistant to Professor A.C. Chibnall, first in University College, London, where we occupied a laboratory belonging to Professor Jack Drummond, and then later at Imperial College. During that period my research work was concerned with the lipids of grass.

In 1932 I was appointed a lecturer in biochemistry at Liverpool University under the direction of Professor H.J. Channon, and in the next four years my research work on lipids continued, but at the same time I learned much about

nutrition and I was even bold enough to lecture on some aspects of the subject. In 1936, wanting to devote more time to research, I accepted a post as Biochemist at the Hannah Dairy Research Institute near Ayr, and it was there that my active interest in nutrition problems developed. Naturally my work there was concerned with milk, and I recalled some of my earliest recollections at the village school which I attended in a remote part of South West Scotland in the years 1911–1922, where my father was a medical practitioner. There were numbers of children at that school who had repulsive looking tuberculous glands in the neck, or ugly scars remaining where such glands had been removed. There were one or two children who were affected by the disease, bovine tuberculosis, in other parts of the body and who as a result were bedridden for long periods; such diseases had all come from milk.

In 1929 the Hannah Institute, whose Director was Norman Wright, had begun an investigation of the feasibility of eradicating tuberculosis from dairy herds; the investigation was made under the auspices of the Medical Research Council, and the result of that and other work was that Ayrshire became the first area to be declared free from bovine tuberculosis. It was not until 1960 that the whole of Britain was regarded as free. Then again in the 1930s there was already much evidence that pasteurization of milk would be of great value in making milk safe for children and adults, but many people believed that the nutritive value of milk was bound to be adversely affected by any kind of heat treatment. Apparently it was perfectly in order and absolutely right, in the view of most people, to cook meat and to cook eggs and fish before eating them, but milk must be taken straight from the cow.

Very heated controversy continued over a period of years regarding the merits and demerits of pasteurization, and round about the time when I joined the staff of the Hannah Institute in 1936, this controversy was at its height. The Institute made valuable contributions towards getting the pasteurization process carried out efficiently and showed also that when it was done efficiently the nutritive value of the milk, even for calves, was not significantly affected, and the spread of bovine tuberculosis amongst the calves was absolutely prevented.

In the late 1930s some of my own work at the Hannah Institute was concerned with factors that affect the milk composition and therefore its nutritive value and later, turning to quite a different matter, I spent a few years studying the utilization of non-protein nitrogen by ruminants. We showed that in the ruminant, non-protein nitrogen can be used to make protein, but at the same time in the rumen protein can be broken down to give non-protein nitrogen, and which process supersedes or prevails over the other depends on quite a number of factors.

At that time I took an active part in studies on dried milk, its flavour and storage properties and on its nutritive value compared with that of the fresh milk from which it came. These investigations were done in collaboration with

research workers at the National Institute for Research in Dairying at Reading, and the Low Temperature Research Station at Cambridge.

Late in 1942 I received an invitation to give a paper at a meeting of The Scottish Group of the Nutrition Society which was to be held in the Physiology Department of Glasgow University on 20th February 1943. The subject of the meeting was the influence of diet of the mother on pregnancy and lactation. In fact there were to be two meetings covering this general subject, the first on pregnancy and the second on lactation. My paper of course was given at the second meeting and was entitled 'The effect of diet on lactation in animals'. Not very long after that meeting a letter came asking if I would allow myself to be nominated as Secretary and Treasurer of The Scottish Group, in place of Mr J.S. Thompson of the Imperial Bureau of Animal Nutrition, who had held the office since the inauguration of the group in January 1942. I had agreed to let my name go forward and was duly elected Secretary and Treasurer at a meeting of the group in University College, Dundee, on Saturday, 29th May 1943. Incidentally, I see from my diary for that year, that my lunch and other light refreshments for which I paid at that meeting cost me four shillings, or twenty new pence.

For the next five years, until 1948, I organized all the scientific and business meetings of The Scottish Group and as its Secretary I also attended meetings of the Council of The Nutrition Society.

Although Scotland is such a small country with only some five million inhabitants, it had by the early 1940s, and still has, many institutions which were actively interested in nutrition and at several of these institutions research work relating to the subject was in progress. For instance, at Aberdeen, there was the University with its medical and agricultural faculties, there was the Rowett Research Institute for Animal Nutrition with John Boyd Orr as Director, The Imperial Bureau of Animal Nutrition with Isabella Leitch as Director and there was the Torry Research Station for research on fish where they were also actively interested, for example, in the flavour and the nutritive value of the fish, its keeping quality and its appetising appearance.

Then coming south, in Dundee there was University College, at that time part of St. Andrews University and Professor R.C. Garry was Professor in the Physiology Department. In Edinburgh, in the University Medical Faculty in the Royal Infirmary, Professor Murray Lyon was very interested in the subject of nutrition. One of the first, possibly the first full time hospital dietitian, Miss Buchan, worked at The Royal Infirmary and she was extremely interested in the subject. Then there was the Royal Veterinary College with Professor Dryerre and later his successor Sir Alexander Robertson.

In Edinburgh also there was The Poultry Research Institute, where nutrition was studied, and also The Animal Diseases Research Institute with Dr Russell Greig. In Glasgow there was the University with Professor Cathcart, Professor G.M. Wishart and Dr D.P. Cuthbertson and the West of Scotland College of

Domestic Science, where there was Miss Mary Anderson, who was a tremendous enthusiast and who lectured to her pupils on nutrition and took an extremely active interest in the subject.

At Ayr there was the Hannah Research Institute, with Norman Wright as Director and the West of Scotland Agricultural College with George Dunlop, who had done work at Cambridge on the nutrition of sheep. For its size, therefore, Scotland had a relatively large number of people and institutions concerned with nutrition.

Another but related matter which led to the formation of the group was the difficulty and expense of making long journeys to meetings in the South of England, particularly in wartime. If I remember correctly, at every railway station booking office in those days there was a large placard asking 'Is your journey really necessary?' It was felt by many of the people just mentioned that there would be a tremendous benefit from having a Scottish group within the Society which would organize meetings in Scotland where junior workers, as well as senior, would be able to attend.

I believe that when the idea of having a Scottish group was first mooted, several people in the South felt that this might lead to the new society being centred in Scotland and having its headquarters in Aberdeen and being organized there, but to the credit of all concerned these difficulties were dispelled, other small problems were solved and the Scottish group was formed. The group was inaugurated at a meeting at University College, Dundee, Professor Garry's department, with Sir John Boyd Orr in the Chair on 17th January 1942. The first scientific meeting was held in Perth Station Hotel on 14th March 1942 with Dr A. Davidson, Medical Officer of Health for Scotland, in the Chair; the subject was food supplies in relation to human needs. Perth Station Hotel was chosen because it was as central as anything could be for Scotland and various people could come from all the four quarters and attend the meeting.

There was a nice legal point about all this in the early days of the Society. Reference was sometimes made, even in print, and you will see this in the early Proceedings, to meetings held by the English group and meetings held by the Scottish group, as if the Society consisted of two separate groups. The true position was, and still is, that there was the Society as a whole, but within the Society there was a group that met in Scotland.

Dr S.K. Kon was a great friend of mine and one whom I admired very much. He had a wonderful command of the English language though his native country was Poland, and he did excellent work as Chairman of the Editorial Board of the Proceedings of the Society and later of the journal. He invited me to join the Editorial Board in 1946 and, except for a break between 1964 and 1966, I remained on the Editorial board of the British Journal of Nutrition until 1971, a period of almost 25 years. During that period I was a member of Council for one or two periods and, because it was a courtesy in those days to ensure that every now and again there was a President from Scotland, for some

reason or other the lot fell upon me. I served as President of the Society for the three years, 1968–1971.

The Fifth International Congress of Nutrition was held in Edinburgh from 9–13th August 1963 and I was Honorary Treasurer and Chairman of the Finance Committee for the Congress. The Honorary President was Lord Boyd Orr, the President was Sir David Cuthbertson, the Chairman of the Organizing Committee Dr C.P. Stewart and the Honorary Secretary was Miss A.D. Watson. Miss Watson was an extremely able retired senior civil servant, and her help and guidance were absolutely invaluable both in organizing the congress and throughout. In fact during the Congress she wore a rather bright green hat and if anyone from this country or from abroad had any problems, they looked for the lady with the green hat and she invariably solved the problem for them.

The Finance Committee, of which I was Chairman, included Sir David Cuthbertson, Dr and Mrs Magnus Pyke, Professor A. Robertson, Dr C.P. Stewart, Miss A.D. Watson, and a well-known dietitian, Miss M.G. Watt. It is interesting to notice that Dr and Mrs Magnus Pyke were on the Finance Committee; Dr Pyke was centred at that time in Perth with the Distiller's Company and his wife was a chartered accountant. She had a large practice in London and also one in Edinburgh and she very kindly joined our Finance Committee. She was the official auditor for the Congress, and being on the Finance Committee helped her when it came to auditing the accounts.

Another very interesting job I have had was being on the Editorial Board of *Nutrition Abstracts and Reviews*, a journal which is known the world over. It was produced at the Imperial Bureau of Animal Nutrition, which later became the Commonwealth Bureau and which was situated at the Rowett Research Institute. The Director of the Bureau was Dr Isabella Leitch and the Secretary for a period of several years, Mr J.S. Thompson. In 1946 I was invited to join the Editorial Board of *Nutrition Abstracts and Reviews* and I remained a member until 1968 – 22 years or so, and during the last few years of that period I acted as Chairman of the Board. It was a great privilege for me to serve on that Editorial Committee, which included people like Sir David Cuthbertson, Miss E.M. Hume, Dame Harriette Chick, Miss A.M. Copping, Dr Angus Thomson and latterly Sir Kenneth Blaxter.

Finally I might add just one further activity and that was with the International Union of Nutritional Sciences. At a Congress in Prague I was asked if I would become Honorary Treasurer of the International Union; I took it on in 1969 for six years and naturally during that period I got to know many well-known nutritionists from all over the world and greatly enjoyed working with them.

Recorded with Elsie M. Widdowson, 1986

Dr Egon H. Kodicek

Director,
Dunn Nutritional
Laboratory,
Cambridge
President 1971–1974



I was born in 1908 in the south of Bohemia, in a little place called Kemenny Ujzed on the River Moldau, a few miles from Budweis, famous for its beer. Bohemia was then still part of the Austro-Hungarian Empire. I went to a tiny school in the village, and my father was a general practitioner. In 1914 we moved to Budweis, where he became military commander of the hospital. I went to the gymnasium or grammar school, and from there to the Czech

University in Prague for my medical studies. I graduated in 1932 with an MD degree. I stayed in the Department of Internal Medicine and there got to know my future wife, who had been a medical student at the same time. She went on to become an ophthalmic surgeon while I became Assistant, which is something like a lecturer in the UK. I was in charge of the Endocrinological Department and I collaborated with the Czech Institute of Health on endocrinological methods.

My father was educated in Vienna. He was a very liberal-minded person and not a strict Jew at all. Both my grandmothers were religious, but my parents were Czech liberals. I was an only child. Unfortunately my father and mother dissolved their marriage while I was still at grammar school. Because of that I did not have an easy life, but it made me more self supporting than I might otherwise have been. I was a social democrat and a follower of Masaryk. I was very fond of his philosophy, and while I was in grammar school I read all his books. He was not very popular at that time, particularly among teachers, who were national democrats and very right-wing. I was in constant quarrel with them.

I married in 1936, and in 1937 we had a small girl, Jana. The first ominous signs of difficulties came at about the time we were married. We were on honeymoon in Switzerland and we thought war was coming, but nothing happened. The time between the two wars was a time of prosperity in Czechoslovakia. 'We had never had it so good'. Then Masaryk died in October 1937 and the political situation began to deteriorate, with increasing pressure from Germany. In September 1938 came Munich, the Germans occupied Sudetenland, and that broke everybody's heart. Many people wept and cried. It did not break my morale, however. I was only afraid of the effects of the German occupation on the Jewish population. One day I arrived at the hospital and was met by the sister with tears streaming down her face. My name had been removed from the board in the entrance hall which listed all the doctors. I had not noticed it as I came in. I let it be known that I would not resign; they would have to force me out, and this the Assistant Professor in charge of my clinic threatened to do. The Head of the Department was more sympathetic. He said, 'You have no future here. Try to get out'. He gave me the address of the Society for the Protection of Science and Learning in London and advised me to write to them, which I did. My English was not very good, but I had an English teacher who helped me to write the letter. It took some time before a reply came. It said I must first find a place where I could work. I thought of Cambridge, because I had exchanged letters and reprints with Dr Harris, the Director of the Dunn Nutrition Laboratory. So I wrote to him, but I had no reply. That was in the summer of 1938. I waited and waited, and then in February 1939 I got a letter from Dr Harris telling me he had been ill. He said he could possibly let me have a place if I could find a grant. I wrote back to the Society for the Protection of Science and Learning and nothing happened for a long time, and time was

short for me. Eventually I got a letter saying that I might have a grant of £200 a year if I managed to get to England. By that time visas to England had been introduced, so I applied for visas for myself, my wife and my child. Then on 15th March 1939 the Germans occupied Czechoslovakia. The next morning I heard that my uncle had been arrested. He was a medical doctor and a Czech nationalist. The Germans then searched out the family one after another. I did not sleep at home – I went from one friend's house to another, which was just as well because the Gestapo came to our flat. My main interest was to get out of Czechoslovakia, but I had to have a Gestapo permit to leave Czechoslovakia and Germany as well as a visa to enter Britain. In spite of the curfew I queued up all night outside the Police Headquarters, and at 7 or 8 am I came inside the office. The Gestapo asked me if I was a Jew, and when I said 'yes' he tore up the permit and threw me out. I then learned that a small travel agency was able to produce a permit if a bribe was given, but this did not work either. My wife had some friends at the Ministry of Agriculture. They were able to contact the Gestapo who were willing to provide a Gestapo permit for money. My wife is not Jewish, and this might have helped in the bribery of the Gestapo. I got the permit, but it was a false one, which I did not know till years later. It was valid for one month, that is until 7th June 1939. If I overstayed this date I would definitely be arrested. I still had no visa. I learned that if I gave 10,000 Crowns to the Czech Liberation Movement they could obtain a French visa for me. So I gave 10,000 Crowns and my passport and waited. They said I should have the visa in a week's time, but no visa came. I contacted the man, as usual during the curfew at night, and he said he could not get the passport out of the Embassy. I decided to go myself, because without a passport I was lost. When I came to the embassy door the porter warned me, 'There is a Gestapo man taking photographs'. I said I didn't care. The French Embassy knew nothing about issuing me with a visa. They found my passport in a bundle in the official's desk. It transpired that he was also being paid by the French Foreign Legion for conscripts, and anyone with a visa he issued was immediately sent to Algeria. By this time the validity of my Gestapo permit was coming to an end. I went to the British Consul, who was very sympathetic but he said he could not issue a visa without authority from London. This was a Friday and my permit expired the following Wednesday. He phoned London, and found they had issued a visa, but forgotten to send it and the man in charge was away for the weekend. The Consul told me to ring up on Saturday. I did. No news. Sunday was a lovely sunny day and we went into the park. Monday, still no news. I rang up again at 8 o'clock on Tuesday morning and the Consul said, 'I have got your visa!' I had a little suitcase packed, and about 10 o'clock in the morning I was on the train. I did not have time to say goodbye to my mother. She and one brother and four sisters died in a concentration camp.

I went on the train to Dresden, but the train was so delayed that when I changed at Dresden I found I had missed the connection to Holland and there

was not another for nearly 24 hours. An English lady who had been on the train from Prague saw me standing around and asked me what I was going to do. I said I did not know, for I had very little money and I had only 24 hours to get out of Germany. She said, 'I've got plenty of money' and she started to take pound notes out of her hair-do and gave one pound to me. I found a little boarding house where the man said he would give me a room, but if there was a raid he would have to give me up. I was so tired I didn't care, and slept like a log. In the morning I got the train to Holland.

The British Consul in Prague had said to me, 'When you get across Germany to the German-Dutch border do not say you are a medical man. If you do they will put you in a concentration camp, because they need doctors in the camps'. I remembered that, and when we reached the German border and the Gestapo took my passport and said, 'What do you do?' I said I was an Endokrinologe. They had no idea what that meant, and gave me back my passport, and that was that. The train started again and I realized I was in Holland.

When I got to England I had a good welcome from Mr Fairer-Smith, a barrister, who I first met when I was 18 and we discussed world affairs. In 1936 he came to see us in Prague. He put me up in Bayswater for a few days. I wrote to Dr Harris in Cambridge, told him I had arrived and that I should like to go to see him. I went on the following Monday. He told me it was not entirely settled that I could be accepted in the laboratory. It had to go through the trustees. One of them was Sir Charles Martin and he invited me to tea. The impression I made could not have been too bad, for he accepted me, and I had a grant of £200 a year which at that time was sufficient for one. I found a little house in Mount Pleasant for one pound a week.

Meantime my wife and child were still in Prague. When I got my visa the day before I left they were also issued with visas, but they could not use them because they had no Gestapo exit permits to leave Czechoslovakia. The Consul sent their visas to the British Consul in Genoa and said they would have to get there somehow. To help over getting an exit permit my wife had to pretend that I had deserted her and, on the advice of a lawyer friend, she started divorce proceedings. She heard that she might get a permit if she or the child was ill. The child had a cold, so she took her to a clinic where a good friend of ours was Professor. He X-rayed the little girl and mixed up her X-rays with that of a tuberculous child. She took this to the Gestapo, but they said their doctor must examine the child. My wife said she had a temperature and she would not bring her out. She was called out during the night to an interrogation by the Gestapo, who tried to get out of her whether she had contacts with me. She was determined not to admit it and she didn't. She eventually got to Genoa, with the help of the English teacher. He was able to help her see the British Consul so that she got her visa. When I got my first grant, which was £4.17s. a week, four weeks in advance, I went to the shipping company and bought her a ticket

from Genoa to Southampton and I sent it to the British Consul in Genoa where she already had a visa. The trouble was how to communicate with her. We had arranged a code and I managed to let her know her ticket was there. She was warned that the Gestapo were at the harbour examining everyone who wanted to board the ship. Again the English teacher helped by lending my wife his wife's British passport. The captain of the Dutch boat knew about this and he returned the passport to the English teacher. Evidently the Gestapo did not notice what was going on, for they did not interfere. The boat stopped in Algiers where my wife sent a postcard to our lawyer in Prague, which was a pre-arranged signal that he should cancel divorce proceedings. It was the last boat to get through Gibraltar, and in the middle of August, just before the outbreak of war, she arrived in Southampton.

In Cambridge she found the house in Mount Pleasant rather damp and cold, for we were used to central heating, and did not understand coal fires. One day we set the chimney on fire and there was quite a commotion. Also, my £4.17s. a week, which had been enough for one, was not really enough for three. However, things gradually improved. In 1941 both my wife and I were accepted by the General Medical Council as medical doctors. My wife got an unpaid job at Addenbrookes Hospital. Her only paid work was from an eye clinic once a week in Huntingdon. However, she was pleased that she could do some work.

In 1941 I had the choice of going into medical practice or staying in research. I had got so involved in my research that I decided to stay, although the remuneration was not big. I think I made the right choice. In 1943 my second daughter Ivana was born, and in 1948 we moved to our present house in Bulstrode Gardens.

From 1941–1942 I was Scientific Adviser to the Czech Government in exile. In 1942 I was offered a grant from the Medical Research Council, and from then I never looked back. I worked very hard, and my first paper was published in 1939. One of my tasks was to develop methods for estimating vitamins, for we were responsible for measuring them in wartime foods. I found that 'orange juice' which was to be shipped from New York contained no vitamin C and was just coloured water. The import was stopped. We investigated rations captured by the British in North Africa from the German tank crews. There was no vitamin C in the dried vegetables or any other food. The story was that the Germans knew about the importance of vitamin C in the prevention of scurvy, but they believed there was plenty of fruit and vitamin C in North Africa (which there wasn't), so they sent vitamin C to the troops in Norway and Denmark, but none to those in the south. The tank crews consequently suffered from scurvy and this may have contributed to the defeat of Rommel's army.

*From a recording made for the
Archives of the Imperial War Museum, 1981*

Extracts from the Biographical Memoir of Egon Kodicek, FRS. Contributed by D.R. Fraser and Elsie M. Widdowson.*

After the war Dr Kodicek affirmed his commitment to research and to remaining in the UK. Under the sponsorship of Lord Boyd Orr, Sir Edward Mellanby, Sir Charles Martin and Dr L.J. Harris he applied for British citizenship, and he and his family became naturalized British subjects in 1947.

For the next 15 years Dr Kodicek investigated ways of estimating vitamin status in experimental animals, as well as the biochemical consequences of vitamin deficiencies. He was particularly interested in vitamin C and the B vitamins nicotinic acid, riboflavin and folic acid. By 1950 he had begun his first experiments on vitamin D, which laid down the groundwork for the important discoveries to be made during his final years at the Dunn.

Although during the post-war years Egon Kodicek had made many scientific friends and colleagues throughout Britain as well as overseas, he was gradually becoming isolated and intellectually lonely at the Dunn. His interests in the molecular mechanisms of the biological function of vitamins contrasted markedly with the more conventional studies in food and nutrition going on around him. Relations with L.J. Harris became strained. Dr Harris certainly recognized Egon Kodicek's originality and flair in research, but he was losing sympathy with the attempts to bring contemporary biochemical knowledge into the laboratory. The proposal by Drs Kodicek and Cruickshank in 1950 to study the cellular distribution and biochemical fate of vitamin D in rats was sceptically regarded as a futile exercise, a waste of time and a waste of the undoubted talent of the investigators. Subsequent developments were to prove the error of this judgement.

By 1962 the Medical Research Council was beginning to contemplate the fate of the Dunn Nutritional Laboratory after the retirement of L.J. Harris as Director. This was a time of great worry and apprehension for Egon Kodicek. His environment at work had markedly deteriorated, but in general he was very happy in Cambridge. The prospect of having to move filled him with gloom, but a decision to close the Laboratory was widely expected. However, during the assessment of the Dunn the visiting MRC Committee came across a scholarly gentleman in an undistinguished back room who presented them with a visionary concept of a role for nutrition in future medical research. When Egon Kodicek came the next day to his small team his face was alight with shock and astonishment. 'They have put me up', was his enigmatic comment. Indeed, the MRC had invited Egon Kodicek to put forward a research programme and to accept the post of Director of the Dunn in October 1963. The most productive and extraordinary ten years of his life were about to begin.

By appointing Egon Kodicek as Director the MRC was acknowledging that nutrition was a worthy topic for research and that the Council was committed

to giving him adequate support. The dilapidated and old-fashioned accommodation had to be modernized, and the MRC agreed to provide an additional building to house extra staff and facilities. At the age of 55 Egon Kodicek had been given a real challenge to create a modern, viable institute which would provide a stream of new knowledge. His enthusiasm for the task was boundless, and he immersed himself in every aspect of the reconstruction. However, from his point of view, the single most important opportunity was to be able to redefine the aim and scope of research in nutrition. He proposed that, 'using modern biochemical and biophysical methods, investigations should begin into the mode of action of accessory food factors. Particular attention should be given to those factors such as the fat-soluble vitamins where the mode of action was still not clear'.

As with most aspects of his thinking, Kodicek's research methods were essentially philosophical. He would always begin a study with a hypothesis, but he did not regard any theory as sacrosanct. For him, to be wrong was an essential part of scientific progress. He considered the classification of vitamins as an unhelpful theory. They had been investigated almost entirely in terms of dietary deficiency. Such classification he regarded as having impeded our understanding of vitamin function. Two anomalies in the standard classification of vitamins concern vitamins C and D. In general biology neither of these are essential nutritional factors. Vitamin C is enzymatically synthesized in most animals except primates, and vitamin D is obtained in all land vertebrates from solar irradiation of the skin. For many years the function of these two pseudo-nutritional factors was Egon Kodicek's main research interest, but it was his ten years as Director of the Dunn that saw his greatest achievement, the discovery of the mode of action through a second metabolite of vitamin D. He did not make this discovery alone; and in fact he did not do much experimental work himself. He established a team of bright young students and colleagues – 'These people are my hands'. They discussed the work together every day, and his was always the guiding hand.

This was an exciting time at the Dunn, for it was a race against time. A team at Wisconsin, headed by Hector DeLuca, who had previously spent some time at the Dunn, and who now had much greater facilities and more staff, had already identified the first metabolite, 25-hydroxycholecalciferol, which was produced by the liver, and they were hot on the trail of the second, which was believed to be the vitamin D molecule with oxygen atoms inserted both at carbon 25 and at carbon 1, 1:25-dihydroxycholecalciferol. It was the investigators at the Dunn, however, who demonstrated that a second metabolite was produced solely by the kidneys, and they identified this as 1:25-dihydroxycholecalciferol. Its biological activity was much greater than that of vitamin D or of the 25 hydroxy metabolite and everything pointed to this being the functional form of vitamin D. It was acting as a steroid hormone in the control of calcium metabolism.

**Biographical Memoirs of Fellows of the Royal Society* 1983, 29, 297–331

This discovery brought Egon Kodicek many honours, including the Fellowship of the Royal Society. Invitations to lecture in Europe and the USA poured in, so that there were more than he was able to accept. Sadly his date for retirement in 1973 came all too soon, for he felt this should really be the beginning, not the end. Many questions still remained to be answered and problems to be solved – for example, what was the function of vitamin D in bone and in muscle, and what was the overall mechanism regulating the functional metabolism of vitamin D?

Throughout his time at the Dunn, and particularly while he was Director, Egon Kodicek's expertise and wisdom were sought by national and international organizations. He had many links with colleagues in Europe, and in 1961 he and a number of friends formed the Group of European Nutritionists. This was a small organization of elected members who were mainly senior investigators and heads of institutes and departments. The Group held annual meetings in various European countries and Egon Kodicek organized two such meetings in Cambridge, in 1968 and 1973. By 1973 he had begun to feel that this rather small club of older people was not really representative of nutritional interests in Europe. At this time he was President of the Nutrition Society, and he proposed to the Society that a European Nutrition Conference should follow the Cambridge meeting of the Group of European Nutritionists which would be open to all. The suggestion was accepted and a three-day programme was planned. The Conference was held at Churchill College and was well-attended and considered to have been a great success.

After his retirement in September 1973 Egon Kodicek worked for a time at the Strangeways Research Laboratory in Cambridge, but he missed the colleagues who had shared in the work on vitamin D, and he was plagued by ill health. As his health declined he gave up research work, and he died on 27th July 1982.

Sir Kenneth Blaxter

Director,
Rowett Research
Institute, Aberdeen
President 1974–1977



At the City of Norwich School I regularly came 22nd out of a class of 24 and it surprised everyone (myself included) when I got credits or distinctions in all the subjects I sat in the School Certificate examination. I thus entered the sixth form at age 15 to study History and English. I was not interested in these subjects and the family thought that I should prepare for a career. Accordingly I joined the Legal Department of the Norwich Union Insurance Company as a clerk with the idea that perhaps, when I was old enough, I might enter the law. I loathed the job; my interests were in the countryside, animals and farming.

The second family conference was then called to decide what should be done with 'Charlotte's boy'. My uncle, the Archdeacon of Loughborough, said 'The boy's tall enough, Lord Trenchard has opened this new college at Hendon – put him in the police'. Eventually it was agreed that since 'he has not the brains for the church and he has this interest in farming, put him on the land'. The problem was lack of capital and so it was decided that I should receive a technical training in veterinary medicine or agriculture. I jibbed at the idea of a five year veterinary course and opted to take an ordinary degree in agriculture lasting three years at Reading University. In all this process my mother had the guidance of her friend Mrs Frank Rayns who was the wife of the Director of the Norfolk Agricultural Station.

First, however, I had to learn some practical skills and became an unpaid farm worker on the Hoveton Estate of T.R.C. Blofeld. There I learned to plough with horses, milk by hand and to carry out most of the then unmechanized, practical tasks of arable and livestock farming. Then at university my eyes were opened to a world of biology which I had never realized existed. F.J. Cole, FRS, then Professor of Zoology, certainly never knew that his lectures on the comparative anatomy of the vertebrate skull excited and influenced at least one student who had never done any science before.

I graduated in 1939 and after a short time working at Sparsholt Farm Institute was appointed on 1st September to the staff of the Dairy Husbandry Department at the National Institute for Research in Dairying. There with Dr S. Bartlett I carried out feeding trials on the value of urea as a substitute for protein in the diets of growing and milking cows. I joined the army in the early spring of 1940. I was promoted several times to the exalted rank of acting, unpaid lance bombardier and fell from that rank as well.

While on leave from the army in late 1941 I received a telegram to return to my unit to learn that this was not because I had obtained a pass when under open arrest but to be discharged to return to the NIRD. The reason behind this was that German workers had shown that iodination of casein led to the formation of a thyroactive product. Following the work of H.D. Kay FRS and S.J. Folley FRS in the 1930s it was known that thyroxine would increase milk secretion in the cow and it was thought that iodinated casein could be used to increase the milk supply for the population. I was needed to run the experiments with cows. Eventually we obtained active preparations and the investigations culminated in a trial with 1000 cows which demonstrated that an increase in yield of about 20% could be obtained. Considerable loss of weight by the cattle occurred and a marked depression of yield (due to pituitary inhibition) when treatment stopped. The use of iodinated protein to increase milk production was not adopted in the UK.

My other work at this time was nutritional rather than endocrinological and related to the problems of maintaining milk yield when the imported sources of grains and oilseeds were diminished through attrition on shipping by the

U-boat campaign. I found that to study these problems in greater depth I needed more chemical knowledge and this was agreed by Dr Kay, Director of NIRD, who arranged my secondment to Weybridge to work with Dr H.H. Green in the Biochemistry Department. Initially I was concerned with analytical studies and field work related to hypomagnesaemia and hypocalcaemia in cows and to swayback in sheep. Studies on swayback in Derbyshire, which Ruth Allcroft and I undertook, indicated that lead derived from the old mine workings might have an effect on the metabolism of copper. We therefore started work on the metabolism and toxicity of lead in ruminants. The results provided diagnostic analytical procedures for lead poisoning and I discovered the role of the reticulo-endothelial system in the sequestration of lead and of the liver in its excretion. Other work at that time was concerned with zinc metabolism and the effects of experimental hyperthyroidism on mineral metabolism.

When the war ended I applied for and was awarded a Commonwealth Fund (Harkness Fund) Fellowship to work with Professor H.H. Mitchell at the University of Illinois. Mitchell was a remarkable scientist and a kindly man. We spent long times talking as well as working together. Besides his studies on the biological value of proteins, Mitchell had been concerned in the 1930s with studies on the energy metabolism of cattle. I developed a considerable interest in this aspect of nutrition and carried out some calorimetric work at Urbana with sheep. I was also taught by Professor Mitchell's technician Haines to analyse air by the classical Carpenter–Haldane methods so as to attain an accuracy of better than 1 in 10,000. This is not easy; one cannot rely on one's results until the apparatus has come into a temperature equilibrium with respect to the heat one radiates and conducts to the glassware during manipulation.

At a meeting of the American Society for Animal Production in Chicago in 1946 I met Norman Wright, Director of the Hannah Dairy Research Institute, who was touring the USA. Together we also toured bars in that city and at some point he suggested that I might apply for the headship of the Nutrition Department of his Institute. This I did. I was appointed without interview but told that I could not receive the offered salary because I was not old enough! I was annoyed at this age barrier and said so. Eventually, and exhibiting the British genius for compromise, the ARC agreed that I should receive a Senior Award at a salary between the two scales.

I spent 18 happy and productive years at the Hannah with colleagues who were also friends. David Armstrong, present President of the Nutrition Society, F. Brown FRS, J.A.F. Rook, who became Director of the Hannah and later second Secretary of the AFRC, N. McC. Graham, A.J.F. Webster, J.W. Czerkowski and F.W. Wainman all worked with me and together with overseas visitors we uncovered much. We discovered the toxicity of the highly unsaturated fatty acids of cod liver oil and its reversal by massive doses of alpha tocopherol, and, with G.A.M. Sharman, the enzootic muscular dystrophy of

cattle in NE Scotland. This could also be prevented by vitamin E but reflects a primary selenium deficiency. We showed later in trials with 5,000 sheep that selenium deficiency is particularly associated with the old red sandstone deposits in Scotland. As part of a programme designed to provide clinical, pathological and biochemical methods for the diagnoses of nutritional disease in calves we provided suitable evidence for deficiencies of magnesium, thiamin, nicotinic acid, iron, protein and energy, together with information on vitamin D toxicity.

This work on nutritional disease was phased out when my interests returned to those which had been aroused when I worked with Mitchell. In the 1950s we built respiration apparatus, based on the closed circuit principle, mostly for animals weighing about 50–60 kg – sheep and calves – but also a massive instrument for 500 kg cattle. With these a systematic attack on problems related to the use of feed by ruminants was undertaken. We examined the cause of the heat increments of the lower steam volatile fatty acids, glucose, lactic acid, protein and long chain fatty acids to conclude that for maintenance their replacement rates were in proportion to the free energy as ATP generated when they were oxidized. Studies with natural feeds followed and together they led to the conclusion that the then current methods of apportioning food to ruminants were not correct. On the basis of these results I devised the metabolizable energy system which is now the basis of most feeding systems throughout the world.

Other work related to the suppression of methanogenesis and on the effects of cold environments on the energy requirements of sheep and cattle. The latter involved studies of changes in infra-red radiation, solar radiation, air movement and rain as well as air temperature alone and led to definition of the thermal characteristics of hair coats and methods for estimating the critical temperatures and feed needs of stock in cold environments. One of the more interesting pieces of work arose when Lord Rothschild, then Chairman of the ARC, asked whether I could collaborate with Professor R.A. Fisher FRS to explore the latter's contention that genetic improvement of cattle would not necessarily result in increased economy of feed use. He envisaged ranking cows in terms of their intrinsic efficiency. Not much came of this but led to my exploring the responses to small changes in feed intake in cattle. The experimental work showed that milk secretion had a strong 'historical' component and H. Ruben and I devised a mathematical model of lactation that incorporated this idea and also those stemming from the calorimetric studies relating to feed utilization.

Interest in providing simple models to facilitate prediction of the responses of animals to nutritional inputs prompted a study of the passage of feed through the gut. Two simple differential equations when solved predicted flow quite well, defined the period that should elapse between change of diet and collection of faeces appertaining to that diet and provided an explanation for certain aspects of appetite regulation in ruminants.

In 1965, on the retirement of Sir David Cuthbertson from the Directorship of the Rowett Research Institute, I was appointed to succeed him. I asked one question at my interview, 'What is my responsibility for human nutrition?' A 30 minute discussion then ensued between the Department of Agriculture and Fisheries for Scotland, the Governing Body of the Institute and the ARC representatives, in which I was not involved. At its end I was restricted even more than Sir David had been by being told, 'None – except in respect of the nutritive value for man of agricultural products'. I regretted asking that question; in the early 1970s it stopped me from using Rowett expertise to undertake much needed calorimetric work with man!

Lord Boyd Orr, the Institute's first Director, said to me after my appointment, 'You will not now do research yourself; you have others to look after'. I did not believe this and Wainman (who accompanied me from the Hannah) and I built new respiration chambers and undertook calorimetric work related to extension of our previous work on feed evaluation. My part in this was subject to considerable interruption and I soon realized that any investigational work I undertook would have to be such that it did not demand constant attention to the detriment of my responsibilities to others. I thus embarked on long-term studies which could be taken up and put down. These related to, among other things, the use of fossil fuels in agriculture, information transfer, farming the red deer, growth and obesity in sheep and the statistical analysis of calorimetric data. These were coupled with short term calorimetric studies undertaken in the summer holiday period. The latter dealt with effects of barometric pressure, acclimation, season and habituation on the energy exchanges of animals.

During my directorship we were able to rebuild and re-equip the Institute. This involved a continuous battle for funds using both legitimate and somewhat dubious approaches. Mention to civil servants of the possibility of a 'Question in the House' about the appalling treatment of Scotland *vis-à-vis* England is an example of the latter. New structures were evolved to facilitate experimentation through automation, computerization and the servicing of animal facilities. Initially there was freedom for staff to pursue their ideas and facilities to do so; later we were circumscribed by the encroachment of bureaucratic controls on what we did.

Inevitably I became involved in national and international activities outside the Institute. Some of these were concerned with provision of advice to governmental bodies on aspects of policy – examples being membership of the committee of ACARD concerned with research for the food industry, the Scottish Agricultural Development Council and policy committees of the ARC. Some however were more directly concerned with the nutritional sciences. For several years I was Chairman of the British National Committee of IUNS. An accomplishment of that committee was to recommend the replacement of the calorie by the joule, a recommendation now accepted throughout the scientific world.

Most interesting was my membership of the Joint ARC–MRC Committee on Food and Nutrition Research. After four year's demanding work we produced in 1974 our report, *The Neuberger Report*. This was a summary of the existing state of knowledge about nutrition together with recommendations to the two Councils on what research should be undertaken. Understandably, since the recommendations were likely to be implemented and funds made available, the report was criticized by various groups. Some thought that insufficient attention had been given to the social and economic aspects of human nutrition, others thought that the review of knowledge was insufficiently justified by reference to the literature and still others thought that the report overemphasized human at the expense of animal nutrition.

Publication of *The Neuberger Report* immediately preceded my term of office as President of the Nutrition Society. I had joined the Society on my return from the army, had been on its Council and on the Scottish Group committee, and my election to the Presidency was a great honour. One of the first steps of the Council under my chairmanship was to institute a discussion of *The Neuberger Report* at which Professor Neuberger agreed to answer questions. This meeting in May 1975 was a stormy one; it was perhaps the first occasion on which the Society had had the opportunity to debate issues of national policy. In the end it was agreed that the report was a good one but that there were differences of view about its balance. The controversy continued, as exemplified by views expressed at the Society's symposium on 'Strategy for Nutritional Research' held in 1978.

Otherwise my presidency was a period of calm. It was a time of monetary inflation and subscriptions had to be increased by a large percentage each year. Indeed, such was the concern about national finances that we changed the status of the Society to that of a Company of Limited Liability registered under the Charities Act. We could then use direct debit methods to collect subscriptions without the additional cost of writing to everyone. The major advantage of the change, however, was to protect the Officers' personal finances in the event of the Society becoming bankrupt! The period included continuation of the perennial debates about the large proportion of papers on ruminants relative to those on man in the journal – a matter that perhaps reflected the funding provided by the ARC relative to that of the MRC at that time. It was hoped that, following the *Neuberger Report*, these might be resolved.

Having accumulated funds at the Rowett and secured the support of the University of Glasgow, I was able to ask Council to add to the sum and take on the administrative responsibility for the Boyd Orr Trust, created in honour of the Society's first President, the Rowett's first Director and a distinguished Chancellor of Glasgow. Most appropriately the inaugural Boyd Orr lecture was given by a successor of his as Director-General of FAO, Dr A.H. Boerma. The other major concern of Council was the Irish question. A movement

commenced to establish an Irish Group, initially including Northern Ireland and Eire but later limited to Eire. The number of Society members in Eire did not approach the 75 required under our By-law 7a and proposals for election to membership were made for many whose nutritional expertise was questionable. Professor R.A. McCance objected to some of these proposals at an AGM of the Society and the attempt to establish an Irish group was abandoned.

I retired from the Directorship of the Rowett in September 1982, leaving Scotland after 35 years to return to East Anglia. Retirement has involved a Visiting Professorship at the University of Newcastle, Chairmanship of the Individual Merit Promotion Panel of Cabinet Office and Treasury, Presidency of the Institute of Biology and continuance of my membership of the Nutrition Committee of the Rank Prize Funds, together with an abiding interest in the Society and its affairs.

Looking back over 50 years of the Society, I mostly remember the great occasions – Lester Smith's announcement of the nature of the extrinsic factor and Sir Joseph Barcroft's practical demonstration of the quantitative importance of the steam volatile fatty acids when he placed a jar of them, isolated from the rumen, before us. Much has been accomplished in terms of the advancement of our subject and in this the Society has reason to be proud.

Typescript, 1989

Dr Elsie M. Widdowson

Head of MRC Infant
Nutrition Research
Division, Cambridge
President 1977–1980



My schooldays were spent in South East London. I lived in Dulwich with my parents and sister and cycled each day to school in Sydenham. Zoology was my favourite subject in the sixth form, and I had the idea of taking it for a degree. However, we had a very good chemistry mistress, and she persuaded me to take chemistry instead. The tradition at the school was for the girls to go to one of the London colleges for women, usually Bedford College. I decided to do something different and I went to Imperial College, which was a man's world with three women in our year of about 100. I took the BSc examination after two years, but had to spend another year at the College before the degree was awarded. I spent this time in the small Biochemistry laboratory presided over by Professor S.J. Schryver, Sammy Schryver as he was generally known.

Everybody in the laboratory was separating amino acids from various plant and animal materials. This was long before the days of chromatography, and we all worked on a vast scale, starting with bucketsful of material rather than beakers. We extracted the proteins from our various materials and converted them to amino acids, which we then precipitated as their copper salts. We made use of the fact that the copper salts have different solubilities in various solvents to separate them. Great was the excitement when, in 1928, one of the people in the laboratory, Bernard Town, discovered a hitherto unknown amino acid, which we now know as proline. I made no such discoveries.

Towards the end of the year an emissary from the department of Plant Physiology, across the quadrangle from the Biochemistry laboratory, came over to find me. Rumour had reached them that I might be looking for a job. A grant was available in the department, and if I was interested would I go for an interview. I was interested, I went, and the result was that I worked there for over three years with Helen Archbold (later Helen Porter FRS) who was in charge of a long series of experiments for the Department of Scientific and Industrial Research on the chemistry and physiology of apples. My part in the investigations was to separate and measure the changes in the individual carbohydrates in the fruit from the time it first set on the tree until it ripened, and then during storage. It was my responsibility to go by train every two weeks to Swanley in Kent, then walk about a mile to an orchard to pick fruit from specified Worcester Pearman and Bramley's Seedling apple trees and bring them back to the laboratory for the various studies that were being made. I thoroughly enjoyed those outings, especially on lovely summer days. I developed a method for separating and measuring the starch, hemicelluloses, sucrose, fructose and glucose in the fruit, and the first paper I ever published on the determination of reducing sugars in the apple appeared in the *Biochemical Journal* in 1931. I little realized how momentous this was going to be for the whole of my scientific life.

All this time I worked under the guidance of Helen Archbold, who was always available to help and advise me, and she initiated me into the art of writing up the results for publication. I owe a great deal to her, and she undoubtedly gave me my life-long love of research. I was able to use my work for a PhD.

At the end of the three and a half years the grant ran out and in any case, much as I had enjoyed my time with the apples, I did not want to devote my life to plants. I was really more interested in animals and man. So in 1932 I went to the Courtauld Institute at the Middlesex Hospital for a year or so, under Professor E.C. Dodds, to get some experience in human biochemistry. One paper came out of that period, a comparative investigation of urine and serum proteins in nephritis. I was quite startled, but gratified, to see fairly recently this little effort of mine referred to as 'the pioneer work on the subject'.

In 1933 I was faced with finding a job, and research jobs were difficult to

come by at that time. I went for several interviews but nobody wanted me. Professor Dodds told me that dietetics was an up-and-coming profession, and on his advice I enrolled for the first one-year post-graduate diploma course in dietetics at King's College of Household and Social Science under Professor V.H. Mottram. As a preliminary to this course I was sent to work in the main kitchen at King's College Hospital to learn something about large-scale cooking. While I was there I often saw Dr McCance come into the kitchen and bring joints of meat to be cooked. I was told that he was doing research on cooking. Naturally I was interested, and one day I plucked up courage and spoke to him. He invited me to visit his tiny laboratory, where he told me about the work he was doing on the composition of meat and fish and their losses on cooking, and about his previous study on the available carbohydrate of foods. This had been published in 1929 as an MRC Green Report. It contained information about the reducing sugars present after acid hydrolysis in fruits, vegetables and nuts. I at once realized, from my experience with apples, that the figures for carbohydrate in fruits were too low, for some of the fructose must have been destroyed during the acid hydrolysis. I told Dr McCance this, and the outcome was that he invited me to join him, and he got me a grant from the Medical Research Council – it was easier to do this in those days than it is now – and we started another study on the composition of fruits, vegetables and nuts which included water, nitrogen, fat and inorganic constituents as well as carbohydrate. Where appropriate the foods were analysed cooked and raw.

I finished the Dietetics Diploma course, and that served me well in two ways. In the first place it aroused my interest in nutrition. Secondly, as part of the course, I spent six weeks in the diet kitchen at Barts with Margery Abrahams. I should really have spent six months there, but those six weeks were long enough to convince me that we badly needed comprehensive tables showing the composition of British foods. The composition of patients' diets was being calculated from American tables which gave values only for raw foods, and in which the carbohydrate had not been determined directly but calculated 'by difference', that is what was left after deducting water, protein and fat from the total weight. It thus contained everything that we now know as 'dietary fibre'. Dr McCance knew all about the difficulties in using 'carbohydrate by difference' in prescribing diets for diabetics, and in fact at that momentous first interview with him he told me about his work on unavailable carbohydrate, the cellulose, hemicelluloses, pentosans, pectins etc. which he had separated from the available carbohydrate, the sugars and starch in his original studies.

I thought a lot about the need for British food tables and one Saturday afternoon in 1934, while I was on a family outing to Box Hill, a beauty spot in Surrey, the idea came to me that meat, fish, fruit and vegetables would soon have been completely analysed, so there only remained cereal foods, dairy products and some miscellaneous items such as beverages and sweets. If these

were also analysed we should have all the material available for making a practical set of tables showing the composition of British foods. I put the idea to Dr McCance the following Monday morning. He was willing and this is how *The Chemical Composition of Foods* came to be conceived and born. The first edition was published in 1940. All the values were checked and rechecked many times over. There were about 15,000 separate values in the tables and it was almost impossible not to let a mistake slip in here and there. For example, the decimal point slipped in the figure for nitrogen in blackcurrants, so that it was ten times too high. We never heard the end of that. I sometimes think that of all the various aspects of nutrition I have dabbled in over the past 58 years my first venture, on the composition of foods, will be the longest lasting.

Because we had all this information about food composition we were in a strong position to calculate the intakes of energy and nutrients by men, women and children. Up to the 1930s almost all dietary surveys had been made on families. The family was assigned a man-value, based on the supposed energy needs of each individual within it. The intake of the family was divided by the man-value, and this was then compared with the existing tables of requirements. This was obviously unsatisfactory, and in fact Professor Cathcart had written a review setting out some of the fallacies of this method of approach in the first number of *Nutrition Abstracts and Reviews*, published in 1931. We clearly needed information about the intakes of individuals, and I started my individual dietary surveys, first on 63 men and 63 women, and I followed this up with the measurement of individual dietary intakes over a period of a week, of over 1,000 children between one and 18 years. These surveys brought out very clearly the large variation in the intake of energy and nutrients between one individual and another of the same sex and age.

In 1938, during the Munich crises, we moved to Cambridge to the Department of Medicine. The first year was spent finishing the food tables and writing up the study on individual children's diets. Then the war started. We all felt we must do something to further the war effort. Professor McCance has described our experimental study of rationing and our balance experiments with various sorts of bread (p. 113). This occupied us fully during the first years of the war. I next turned my attention to another use of the analytical experience we had gained during our years of food analysis, the composition of the human body. This was a more difficult problem than food analysis. I will not go into the difficulties we had in obtaining bodies, or in dealing with them once we had obtained them. We managed to overcome the difficulties and we started the work. Then the war ended and we changed course completely; for in the spring of 1946 we went to Germany to study the effects of undernutrition on men, women and children. Professor McCance has described some of our experiences during the first part of our time there (p. 115). We intended to stay in Germany for six months, but in the end some of stayed for nearly three years. This came about because in December 1946, while I was home on leave,

Sir Edward Mellanby called a meeting to discuss the post-war loaf. Up to that time there was no question in the minds of nutritionists that high-extraction flour was more nutritious than white, but there was a question as to whether white flour could be made as nutritious as wholemeal by adding to it the B vitamins and iron. At the meeting Sir Edward Mellanby said to me, 'There must be a lot of hungry children in Germany. You go and find out the truth about all this'. I returned to Germany at the beginning of January and Rex Dean and I drove about in deep snow looking for a suitable orphanage where we could feed children on different kinds of bread. We found one in Duisburg, about 30 miles from Wuppertal where we had our headquarters. The children, aged between five and 14, were underheight and underweight at the outset. They gained height and weight equally rapidly on bread made from all five types of flour, 100% (wholemeal), 85% and 72% extraction, and white (72% extraction) enriched with B vitamins and iron to the amounts in 100% and 85% extraction flour. All the flours contained added calcium carbonate. Bread provided 75% of the energy and the diets contained only 8 g of protein from animal sources a day. The experiment lasted for 18 months; the children improved physically, and it was impossible for the outsider to tell which kind of bread the child was eating. During the latter part of the experiment the BMA held its annual conference in Cambridge and I brought five of the girls, one from each group, from Wuppertal to Cambridge, so that the audience could see for themselves the results I was describing. The girls thought this was a tremendous adventure.

We left Germany in January 1949, and I returned to the work I had just begun four years earlier on the composition of the body. I approached this in two ways. The first was to study the effect of growth and development on body composition, and we analysed the bodies of 19 human fetuses and still-born babies, of one four year old boy, and of three men and one woman. The adult bodies were dissected by Dr Barrett, the hospital pathologist, which was a great help. We measured the amounts of the same constituents as we had done in the foods.

I have always had and still have a great interest in the similarities and differences between species, and the second approach to body composition, which was really an extension of the first, was to study changes in composition during development of other species. We included pigs, cats, guinea pigs, rabbits, rats and mice in the investigations. As a result of all this work we were able to establish some general principles, and also some important species differences, which were linked with the state of maturity the young of each species reaches when it is born.

Running along with this analytical work were our studies on severely undernourished pigs. This arose out of our work in Germany, and was possible because Professor McCance had facilities at his home in Bartlow for keeping pigs, and these pigs became an important part of our lives for about 15 years.

We found we could undernourish them so severely from ten days of age that by one year they weighed only 3% as much as their well-nourished littermates. This is, I believe, the most severe growth retardation that has ever been produced and was only possible because the difference between the weight of a newborn pig and an adult is so large. We made many studies on these animals, anatomical, physiological, chemical and psychological. In 1966, Professor McCance went to Uganda for two years and I became more adventurous. I continued the undernutrition for two and three years before I rehabilitated the pigs. They had to be allowed to gain weight very slowly all the time, for if they lost weight they died. When the animals were rehabilitated after one, two or three years they ate a great deal of food and gained weight rapidly for a time, but the longer they had been undernourished the sooner they stopped growing, and the final weight of those rehabilitated after three years, the age at which the normal pig stops growing, was only half that of pigs well-nourished throughout. In spite of this they matured sexually, and when rehabilitated males and females were mated, the females produced good litters with normal-sized piglets which they suckled satisfactorily, and these piglets bore no mark of the nutritional adventures their parents had undergone.

Another investigation we were busy with in the late 1950s and early 1960s was made on rats suckled in large and small groups. Those suckled in groups of three were two or three times as heavy at weaning at three weeks as those suckled in groups of 15 or so. From weaning they all had unlimited food, but those that were small at weaning remained smaller all their lives. We used this technique to study the effect of early nutrition on many aspects of growth and development, and this little trick of rearing rats in large and small litters has since been used for various purposes by investigators all over the world.

Rex Dean, who had been with us in Germany for the three years we were there, and was my partner and supporter in the experiments with bread, was asked by Sir Harold Himsworth, then Secretary of the Medical Research Council, to go to Uganda to look into the malnutrition among young children that had been reported there. This was in the early 1950s, and the Infantile Malnutrition Research Unit was subsequently set up in Kampala. Rex Dean was in charge of it until he died in 1965. He had often invited me to visit him and I eventually went in 1964, but by this time he was not at all well, and I wished I had gone before. Roger Whitehead, who looked after the biochemical work, was really running the unit. He took me on a wonderful trip to the Queen Elizabeth Game Park. I went to Uganda twice more, once shortly after Dean died, and Roger was Acting Director, and again during the period when Professor McCance was there and Roger was working with me in Cambridge. Those three visits to Uganda made a lasting impression on me, not only the colour and beauty of the African scene, but the other side of the picture, the severe malnutrition among the young children, and the way they recovered in response to treatment with the right kind of food.

After Professor McCance retired and went to look after the Unit in Uganda I moved to the Dunn Nutritional Laboratory, as Head of the Infant Nutrition Research Division. Then in 1973 I retired for the first time. I moved to the Department of Investigative Medicine at Addenbrooke's Hospital. This was the successor to Professor McCance's Department of Experimental Medicine, for the hospital had always disliked the word 'experimental'. The authorities there had never heard of Claude Bernard! For a time I had laboratory accommodation and some PhD students, and even when laboratory space was no longer available I still had an office which Professor Ivor Mills allowed me to keep until he retired in 1988. I then retired for the second time.

In 1986 I went to Washington, DC for a few weeks to work in the Nutrition laboratory at the zoo, with a grant from the Smithsonian Institution. It came about in this way. Some time in the 1970s I had been invited to Cornell to lecture. My plane to New York was delayed, I missed the connection to Ithaca, and I finally arrived there at midnight one snowy winter's night. I was afraid there would be no one to meet me, and I did not know where I was staying. But there stood a solitary figure, a young man, who greeted me by telling me I was the only person in the world who would appreciate what he was doing for his PhD. This was Olav Oftedal. He was collecting together information about the composition of the milk of all the species whose milk had been analysed, and he was filling in the gaps by analysis where possible. We have remained friends ever since. Olav later became nutritionist at Washington Zoo. In 1984 he and two colleagues mounted an expedition to the pack ice off Labrador to measure the milk intake and milk composition of two species of seals born and suckled on the pack ice. One of the species, the hooded seal, doubles its birth weight of 20 kg in four days on a milk containing 60% fat, of which it takes 10 kg a day. The mother then leaves it and goes back to the sea. Olav had brought to Washington 20 frozen bodies of newborn and suckled seals, killed according to the Canadian Sealing Regulations. These bodies remained in the cold store at the zoo for two years, along with the bodies of newborn and suckled black bears, born and suckled while their mothers were 'hibernating' and taking no food or drink for several months. I visited Olav several times during those two years. Each time he took me to the cold store to view the frozen bodies, and I realized that nothing was going to be done with them unless I lent a hand. So I offered to go and help. I got a grant from the Smithsonian Institution, the laboratory was cleared of all other work, and we had a hectic few weeks dissecting the animals, weighing and measuring the various parts of the body and preparing the material for analysis. This was rather a complicated job, wrapping up and labelling the various parts of the body from so many animals. I thoroughly enjoyed being associated with it all, and getting my hands, or rather rubber gloves, dirty again. Moreover, it gave me many new problems to think about in comparative nutrition.

My connections with the Nutrition Society go back to its beginnings. I was

a founder member, and was present at the first meeting, held in Cambridge in 1941. My first term on the Council was in 1953 when Dr Harris was President and Geoffrey Bourne the Secretary. Council meetings were then held at the London Hospital.

The main outside activity with which I was concerned in the late 1970s, while I was President, was the setting up of FENS (Federation of European Nutrition Societies). After a successful European Nutrition Conference in Cambridge, which Dr Kodicek organized in 1973, Professor Zöllner arranged an equally successful one in Munich in 1976. On that occasion he and I proposed that European Nutrition Conferences should be put on a more formal basis, and held regularly. This proposal was passed enthusiastically. During the next three years Professor Zöllner and I, with a small European committee, drew up the Constitution, and at the Conference in Uppsala in 1979 the Federation of European Nutrition Societies came into being.

When I moved out of Addenbrooke's Hospital in 1988 I transferred my books and my work to my home. I live in a thatched cottage in the middle of an apple orchard, and I enjoy the thought that I have come full circle, from apples to apples, but I now deal with them in a more practical way.

Typescript, 1990

Professor I. Macdonald

Professor of
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Medical School,
Guy's Hospital,
London
President 1980–1983



I was born in 1921 and educated at a grammar school in North West England. Due to my father's work in the Colonial Service taking him overseas I was brought up by a very strict Scottish grandmother, where the Sabbath was reserved for the Kirk (twice) and learning the catechism.

However, in 1940 I moved to the other end of the moral spectrum and

became a medical student in a London teaching hospital, and qualified four years later. These four years were not without incident. I was cycling through Kent on the day of the Battle of Britain and witnessed several 'dog fights', and later was a clinical student when the 'flying bombs' and rockets fell on London, and our duties were mainly confined to servicing and assisting in the operating theatres at night when most of the flying bombs penetrated the defences.

After a year doing house jobs I joined the RAMC and spent the next two years in the Middle East as a Medical Officer on a troop ship or in a transit camp, or serving the large camps in Cyprus set up for illegal Jewish immigrants.

On my return I was appointed as one of the resident Obstetricians at Guy's, whose duties at that time included attending deliveries at the homes of the people who lived around the hospital. Transport was by bike painted with yellow stripes – it is of interest to note that these bikes were never stolen. I then joined the Physiology Department at Guy's Hospital Medical School in order to survive while studying for the Primary FRCS, with a view to continuing in obstetrics and gynaecology. However, having succeeded in passing the examination, and having experienced some research while in the department, the thought of spending the next 40 years or so in clinical medicine did not seem nearly as exciting as doing research, needless to say, in obstetrics, which is, after all, physiological. Having learned about gastric activity while working in the department with J.N. Hunt, I felt confident to branch out on my own and decided to try and assess the growth and hardness of the human fetal head with reference to how it may hinder labour in post-maturity. This resulted in a PhD, and to me, just as important because it might help a patient somewhere, my findings were included in a popular medical student text book on obstetrics.

Still with an obstetric interest and with some experience in gastric research, I postulated that 'morning sickness' was due to the hormone chorionic gonadotrophin, produced by the fertilized ovum and whose levels were highest in the first three months of pregnancy when sickness was most common. Knowing that female rabbits after sterile mating undergo, for two weeks, hormonal changes associated with early pregnancy but without chorionic gonadotrophin because there is no fertilized ovum to produce this hormone, it seemed a good idea to study gastric activity, using a stomach tube, and to learn how it functioned when the doe was pregnant and pseudo-pregnant. There was, however, one minor problem to overcome, and it was the attempt to solve this problem that brought me into nutrition. The problem was that the stomach of the rabbit is never empty but full of a green purée, so I reasoned if the animal was given a water soluble diet then all that would be necessary would be a simple gastric wash-out before giving the test meal. However, this was not to be as the diet of calcium caseinate, sucrose with salts and minerals, resulted, after three months or so, in the death of the animals with post-mortem findings of fatty liver, cirrhosis and even ascites. To me this looked as if it might be a good model for kwashiorkor, and when I made my first presentation to

the Nutrition Society in 1956 Dr Cicely Williams congratulated me and encouraged me to continue in this area. The interest shown in my work by such an internationally respected person in nutrition had a strong influence on me to switch from obstetric research to nutrition research.

I was invited by Dr Joe Gilman, then Professor of Physiology in the University of the Witwatersrand, to spend six months teaching in his department while I collected and analysed post mortem livers from cases of kwashiorkor. This was in 1958, and the work resulted in an MD. The following year I received a bursary to continue these studies in Dakar, French West Africa, under Professor Senecal, who was at that time called 'le petit Schweitzer'. Two years later I spent three months in the Department of Medicine at Cape Town with Dr Bronte-Stewart and under another international name in kwashiorkor, Professor J. Brock. My conclusion at that time was that the fat in the liver and in the depots in kwashiorkor were very similar in composition.

About this time there was considerable discussion on the role of diet, especially saturated fats, as aetiological factors in coronary heart disease, and knowing that dietary carbohydrate could alter intermediary lipid metabolism, I gave human volunteers, including myself, diets which were low in fat but high in carbohydrate, as either raw starch or sucrose, for periods of three weeks, while monitoring the fasting plasma triglyceride concentration and the composition of the subcutaneous depot fat. Differences between the effects of these two carbohydrates became apparent and these became even more exciting when I found that pre-menopausal women – who have a low incidence of coronary heart disease – did not differentiate between sugar and starch as far as plasma triglyceride levels were concerned, whereas men and post-menopausal women – the 'at risk' groups, did. Subsequently it has become apparent that plasma triglyceride levels are not good prognosticators of coronary heart disease, and also it seems that after several weeks on a high carbohydrate diet, including sugar, the levels of fasting plasma triglyceride gradually return to control levels, so that period of excitement gradually petered out.

For the work I had done, mainly on dietary carbohydrate and lipid metabolism, I was awarded a DSc by the University of London in 1966, and I learned unofficially, so I do not know whether it is true, that Dr Hugh Sinclair was my assessor on this occasion.

My interest and research in dietary carbohydrates in man continues to this day, though the relationships between various dietary carbohydrates and lipid metabolism is not a prime concern of mine now. In passing it is amusing to read recent research papers in which it has been found that sucrose and glucose have differing effects on lipid metabolism with no reference to papers published over 20 years ago!

When the opportunity to be in charge of a large teaching department arises a difficult decision has to be made because acceptance means, in effect, much less time at the bench. I think I made the correct decision, as well as taking on

extra-mural activities such as serving on the Food Additives and Contaminants Committee, later to become the Food Advisory Committee of MAFF, being a Member of Council, and later chairman of the British Nutrition Foundation, serving as Secretary to the Federation of European Nutrition Societies and Chairman of a joint WHO/FAO Expert Committee on Dietary Carbohydrates – all these combined to reduce to nought the time I could spend at the bench. Also as a physiologist I was an examiner at various medical schools in the Commonwealth, including Malaysia, Hong Kong, Kenya and Nigeria, and as a nutrition person examined dietetic students in the Universities of Surrey and London, and the Polytechnics of Leeds, Aberdeen and North London. So any hypotheses I might postulate would have to be tested by PhD students and research assistants. This I regret, as for me getting my hands dirty and getting results I find very exciting. Now I sublimate that pleasure by making bread, cooking etc., at the weekends, so the kitchen has, for me, replaced the laboratory bench – with, I might add, not nearly the same success.

One of the rewards of working in a teaching department in a University is teaching the eager, vital, bright students that come fresh and starry-eyed every year with a keenness for eventually helping those who can benefit from their knowledge and skills. The yearly intake of these top drawer young men and women plays an important part in delaying the ageing intellectual processes of their teachers!

One of the more sensible decisions I made in my academic career was to accept the invitation to become Programmes Secretary of the Nutrition Society. This was, and still is, a fairly onerous post and it meant attending all the meetings the whole time, whether they be on man or large animals. The undoubted reward was meeting all those in the UK who were active in nutrition research – and maybe they got to know me! However, in my view the greatest accolade of an academic career is to be appointed as President of your peer group and nothing has given me more pride than the three years I served as President of the Nutrition Society. What, if anything, did I consider I achieved during this time? My main concern at that time was that the clinicians, who had all of a sudden began to realize that nutrition could be a valuable therapeutic tool, might form a group of their own, and to try and prevent this I persuaded the Nutrition Society to hold joint meetings with clinicians and two such were held, one in Sheffield and one in Salford. I was, therefore, pleased to learn that the Society has recently embraced a clinical grouping. Nutrition, which is not a science in its own right, tends to have a poor image in the lay mind because of contradictory pronouncements by media persons. If the scientific side of human nutrition were to be split, then greater knowledge, which often comes through cross-discipline co-operation, would be retarded.

All research needs its share of luck. I consider I have had more than my fair share.

Recording and typescript, 1989

Professor J.C. Waterlow

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President 1983–1986



My father was a civil servant and also a literary critic and a minor member of the Bloomsbury Group. I remember E.M. Forster and Desmond McCarthy visiting us often. I must have seen Virginia Woolf although I don't remember it, because she records in her diary coming to our home and then writing that she met there two of the most disagreeable children in England – my sister and me.

In 1926 when I was ten, father, who had been working at the Foreign Office, was transferred to the Diplomatic Service owing to a disagreement with

the Secretary of State and he was sent to Bangkok. We children weren't allowed to go to such places because of malaria and other diseases and so I was sent to boarding school. I then had the good fortune to get a scholarship to Eton and, although it's rather unfashionable to say so, I really enjoyed my time at Eton. I think that possibly I was more successful there than at any other period of my career. The work was mainly in classics, which I enjoyed. I boxed for the school, was in the Second Eight and was Captain of that solely Eton game, the Wall Game.

What was good about Eton was that one had plenty of spare time to do all the things that one liked, and being in College, that is to say, a scholar, there were many people of my kind and I made many friends. I ended up as Captain of the school; I got the main scholarship in Classics and Divinity – you had to be able to read the Old Testament in Latin and the New Testament in Greek – and I got a scholarship in Classics to Trinity College, Cambridge.

However, I never felt inside myself that I was a classical scholar; when I began to think what I was going to do in life I really didn't know. It seemed the course I was on led to the church or the civil service or perhaps to being a don; it never entered my mind, or my father's, that one might go into the City as bright young people do nowadays. In my upbringing there was no science, except that my father had a great friend, George Barger, a colleague of Henry Dale and Charles Harrington, and we used to go and stay with the Bargers in Scotland. He was Professor of Physiological Chemistry, first in Edinburgh and then in Glasgow; we used to go for long walks and he aroused in me an interest in biology. The other most important turning point for me was a lecture given at our school by a man called Tubby Clayton, a clergyman who founded an organization called Toc H after World War I. Tubby Clayton came and gave a talk about work on leprosy in West Africa and I thought this was extraordinarily interesting and that's what I'd do, much to the dismay of my tutors, who were thinking of a career for me in politics or academic life.

I left Eton in 1934 and spent a year in Greece where my father was then Minister. I walked over a large part of the country, which in those days was very wild, with practically no roads and many bandits about. In 1935 I went up to Trinity. In my first year I had to do the 1st MB, because I had done practically no science at school. I've felt all my life that I have a very poor scientific background and I bitterly regret also not having done more mathematics.

After that first year I took the Natural Sciences Tripos. In those days medical students seemed to me to be regarded as the lowest of the low and most of the dons paid little or no attention to us. There were only two exceptions and both oddly enough were American; one was Glen Milliken, the son of the physicist, who was killed climbing in the Rockies some years later; the other was Don Barron, who later became Professor of Physiology at Yale. These two people influenced me a lot. They were both students of Sir Joseph Barcroft, Professor at that time, who was the most marvellous and superb lecturer; nobody could

be uninterested in Physiology who listened to him.

I then went on to do a Part II in Physiology. This was a good small class, containing Andrew Huxley and John Gray among others, and I just managed to scrape a First. I didn't have the same feeling about Lord Adrian as I had about Barcroft; when I had got my degree I went to see Adrian to ask his advice on whether I should go on to medicine or remain and try to get a Fellowship and be a physiologist. I came away without having got any interest on his part at all.

My other main activity at Cambridge was in politics. That was the time of the Spanish Civil War and the slump and the hunger marchers and it was very difficult not to be interested in politics. A great friend and stimulus to me was John Cornford, who joined the International Brigade and was killed at the Battle of the Ebro. I became a member of the Communist party until I resigned at the time of the Molotov/Ribbentrop Pact in 1938. As such, I got to know people a little who later were very notorious, like Guy Burgess, Anthony Blunt, Michael Straight, Leo Long and others. Of course, we had absolutely no idea of what their activities and motives were at that time, but later, after the Burgess/Maclean debacle I was interviewed by the Intelligence Services, actually by Peter Wright, who has recently become so well-known. They told me after a bit that they were not interested in me, since my career suggested that I was not likely to be a traitor. I must say that I have been very fairly treated; I've never had cause to suffer from my Communist past.

I was also during that period elected into the society called the Cambridge Apostles, which used to be secret but is now rather well-known. About this I just want to say that the idea has been bruited about that the Apostles were all either traitors or homosexuals, or both, but it is completely wrong. Four of my closest friends were killed in the war, as subalterns in the army or pilots in the RAF, and two of them were Apostles. Apostles were really just like everyone else in their range of characters and interests.

I went down from Cambridge and started my clinical work at the London Hospital in Whitechapel in September 1939, just on the outbreak of war. We students were therefore scattered about among different hospitals in north and east London: at the North Middlesex in Edmonton, in Mile End for midwifery, in the Poplar Hospital, West India Dock, in casualty work, for example. The result was that my medical training was very sketchy. We did a lot of things in the height of the blitz which no medical student would be expected to do nowadays. I am horrified when I look back and think of giving emergency anaesthetics in the middle of the night, with a bottle of ether and some cottonwool, but nobody ever actually died under our really very incompetent treatment. There were many subjects which we could really hardly learn; very little paediatrics as all the children were evacuated; ears, nose and throat, skins, eyes, all these were done very sketchily. Thus my training in medicine was as sketchy as in science.

I qualified in 1942 and was classified as a physiologist because of my Cambridge degree. I volunteered to join the Parachute Brigade but was turned down because of my eyesight. Then I was sent for a short period to an operational research section of the Armoured Corps at Lulworth, but soon brought back to work on problems of heat in London under E.A. Carmichael and the Military Personnel Research Committee of the MRC.

Early in 1943 I was sent out to Iraq to work on heat stroke in that area. At that time, after the Battle of Alamein, very large numbers of troops were transferred to Iraq and Persia, the so-called Paiforce – Persia/Iraq force – from the Middle East and from India because it was expected that Hitler would break through the Caucasus and come down on the oilfields; that expectation of course was prevented by his defeat at the Battle of Stalingrad. Nevertheless, there were huge numbers of men in that command, on the supply lines to Russia, and I worked there for a year, with the late W.S.S. Ladell on heat stroke and heat exhaustion, which again was very interesting. The place where we were, Shaibe, in the desert near Basra, was just about the most horrible place one could ever expect to be, with temperatures in the summer of 140° in the shade.

After a year I returned to London and came under Professor B.S. Platt, who was about the only person in Britain who had any knowledge or interest in nutrition in the Third World. As the war drew to a close Platt said to me, 'The problem of the future after the war is going to be nutrition'. That rang a very strong bell; it seemed to make good sense and I decided to go into nutrition.

During the war Platt had made a visit to the West Indies to look into the situation there, which was very bad because those islands have always relied on imports for their food, and a great many ships were sunk by submarines. In 1945 I was asked by the Colonial Office to go and look in more detail at why so many children were dying. I visited successively Trinidad, Guyana and Jamaica, working in the paediatric wards and trying to understand what was going wrong with these children. Conditions were, by present standards, very bad indeed, and it wasn't too easy to carry out any systematic studies.

In Guyana I was invited by the Governor, because those countries were still colonies at that time, to go into the interior and report to him on the nutritional status of the aboriginal Indians on the borders of Guyana with Brazil and Venezuela. I spent about three weeks in those forests and mountains travelling on foot and in canoes, looking at the forest Indians wherever we could find them. At the end of this stay the District Commissioner gathered the chiefs together and said, 'We're going to bring you a hospital and a school and agricultural services, etc'. At the end the chiefs just grunted; the District Commissioner said to the interpreter, 'What do they say?' and he replied, 'Chief said we've heard ducks quack before' – which seemed to me a very reasonable assessment.

When I got back and reported to the Governor he immediately offered me the job of Director-General of Aboriginal Medical Services, which I found a very interesting proposition, but couldn't accept it because I didn't think I had enough experience of midwifery and surgery. I have sometimes regretted that I refused.

From Guyana I went to Jamaica and it happened that the Irving Commission, which had been sent out from the UK to plan the setting up of the University of the West Indies and its medical school, was visiting there at that time. I was so impressed with the possibilities of this new University that I decided to go back there whenever I could. In fact it took me five years to fulfil that intention.

In the next few years I was working in Platt's MRC unit, and feeling my way. The only thing I did of any interest during that time was to adapt for studies on human tissues the Cartesian diver, which had been developed by Linderstrom-Lang in Copenhagen during the war. This instrument had about a thousand times the sensitivity of the Warburg for gasometric measurements and my idea was to apply it to biopsies of human tissues in malnutrition, to follow some of the enzyme reactions which I had learned about in Cambridge. Having made some progress in adapting this instrument so that one could do everything in it that could be done in a Warburg, I took my work over to Copenhagen to Linderstrom-Lang. I've never forgotten the extraordinarily kind welcome that this very distinguished scientist gave to a complete nonentity of a young doctor and he agreed to publish my two papers in the *Proceedings of the Carlsberg Laboratory*, which was very unusual for work that had not been done in that laboratory. Except for two papers in *The Lancet* on effects of heat during the war, these were my first publications and therefore very important to me.

During this period Platt was establishing an MRC laboratory at Fajara in the Gambia, with a satellite station at Keneba, where the Dunn group now work, and he asked me to go out there as a junior medical officer to carry on research and to help with the clinical work. The station was at that time run by John Walters, who had been a colonel in the Indian Medical Service and was an absolutely superb organizer and clinical teacher; from him I learned a great deal and I was very glad that I was able to team up with him again many years later when I came back to London to the Hospital for Tropical Diseases.

In the Gambia I worked very happily for a year applying the biopsy method as well as doing a lot of general clinical medicine. However, it wasn't a place where one could bring up a young family and after a year I came back to England. It happened that I was sent as the British representative to a FAO Regional Meeting in Rio de Janeiro, and on the way back I went via Jamaica and was very kindly received by the Principal of the University College, as it was then, Sir Thomas Taylor. He suggested that I should come out to Jamaica to the medical school which was just then starting, as part-time lecturer in

Physiology and part-time MRC staff doing research.

This was a very good arrangement for me; I had earlier applied for the post of Professor of Physiology but had been turned down, quite rightly I think. At the interview with about ten Professors of Physiology around the table, I was asked how I would teach the subject; as I had given no thought at all to this matter, I simply said I thought the teaching was well organized at Cambridge and very successful and I would follow the same model. This of course pleased Adrian but didn't please any of the other people there.

It was just as well that I didn't get that job because it was very good to work part-time for the MRC. I had access to patients in the paediatric ward under Dick Jelliffe and a laboratory of my own in a wooden hut, formerly used by prisoners of war. There I worked as a one man unit for about three years. My main activity at that time was to apply the Cartesian diver technique to studies of metabolism in livers of malnourished children. In my first visit to the West Indies I'd taught myself how to do liver biopsies and I thought they were completely safe, which indeed they were. I thought also that perhaps these children died, as they very often did, because something had gone wrong with the main mechanism of energy transformation, the mitochondria, which were very much in the centre of biochemistry in those days. I worked hard to measure oxidative phosphorylation in the livers of these malnourished children, and showed that it was reduced in the malnourished and fatty liver. Through the help of Professor McCance this paper was published in the *Proceedings of the Royal Society*. I've never yet met anybody who's actually read it but I mention it because for me it was quite important in my scientific development.

During this period I had to make a lot of my own apparatus, such as my micro-balance and Cartesian divers – that I very much enjoyed. I also enjoyed the teaching of physiology; I taught the first class that had entered the medical school and many of them are now quite distinguished people.

That period went on for three years and then my tour was up and I was supposed to go back to B.S. Platt in London. Before doing that, my wife and I went on holiday in Italy and I remember very vividly lying on a bed in a hotel in Urbino, the birthplace of Raphael, and suddenly thinking, 'Why should I not go back to Jamaica? Why should I not ask the MRC to set up a Unit there?' So I wrote there and then and proposed a plan for a unit and submitted it to the Secretary, Sir Harold Himsworth. He welcomed it, and insisted, absolutely rightly, that anyone doing clinical research must have care of his own beds and not be subject to anyone else, and that there should be a ward and laboratories under the same roof. So that unit, The Tropical Metabolism Research Unit, (TMRU), came into being. Seeing how complicated things are nowadays, it seems amazing to me that Himsworth, virtually with a stroke of the pen endorsed, I suppose, by the Council, was able to get this going so quickly.

There were some problems; Jamaica was still a colony and the Treasury in Britain insisted that Jamaica should make some contribution to match that of

Britain. When I went to see the Governor, Sir Hugh Foot, he said, 'My dear fellow, if you think that Jamaica is going to stump up for this it's a non-starter'. However, we managed to devise a formula, according to which, if the hospital provided the normal services for our ward, that's to say nursing, food and drugs and X-rays, which they had no worry about doing, this would be counted as the Government of Jamaica's contribution. So the Treasury at home was satisfied and we got over that hurdle. We were quite successful in recruiting staff from Britain; I'd like to give some names: Roger Smith was one of the first, now a consultant physician in Oxford, Robert Montgomery who is a consultant gastroenterologist in Birmingham, also John Garrow, who needs no introduction. André McLean, Professor of Experimental Pathology at UCH London, was another, Joan Stephen, who remained in the Unit till the end, David Milner, now Professor of Paediatrics in Sheffield, then two West Indian UWI graduates, David Picou, who later had much to do with setting up the new medical school in Trinidad, and George Alleyne, who is now in charge of all the health programmes of Latin America. Others who have made names for themselves in nutrition were Ann Hill, David Halliday, Oliver Brooke, Joe Millward, Roger Neale. So we were very fortunate in getting young people of good calibre who stayed in Jamaica usually for two years and then went back to develop their own careers.

At that time we regarded severe malnutrition as a disease with a high mortality. We were thinking of it as one would think of any disease that one sees in hospital; to reduce the mortality was the main challenge and to do that one had, of course, to understand the disease processes. Many things were investigated during this period; body composition, electrolytes, carbohydrate and fat metabolism; the fatty liver remained of great interest. We had 25% mortality when we started and it was brought down almost to zero in the late seventies.

I was not at all interested at that time in the problems of the community. Our attitude was that it was our job, on behalf of the MRC, to do research which would have general relevance throughout the world and that it was the responsibility of the Government of Jamaica to support research into purely local problems, nutritional surveys and so on. The contribution we did make to that was to train local people: Kenneth Standard, now Sir Kenneth, who was Head of the Department of Preventive Medicine; Helen Fox, who became Chief Government Nutritionist, and others. It was quite a good relationship.

I got the idea towards the end of this time that perhaps children died because they were protein deficient. This was still our basic hypothesis, derived from the work of Cicely Williams, that protein depletion led to a failure of the mechanisms of protein synthesis, which of course are themselves proteins and enzymes. I began to think about how to measure protein synthesis. We were able to get practical support from the Wellcome Trust so that we could begin work with ^{15}N . At this time it was thought absolutely crazy to have such an

advanced instrument as a mass spectrometer in a place like Jamaica and there were indeed great problems initially in getting it going, which were solved largely thanks to David Halliday, at that time a PhD student.

After I had been in Jamaica for seven years and the work was going quite well I began to feel that I was getting rather stale; that I didn't quite know how to work out these ideas about protein metabolism and protein synthesis. The MRC allowed me to take a sabbatical spell in the UK, while John Garrow took over as Acting Director. I was very fortunate to be received by Albert Neuberger with immense kindness in the laboratories of the Mint Stables of St. Mary's Hospital Medical School and I worked there for three years, from 1961–1964, with Joan Stephen. During that time we did our earliest experiments on protein turnover and measurement of protein synthesis rates in rat tissues, using labelled lysine as tracer and the Cartesian diver to measure the amounts biochemically. Here the diver method, used in a completely different way, played a very important part, because the early models of amino acid analysers were not sensitive enough for the small amounts of tissue we could get. Also, during that time I tried to get thoroughly familiar with the, to me, rather difficult subject of tracer kinetics and compartmental modelling, which was to stand us in good stead.

In 1964 I went back to Jamaica, while John Garrow returned to join the new MRC Clinical Research Centre, having done a very fine job of carrying on with the Unit during those three years. David Picou from Trinidad was the next most senior member of staff and he and I developed the first studies with ^{15}N for measuring protein turnover in children. A paper published by Picou and a St. Mary's student, Taylor Roberts, supported by the Wellcome Trust became a classical paper in that field.

Looking ahead, it became clear that we ought to think of this Unit becoming a West Indian Unit; we had two senior people whom I've already mentioned, who were extremely good, George Alleyne and David Picou. I tried to persuade the MRC to give them permanent appointments but failed. It seemed to me that this step was essential for continuity. Eventually Himsworth persuaded the Ministry for Overseas Development, the successor to the Colonial Office, to finance the Unit for three years in full and hand it over to the University of the West Indies as the administrative authority; this was done and is the only example I know of such an outcome for an overseas research unit.

This was 1970. I decided to go back to the UK and would have been perfectly happy to return to working in Professor Neuberger's laboratory at St. Mary's. However, it happened that Professor Platt, the Professor of Human Nutrition at the London School of Hygiene and Tropical Medicine, died, and I was appointed to succeed him. The challenge was to build up the department in the four areas of metabolism, clinical medicine, public health and policy and planning. Money was not as scarce then as it is now and the Wellcome Trust generously provided the Clinical Nutrition and Metabolism Unit at the

Hospital for Tropical Diseases, without which we could not have continued the metabolic research that we began in Jamaica.

The demands of teaching in a school of public health forced me for the first time to take a serious interest in community nutrition. I was also appointed to be part-time Nutrition Adviser to the Overseas Development Administration – the first time that they had had such a post. It was an interesting but sometimes frustrating experience to work as a member of a government department, and it gave me an insight that I had not had before into the general problems of development aid.

In this account I have concentrated on the early parts of my life that determined my subsequent career. I find it embarrassing that such an account is inevitably egocentric. I have always felt myself an amateur, both in medicine and in physiology and biochemistry, because I had no consistent training. I was a late developer and did not have anyone working under me or publish any substantive scientific paper until I was nearly 40. I think that I have been extremely fortunate: in having been given a good start in life; in having survived the war; in having been projected, almost by chance, into a most interesting field of work at a time when good support was available for it; last, but certainly not least, in having had so many able, enthusiastic and loyal colleagues.

In 1983 I was appointed President of the Nutrition Society following on from Ian MacDonald. This was an interesting challenge, particularly since during my presidency there was the International Congress of Nutrition at Brighton. I had been I suppose a moderately active member of the Society and at one time on its Council, and its Programmes Committee. I think it is a very important Society, and there were two or three things in which I was particularly interested. One was that we didn't really know what our Society was composed of, who the people were, what they wanted, what their interests were. The first step was to try and find out more about this and the questionnaire, which I think was so successful, was organized by Margaret Ashwell and the results analysed by her and Tim Coles. Everybody had predicted it would be a great flop, but to our amazement we got a more than 90% response and this gave us an idea of what the members of the Society wanted. The other thing I was rather keen on was trying to develop some sort of professional as opposed to scientific organization for nutritionists. There are of course people all over the place who call themselves nutritionists without any right to do so. Everybody in medicine and related sciences seems to think they know all that is necessary about nutrition. I felt that some structure was needed which would on the one hand lay down standards, and on the other help nutritionists with their careers. I sincerely hope that the Nutrition Society will pursue this in the not too distant future.

Recorded with Elsie M. Widdowson, 1988

Professor D.G. Armstrong

Head of Department
of Agricultural
Biochemistry
and Nutrition,
University of
Newcastle upon Tyne
President 1986–1989



It was a great privilege to be elected President of the Nutrition Society for the three year period 1986–1989, an event which I shall always remember as one of the highlights of my career. The reminiscences that follow must be of little

interest to members of the Society, particularly in the light of those which precede them and which relate to a distinguished company of eminent scholars. Mine may serve to indicate how occasionally good fortune rather than great ability allows one to hold the highest office in the Nutrition Society.

As my thoughts turn back to early days I realize that there were two aspects of my early life which greatly influenced the development of my interest in nutritional science. The first was the great liking that my twin brother and I had for farming; the annual two-week holiday on a small farm in the Yorkshire Dales with our widowed mother and elder brother were always looked forward to with great enthusiasm, and allowed us unlimited opportunities to care for sheep, milk cows, make hay and attempt those varied tasks which arise daily on a small livestock farm. How patient and kindly the farmer was! The second aspect was really an event. As a young six or seven year old schoolboy attending the local primary school I can still recall my thrill and excitement in watching one of the school masters – a Mr Sanderson – demonstrate the distillation of water. He used a standard boiling flask, a water-cooled condenser, a collecting flask and a Bunsen burner as his source of heat. From that moment on I knew that chemistry was to be one of the great interests of my life, and so it turned out to be. Over the intervening years I have often thought back to that demonstration by Mr Sanderson. It is one of my regrets – as must be the experience of many persons – that by the time my career had developed I had lost touch with my mentor and was never able to thank him for the inspiration he gave to my twin brother and me on that particular occasion. In this regard I was also fortunate in having as chemistry teacher in the local grammar school, a Miss Venables, who was a dedicated and able teacher. She instilled into pupils such as myself the basic tenets of chemistry and always in a most stimulating manner. How much so many must owe to our teachers at school who interested and inspired us and so influenced our future careers.

With this early background it is perhaps not surprising that I elected to read Agricultural Chemistry at university and so enrolled in the Faculty of Agriculture, Durham University. The Faculty and Department of Agricultural Chemistry was situated at Kings College, Newcastle upon Tyne in the University of Durham and was headed by Dr – later Professor – Brynmore Thomas who, although a Welshman, was proud of his Scottish education. Professor Thomas, a very modest man, had a great knowledge of the agricultural industry, and was keen on the application of chemistry to current problems in soil science and in animal nutrition (in those days the subject of Agricultural Chemistry encompassed both subjects). His considerable knowledge of the agricultural scene and his penetrating intellect proved a great stimulus to young persons who studied under him. He was not a ‘bench’ man himself – rather an ‘ideas’ man who did so much to assist his students in thinking logically and constructively about problems and where their solutions may lie.

After a two-year period of postgraduate research leading ultimately to the

award of a PhD degree I was fortunate enough to have the opportunity of spending a year in Professor H.H. Mitchell's laboratory of Animal Nutrition at the University of Illinois. ‘Mitch’, as he was known affectionately by staff and students alike, was a recognized world authority in the field of nutrition. His particular interest lay in energy metabolism studies, particularly with cattle and sheep, and in the nutritional evaluation of proteins. It was Professor Mitchell and another of his colleagues, Professor W.C. Rose, the discoverer of the last of the essential amino acids, who encouraged me to concentrate on biochemistry as one of the foundations of nutritional knowledge. Strangely enough, although each greatly respected the other, they would never speak directly to each other; on numerous occasions I acted as a go-between for their exchange of views. Dr, later to become Sir, Kenneth Blaxter FRS, and Dr P. Braude had each spent a period of time in Dr Mitchell's laboratory before I arrived. As Mitch said to me once, ‘With your arrival I have seen all types of Britishers’! During my year at Illinois I was greatly privileged to meet a considerable number of eminent scientists interested in various aspects of nutrition. Some were Americans such as Dr Kleiber from California and Dr L.A. Maynard from Cornell, while others like Sir David Cuthbertson and Dr S.K. Kon from the UK visited the laboratory while I was there.

It was during my year at Illinois that I first met Dr K.L. Blaxter. I had gone up with Dr Mitchell and his wife to spend a week's holiday at a lakeside cottage in Wisconsin. Dr Blaxter, on a visit to various laboratories in the USA, was invited by Dr and Mrs Mitchell to spend two or three nights at the cottage. In the later part of each evening Dr and Mrs Mitchell used to enjoy a singsong, and it was KLB and I who introduced them to the song of ‘Cocaine Bill and Morphine Sue’ which, for some reason that I never fathomed, Mitch took particular pleasure in. (It was the words, and not the singing that so impressed our hosts.) During my year's stay at Illinois, and after completing a research programme, I was able to visit a number of laboratories, including that of Dr Samuel Brody at Columbia, Missouri. He was a remarkable scientist who, on the basis of much of his own research and his interpretation of others' work, wrote *Bioenergetics and Growth*, by any standards a classical work in the field of energy metabolism. On the first day of my visit there he announced at the appropriate time that we would take some lunch; he said we would go in his car since he knew the battered vehicles that young workers travelled around in. What a shock he had when I showed him a brand new Ford ‘Custom Liner’ which I had inadvertently parked near his respectable but ancient Chevrolet. I was in the process of delivering the new Ford from Chicago to Denver, Colorado, as an agent for the United States Gypsum Co., and had decided to travel via Columbia, Missouri!

Subsequently, on my return to the United Kingdom, Dr Blaxter offered me a position in the Department of Nutrition, Hannah Dairy Research Institute which he headed, and hence began a close friendship and understanding that

is still very strong today; Sir Kenneth Blaxter holds a Visiting Professorship in my Department at Newcastle. The ten years I spent at the Hannah were happy and memorable ones for me. Dr Blaxter had already made a considerable contribution to man's knowledge of energy metabolism of ruminant livestock, assisted by the late Dr J. Rook and by Dr N. Graham. Dr Graham is still active in the CSIRO Ian Clunies Ross Research Laboratory at Prospect, Australia. In joining KLB's staff I replaced Dr Rook, who had taken up an appointment at the then National Institute for Research in Dairying, Shinfield, Reading. KLB and his colleagues set to work to build six closed circuit respiration chambers for sheep, and eventually one for cattle. It was a very interesting time, and I gained valuable experience in how to weld and do a variety of plumbing jobs and general construction work. KLB must frequently have wished for the services of skilled workers, for which, unfortunately, there was no source of funding.

With the various respiration chambers completed much research was conducted into various aspects of energy metabolism, including studies on the contribution of the volatile fatty acids to overall supply of energy in ruminants, on climatic environment in relation to energy metabolism and on the evaluation of foods for ruminant livestock. The laboratory was serviced by a wonderful group of technicians and, under the leadership of KLB, the laboratory gained world-wide recognition. 'Work hard and play hard' was a motto fitting for the Department.

In 1963 I was appointed to a readership at Kings College in the University of Durham, later to become the University of Newcastle upon Tyne, and with it headship of the Department of Agricultural Biochemistry. The old Department of Agricultural Chemistry had been split into a Department of Soil Science and a Department of Agricultural Biochemistry. In those early days it comprised three staff, Mr A. Thompson, Mr R. Seeley and myself. What loyal and hard working colleagues Alan and Bob proved to be! We were also fortunate over the ensuing years in attracting a number of UK and overseas visitors to work in the Department for periods of time varying from 12 weeks to a year. In the beginning we concentrated on nutritional studies with particular reference to digestion and metabolism in ruminant livestock, to major mineral requirements of pigs and to the nutritional evaluation of proteins for simple-stomached animals.

Over the years staff numbers increased until by 1987 they numbered eight. Two important extensions to our work have taken place in these later years. Firstly, the Department has built up a considerable teaching and research expertise in the field of human nutrition under the capable guidance of Dr J. Mathers. More recently, and through the award of a 'New Blood' lectureship to the Department, we have been able to develop a teaching and research interest in the field of biotechnology under the guidance of Dr H. Gilbert.

One of the great attractions of being a member of staff of a university

department is that it allows one to continue the roles of teacher and researcher. I have derived great pleasure over the years in watching the young undergraduates develop and mature during the three years they are with us, and in stimulating their interest in, and enthusiasm for, the subject of nutrition. In addition I have had the excitement of sharing progress in research with post-graduate and post-doctorate personnel. It is appropriate to express my thanks to the University and to organizations such as the Royal Society and the British Council for making it possible for me to visit laboratories in many parts of the world for the exchange of knowledge and research experience, and also for the Department to receive visitors from numerous countries of the world. It is certainly true that for the biological scientist, and with few exceptions for other scientists, there are no barriers to international exchange and co-operation.

It would also be remiss of me not to express my deepest thanks to colleagues in the Department over recent years. The high regard in which I believe the Department is held owes so much to their abilities and hard work, and I owe them much for their friendship and loyalty.

These later years have been associated with considerable changes in the government's attitude to the funding of universities and their teaching and research. The University Grants Committee, which over the years has served the interests of the universities well, and I believe also the nation, has been replaced by the University Funding Committee. This latter body is likely to exert a much more direct influence on the way the individual universities carry out their business. While I think there is widespread agreement within the universities that some change was necessary – indeed in my opinion it was long overdue, it is to be hoped that the forces that operate in a market economy do not become so dominant as to impair seriously the role of these institutions as centres of learning and research. The UK has few natural resources remaining. One of these, and it may well be the most important of all, is that which results from the education of the young; they become the leaders of the next generation.

The Nutrition Society itself has undergone a considerable restructuring during the last three years. It is my belief that the changes introduced will strengthen the Society and allow it to play an even greater role, as befits a prestigious scientific society, in advancing knowledge of nutritional science throughout the world and, through the provision of guidance and counsel on nutritional matters, serve well the nation and its government.

Typescript, 1989

Sir Rudolph Peters

Professor of
Biochemistry,
University of Oxford
Elected Hon. Member 1962



I was first introduced to research by Sir Joseph Barcroft when I was an advanced student in the Department of Physiology, which included Biochemistry, in Cambridge in 1910. I worked out the relationship between iron and oxygen in haemoglobin, and found that it was stoichiometric; this was probably the first time a colloid was shown to react stoichiometrically. Then I worked with Professor A.V. Hill for one year, where I studied the problem of

heat in muscle. This was of great interest in the laboratory because Hopkins and Fletcher had produced a fundamental paper on the subject not so very long before. The laboratory was completely geared to research and any young man fortunate enough to work in it could hardly fail to continue to do research for the rest of his life.

During the 1914–1918 war I worked with Sir Joseph Barcroft again, this time at the Porton Research Establishment, trying to find antidotes against poison gases. In this way I became interested in arsenicals and I started to investigate these in 1919–1920 with a chemist called McClelland, and we used protozoa. It was during the course of this that I thought it would be interesting to rear the protozoa on an artificial medium and not upon hay extract. This led me to the problem of isolating vitamin B₁ which was then called vitamin B. This is how I got into nutrition, and I continued in a mild way to work on what is now known as thiamine, in the laboratory of Hopkins, where I was a demonstrator, until 1923.

In 1923 I was invited to become Professor of Biochemistry in Oxford, a post that I held for 31 years. During the time there I collaborated in the first place with Mr Kinnersley on the isolation of thiamine. This was a subject on which many people in the world were engaged, though at the time I started nothing much had been published. I stuck to the pigeon test, though this was regarded by some, even by Sir Jack Drummond, as very obstinate. It had been given up as hopeless by some of the then great authorities on vitamin B because they had got strange results with it. I set out to find the cause of these strange results and discovered that with pigeons you could get pseudo-cures of vitamin B₁ deficiency if you gave them water with a little sugar in it. When a bird first became convulsive very often it appeared to be cured for a few days. After this, when it got convulsions again, then the dose you gave cured it for a definite number of days according to the number of doses that you gave. We used the test continuously until the end of the research when we isolated thiamine about 1933. I think we were the first people to get the water in the crystals right. Windaus had been at it before, and afterwards came Osaka and Williams. In 1925 Jansen and Donath isolated what was very nearly pure thiamine from rice polishings. I say it was very nearly pure thiamine because it still gave a reaction which is not given by pure thiamine. I did in fact work out a test for thiamine which we called the formaldehyde azo test, which was very specific for thiamine.

During all this time, with the pigeon test before me, it was impossible not to get interested in the origin of the convulsions which were so definitely related to the dose of thiamine. This led to our trying various muscle and other preparations to see if we could not get *in vitro* action of the vitamin. This failed completely, as indeed Jack Drummond and Dr Marrian had found with skeletal muscle of thiamine deficient pigeons, and it was not until I turned to the brain that I began to get any results which were of significance. At once we

were able to show that there was a bigger, more rapid capacity for reducing lactic acid in the brains of vitamin deficient pigeons. This gradually led us to the general issue of the oxygen uptake. After tying this firmly to lactic acid, my colleagues Dr Passmore and Dr Meiklejohn and I finally obtained a result on adding the vitamin in the presence of lactic acid or glucose. We got an increased oxygen uptake. It was so small that at first I didn't believe it and did not bother to work out the results that night. But the results were right. This I think was the first example of the *in vitro* action of a vitamin. It led to the connection with pyruvate work especially done with R.S.H. Thompson, and then to the whole field of the co-factor, because not so very long afterwards colleagues in Germany, Lohmann and Schuster, discovered that thiamine was part of thiamine pyrophosphate, and that this was the factor necessary for yeast. From 1927 onwards I was joined by Vera Reader to work on the microbiological side of the problems. She soon got interested in our problem, and started the colony of rats in my laboratory in Oxford, so that we were able to relate the work we were doing on pigeons from the point of view of the daily dose also to rats. During all this time, up to about 1935–1936, we were naturally interested in other factors and were quite convinced that there were at least two thermolabile factors required for pigeon nutrition, and possibly two more for rat nutrition. The work on this side was done with Dr Carter and Mr O'Brien. I think that there has been some misunderstanding about this work. What was called vitamin B₃ had been already described by Williams and Waterman. We called our factors B₄ and B₅ because it did not seem right to characterize them more definitely. There was no doubt about the facts, and there was no doubt that they could only be explained by the presence of other hitherto unrecognized factors. But one has to remember that it was not until 1926 that people were convinced that there were two factors in the vitamin B complex, and now we know that there are anything from 12–14 factors. However, people thought that our factors were not realities, and that they simply did not exist. If they did not exist one still had to explain the experimental findings on pyruvate oxidase systems, as we called them, in which thiamine was a definite factor as was the pyrophosphate.

The same system was subsequently used for some of our work on toxic substances because we found that pyruvate oxidase systems tested very easily for thiamine. During the war we were turned on to the problem of an antidote for Lewisite. I had been interested in this especially with Dr Ernest Walker in the 1920s, and then we knew, as did some of the folk in the United States, that it was somehow connected with SH groups. We were not quite clear how this was so. When it was taken up by my team in the war, Stocken and Thompson discovered that the clue lay in the fact that Lewisite was attached to two essential thiol groups in the pyruvate oxidase system. This led, I think we can say, to the first instance of an antidote being discovered by logical means. The antidote became known as British Antilewisite, 2:3 dimercaptopropanol.

During the war we also worked as a team, especially with Dr Austin and Mr Philpot, on the action of mustard gas, which produced vesicles, but this is a very long story. However, some very important theoretical advances were made.

After the war I had another challenge. There was a fluorine complex called fluoroacetate which attacked no isolated enzyme but was very toxic, and it produced convulsions. Marrack had discovered in South Africa that it was the active principle of a well-known poisonous plant. I started to work on this with two colleagues, Bedford and then Buffa, because it seemed important to find out how the fluoroacetate was working. We found the interesting fact that it wasn't fluoroacetate itself that was toxic, but that it was converted by the action of an enzyme, now known as synthase, into fluoroacetic acid, and that it was this that blocked the citrate in the appropriate stage of the tricarboxylic cycle and caused enormous accumulation of citrate. I called this process lethal synthesis, and we were still working on it.

Recorded with Alice M. Copping, 1970

Professor R.A. McCance

Professor of
Experimental
Medicine,
University of
Cambridge
Elected Hon. Member 1973



I was born in the north of Ireland in 1898 in what was then real country north west of Belfast. I went to school at St. Bees, Cumbria, as my two brothers had done. I then joined the RNAS (Royal Naval Air Service) and served as a pilot

in the latter part of the 1914–1918 war. I had been trained to fly ‘Camels’, which were single seater fighter machines, but found myself flying two-seater ‘observation’ aircraft off one of the midship turrets of the *Indomitable*, one of the second Battle Cruiser squadron of the Grand Fleet.

After the war I thought of going into the Department of Agriculture in Ireland. On the advice of those in charge of it I went up to Cambridge, intending to take a diploma in Agriculture. So 1919 found me working at the County Farm near Antrim for six months, a valuable experience which has stood me in good stead ever since. I went up to Sidney Sussex College, Cambridge, in October. There I took both parts of the Natural Sciences Tripos instead of the Diploma in Agriculture because, during those three years there had been a ‘rebellion’ in Ireland and no one could offer me anything with any certainty.

Towards the end of this time I was appointed supervisor in physiology at several colleges in Cambridge, and the money I earned enabled me to marry Mary Lindsay MacGregor in 1922. I worked under Professor F.G. Hopkins for three years, where I learned something about Biochemistry and what research it might involve. I obtained my PhD on the strength of the work I had done.

I went to King’s College Hospital in 1926 to complete my qualifications in medicine. I had a grant of £30 per annum to assist Dr R.D. Lawrence in the Biochemical and Diabetic Department. I worked there in my spare time and made some analyses for RDL on the carbohydrate content of cooked fruit and vegetables, a matter of great importance to diabetics in those days. I introduced a new dimension to this work by separating the available carbohydrates, sugars, dextrans and starch from the polysaccharides that are always present in vegetables and fruits and which are now known as dietary fibre. Professor Cathcart of Glasgow looked with favour on these efforts of mine to study the composition of cooked foods. He was studying the foods eaten by the poor, and wanted information about the composition of cooked foods, particularly meat and fish, and he suggested that I should undertake this work, and apply to the Medical Research Council for a grant to cover the cost of an assistant and a technician.

I used to go to the hospital kitchen in the basement to get the big joints cooked in the hospital ovens. There I met Elsie Widdowson, a momentous meeting, for we have remained together for well over 50 years.

A highly productive period followed. Diabetics, and particularly those in coma, provided me with many problems, one being that their urine contained no chloride. This observation led on to my experimental and quantitative study of salt deficiency in man, which involved making a number of subjects, male and female, salt deficient. This was really rather a herculean task, for it involved persuading healthy young men and women to eat a salt-free diet and to lie and sweat in a hot air bath for two hours a day for 14 days. Then, when they were salt-deficient, they had to submit to a variety of tests, in particular of their renal function.

Somewhere about 1934 I was allowed some beds for my patients and began admitting those referred to me that presented problems. One in particular I remember with polycythemia rubra vera. This lady, although she never knew it, played a large part in the future of three members of the department. I treated her with acetyl phenylhydrazine, and by so doing broke down enough red cells to liberate 5 g of iron in her body. To our intense surprise none of it was excreted, in spite of all I had been taught. We then injected iron intravenously into ourselves and colleagues, and we did not excrete iron either. This led us to suggest that the amount of iron in the body must be regulated, not by excretion, but by controlled absorption. The publication of this led to an invitation from Professor Ryle, Regius Professor of Physics, to come to Cambridge as Reader in Medicine, and a Fellowship in my old College. I accepted, and the Medical Research Council agreed to my taking Elsie Widdowson and a good technician with me, so our joint work was little interrupted.

Before this, however, my house physician at King’s, Winifred Young, who was there while the salt deficiency experiments were going on, had gone to the Children’s Hospital in Birmingham under Sir Leonard Parsons. One of her jobs there was to test the babies’ urine for albumen and sugar. Partly from habit she tested some for chlorides and to her surprise found they contained none. This led to a great deal of work and the discovery of the immaturity of infant kidneys, which opened up the whole subject of the physiology of the newborn.

As soon as the war began Elsie and I started an ‘experimental study of rationing’. This was fun, and indeed all our work in the first years of the war was made interesting because the subjects of our experiments were such stimulating people; Colin Bertram, a zoologist who had worked in the Antarctic, and his wife Kate, a zoologist, Douglas Black and Andrew Huxley, both later knighted and one of them a Nobel prize winner, and last but by no means least Jimmy Robinson. He met his wife Marion in our department and both later became Professors in Dunedin, New Zealand.

In our experimental study of rationing our rations of milk, meat, eggs and other good things were so small that they were considered intolerable by our critics; we had wholemeal flour for bread and cooking purposes, and this was unrationed, as were potatoes.

At the end of three months we decided to go to the Lake District to see if we were really as fit as we thought we were. Jimmy Robinson and I started at 5.30 am on Boxing Day 1939, and cycled up in two and a half days against a northerly wind. The others started a day behind us by car with the special flour which was part of our rations. We met by arrangement at the Black Horse, Skipton. The last day of our ride was made difficult by snow-covered roads. Once at our base we made a number of timed ascents, carrying loads of 40 lbs or so, at our best speeds up well-known hills and mountains and enjoyed a fortnight of superb frosty weather. On 2nd January 1940 Andrew

Huxley and I, carrying loads of 8 and 9 lbs, covered a distance of 36 miles and 7000 feet of ascent and descent at an overall speed of 3.2 mph including stops.

Although we had added 'creta preparata' (in other words chalk) to our flour to compensate for the small amounts of calcium supplied by our rations of milk and cheese, there had been a suggestion from limited data that some people (myself included) were seldom in calcium balance. We accordingly embarked upon a long series of balance experiments to find out more about this. We discovered that phytic acid, an ingredient of wholemeal flour but not of white, formed complex insoluble salts with calcium and iron. This ultimately led to the statutory addition of chalk to all flour used for bread-making in the United Kingdom.

Meanwhile I was getting involved in work on naval matters, for I had been made Chairman of a joint Medical Research Council and Royal Naval Committee on Survival at Sea. Our work involved the problem of whether one should ever drink sea water when dehydrated after shipwreck. The answer was, one should not.

Much of this work would have been impossible had it not been for two Cambridge graduates in physiology, Romaine Hervey and Bill Keatinge. Both joined the Navy for their war service and were seconded to work on these problems in our department. We made a useful discovery in this work by showing that 100 g of glucose or cane sugar – or for that matter boiled sweets – were metabolized to 100 g of water, all much easier than water to fit into a survivor's ration. When Eric Glaser joined the department he pioneered some work of a different kind. One of his exploits was to plunge into the swimming tank in the Emmanuel College garden in the middle of winter to study the effects of sudden immersion in cold water.

An important part of our task was to study the habitability and the boarding facilities of the new inflatable life rafts. There were numerous trials at Portsmouth. Volunteers, some Wrens among them, jumped into the water from a height of 10–20 feet and boarded the rafts waiting for them not far away. The habitability of rafts was tried out in the cold of Tromsø in January and later in the heat of Singapore. Motion sickness was a very real problem for soldiers in landing craft and in this Eric Glaser was at his best. We used the artificial waves in swimming baths for our trials and finally the naval ship tank. This always impressed me for it was so long it had to conform to the curvature of the earth's surface. The only effective drug against sea sickness was 1. hyoscine, and in very small doses too.

We ended up with some Atlantic trials conducted from a small destroyer, the *Carron*. Our time was limited and the sea very rough. We took a battering on the way to our appointed spot. During the morning the weather eased a little and the rafts were prepared for launching. The Royal Navy raft, looked after by Surgeon Captain Baskerville, behaved splendidly, although we could only see it now and again when it was on the right side of the really monstrous waves.

The crew were re-embarked with the aid of helicopter straps. Glaser now set off in a round raft produced commercially, with a crew of volunteers. Within minutes all were sick and one would have been overboard had not Eric Glaser grasped him by the leg. Things began to get dangerous and flares were sent off. The *Carron* came alongside and the volunteers were all safely transferred to her.

As the war drew to a close we persuaded Sir Edward Mellanby (Secretary of the Medical Research Council) to sponsor a visit to Germany to see what effect the war and the rationing and shortages of food had had on the civilian population. Accordingly, in the spring of 1946 Elsie and I set out to see whether we could find somewhere we might set up a base for studies we wished to make. We visited Hannover, Hamburg, Kiel, Göttingen, Essen and Wuppertal. Only in the last place did we find a satisfactory hospital in which to work and a good laboratory (the I.G. Farben). Best of all we found a doctor, Dorothy Rosenbaum, who spoke colloquial German and English, and who was anxious to help us.

Although we surmised that undernutrition must alter the composition of the 'lean body mass' we were anxious to find out why some people got oedema when they were undernourished and some did not. We wanted, therefore, to take samples of blood and compare the concentration of albumen in it in men with oedema and those without.

When we visited the gaol at Kiel we asked for three subjects with oedema and three without to be shown to us. The warders, feeling perhaps that they were a little unsure what oedema looked like, and that we had better be left to make our own selection, lined up eight men completely naked in front of us, thus leaving us to make our own selection as to which of them had or had not got oedema.

The policy we adopted at our base was to invite people experienced in the new subjects which none of us felt competent to cover ourselves. Sheila Sherlock came and examined the livers and Sheila Howarth the hearts, and Philip Gell to see if anything was wrong with the immune response. This put rather a strain on our admirable research nurse, Lois Thrussell, and indeed on myself too, because it was all very well to catheterize people with heart disease and puncture those with faulty livers but normal people had to be examined to establish a base line. Tact was almost as necessary as catheters and puncturing needles, and we owe a great debt of gratitude to Dorothy Rosenbaum for all she did to help to overcome the problems.

Other 'working' visitors were Derek Russell Davis who looked sympathetically into patient's anxiety neuroses, and Mavis Gunther who studied breast milk production and tried to improve it. Dr Berridge – now alas dead – put all his radiological skill at our disposal. Patients' bones were X-rayed and also their gastrointestinal tracts. He found, for example, that wholemeal bread passed through more rapidly than white. I was one of the well-nourished

controls, and Berridge found that my appendix had a lot of calcified lumps in it. This explains the grumbling appendicitis I had as a child reared on unpasteurized milk from a very few cows.

We had some distinguished visitors. Sir Edward and Lady Mellanby, Dame Harriette Chick and Dr Hume, and one I shall never forget was Sir James Spence. When he left we all went along in two cars to see him off at Köln station only to find that the time had been advanced one hour, which none of us had remembered, and the train had left. The only thing to do was to beat the train to the Dutch frontier where we knew it would make a long stop for 'customs'. When we got near the frontier we could see that the train was still there. We had made it!

I have already touched upon the discovery made by Winifred Young at Birmingham about the comparative inefficiency of the infant's kidneys when compared with those of a healthy adult on the bases of body weight or surface area. Work had been proceeding quietly on this, and Rex Dean had shown that phosphates were poorly excreted by babies. We found that renal immaturity was also true of newborn rats, rabbits, puppies, kittens and finally pigs. Each species was different, but all the newborn animals maintained a normal 'internal environment' in spite of the proven inefficiency of their kidneys and a large intake of protein in their food. We pondered for a long time over this before the solution dawned upon us. Growth of the body was so rapid in all these young animals that the increase in number and size of the cells in the body was taking care of such a large part of the nitrogen intake from colostrum and later milk that the newborn animal could almost do without kidneys at all.

In parallel with our work on undernutrition in Germany we made comparable experiments on animals. My responsibility was the pigs. 'Large White' pigs grow very fast and should reach a weight of 250 kg or more in the course of a year. They can, however, be made so severely undernourished that at the end of a year they weigh only 5–5.5 kg. They grow well if rehabilitated, and become pregnant if they are served and make good mothers, but they never attain the size of a pig that has not been held back by undernutrition. Elsie had the bright idea of keeping these pigs undernourished for longer periods, say for two or three years. At the end they will show a burst of growth if they are rehabilitated. The extent of this growth is more limited, however, the longer the period of undernutrition has been.

When we were in Germany milk and even dried milk was in short supply. Rex Dean worked for some time on a soya bean preparation for rehabilitating undernourished infants. After we had returned to Cambridge Sir Harold Himsworth suggested to Dean that he should go to Uganda to investigate the use of this preparation for malnourished children. A Unit was eventually established in Kampala, near Mulago Hospital, destined to become the medical and surgical centre for East Africa.

While working there Dean fell victim to a slowly ascending paralysis of his

feet and legs which crippled him, but with great fortitude he struggled on till he was on the point of death and had to be invalided home. Roger Whitehead, who had joined Dean some years earlier, looked after the Unit for a time. When I retired from Cambridge in 1966, my wife had recently died and when Sir Harold asked me if I would look after the Unit for a year or so till a suitable young medically qualified director could be found to replace Dean, I was glad to go, recruited some staff and set out in 1966.

I soon found that the wards, even in Mulago Hospital and more especially in the small ward we had in the Unit, were different from anything I had seen before. I did not expect to find the mothers lying about on the floor, for instance, and most of the children suffering from worms or horrid superficial ulcers on their hips and buttocks, sometimes with dangerous diphtheroid micro-organisms in them.

I was lucky in my staff, for Brian Wharton – now a Professor in Glasgow – made some really first class observations, among others that the children were suffering from hypothermia during the night. Tom Hall, the son of our clergyman at Woodbourne near Belfast where I had spent my childhood, was one of the staff I had recruited. He had been a public health officer all his active life, and I had met him in Gibraltar during the war. He was my ally in retirement, as he had been as a boy.

For anyone like myself Kampala was a lovely place in which to live in the 1960s. The villages were a mass of bougainvillaeas, a tree in the garden grew avocado pears, and the geckos ran upside-down on the ceilings, not to speak of the bird life of which I identified about 200 different species. The game parks and reserves were rather fun, especially for visitors. If one rose early and went for a walk, one might always find an elephant taking the lid off one's dustbin to see what he could find to eat in it. A colleague, Keith McCullough, was working on elephants, and I had the interesting experience of an aeroplane trip with him, flying low over a large area including the Murchison Falls, to locate the herds of elephants. Two men were with us who had been assigned the job of culling a large part of the elephant population because they were breaking into the Budongo forest and destroying all the young mahogany trees.

Then I retired to Cambridge only to be knocked off my bicycle, on which I had cycled well over 25,000 miles, by a pedestrian on Midsummer Common. This fractured my femur, and after a few more minor mishaps, I fell down the stairs in my residence in Sussex Street and fractured my pelvis, which was very painful. I don't advise this accident, but it led to my finding a less risky home for myself at 32 Havenfield.

I was brought up to think of myself as a physiologist, but nutrition is surely a branch of physiology. I was present at the inaugural meeting of the Nutrition Society. Our proceedings were recorded in small volumes, only 8½" × 5", which some of you may never have seen! I have served on the Editorial Board

and someone must have thought well enough of me to propose that I be made an Honorary Member, a distinction I very much appreciate.

Typescript, 1989

Dr R. Passmore

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Elected Hon. Member 1983



My first interest in nutrition was when, as a second year medical student in the biochemistry class at Oxford, Professor Peters asked Sir Robert McCarrison to give us a lecture. He was a handsome man with a striking appearance. I well remember him giving us a splendid picture of the problems of nutrition in the world, and this made a great impact on a young medical student. When I had

finished Anatomy and Physiology, I asked Professor Peters if I could work in his laboratory for a year before I went on to do my clinical work. He not only said 'yes', but he offered to take me, and my friend Peter Meiklejohn, into his own laboratory to help him with his work. In a sense all my research career has been an anticlimax because in 1932 my name and Meiklejohn's were on a paper which was read to The Royal Society, in which the first description of a vitamin, thiamine, acting as co-enzyme was made. I well remember being at The Royal Society when Professor Peters read the paper, which we all realized was important, and it was the start of what is an enormous development in biochemistry.

I would like to say a word or two about Sir Rudolph Peters, as he became. I never worked with him afterwards, but I met him often and even 50 years after, when he was 80, the conversation was always exactly the same. He would tell me about the experiment he did the previous afternoon and say: 'Passmore, what do you think that means, did I do the right thing?' He was an enormous enthusiast for laboratory work and he gave me great interest in it, but he was also a person of wide vision. His father had been a general practitioner and he himself had done some medicine; he was always interested in the application and meaning of biochemistry in terms of practical medicine. He had a very great influence on me.

I next went to St. Mary's Hospital and qualified in medicine and then did post-graduate work for two years in the Department of Pathology at Cambridge; I enjoyed that very much and learned a lot, but when I got an opportunity to go abroad, I took it. I went in 1937 as Assistant Director in McCarrison's old laboratory in Coonoor, South India, where Dr W.R. Aykroyd was then the Director. For three years I worked closely with Wallace Aykroyd. He was a man of enormous common sense and practicality and was then introducing systematic methods of food analysis in India and dietary surveys and methods of clinical assessment and also health education. In all of these he put India well in advance of most other countries. He was a very different man from McCarrison, a much more down-to-earth person, with extraordinary facility for knowing what was practical and what could be done within a very limited budget. I think this was the reason for his success.

Soon after war broke out I was called into the army and spent the first three years of the war on active service with the Fourth and Fifth Indian Divisions in North Africa. Then I came back to India and was posted to base hospitals, first in Calcutta and then in Lucknow, huge hospitals with over 1000 beds. We had a large number of sepoys going through there who had been evacuated from the Burma front, and I acquired practical experience in dealing with patients who had been severely undernourished and who had severe anaemia as a result of the combined infections and poor diets; many of them had been behind the Japanese lines with the Chindit forces.

In India I really had two separate activities. I learned a great deal about the Public Health direction of nutrition from Wallace Aykroyd, and in the second

half of my ten years there I learned about the army, for which I have a great respect, and about the treatment of severely undernourished people.

Like many people who were in the Indian Medical Service in 1946 I knew that soon I would be out of a job. I wrote many applications and the successful one was to Professor Frank Crew, professor of social medicine in Edinburgh who offered me a post as applied physiologist in the Department of Public Health, or Social Medicine, as it was then called. I turned up there after ten years in India in the first week of January 1947, and I remember it well because it was snowing and the snow didn't lift until April.

I got on well with Frank Crew; he was a man of wide interests, first and foremost a biologist but he was also a doctor. He had also been a keen army man, a gunner in the First World War. So we had many things in common. I remember Crew now primarily as a conversationalist, and I suspect that he was in the same class as Oscar Wilde. I used to enjoy the mornings at the Usher Institute when we had coffee in the Common Room, and the splendid conversation on many subjects.

I was starting to build up an Applied Physiology Unit in Edinburgh and then a chance came into my life. I had known Hugh Magee for some time, who was then the nutrition adviser in the Ministry of Health. My wife and I first met Hugh and his wife on a trip down the Volga in 1935 after going to the International Physiological Conference in Leningrad and we became friends. Hugh Magee had been in Germany after the war in his official capacity; he was an attractive Ulsterman and so a great scrounger; he came back from Germany with a machine called a Kofranyi Michaelis respirometer with which the Germans had been able to make a large number of measurements, by indirect calorimetry, of energy expenditure in industry. This was lying in his office in the Ministry and he didn't know what to do with it; so he gave it to me and asked if I could see if it would work. I took it up to Edinburgh and found it did work very well. We got some more from Germany, and with them we could get assessments of energy expenditure, not only in industrial circumstances, but also in the home and off-duty recreations. I was soon joined by John Durnin, whom I first met when he came over as a young post-graduate student from Glasgow. With him and Professor Garry, a big survey on the energy expenditure of miners and clerks in a mine in East Fife was carried out in 1952. Professor McCance, Dr Elsie Widdowson and the late Dr Otto Edholm, an old school friend of mine, made a similar survey on Sandhurst cadets, and these two surveys put energy expenditure on the international nutrition map.

In the middle 1950s Sir Stanley Davidson, who was Professor of Medicine in Edinburgh, asked me and Dr Meiklejohn to produce a new edition of his book on Human Dietetics which had been published when he had been Professor of Medicine in Aberdeen, where he was a friend of Boyd Orr before the war. We agreed to do this and it first came out in 1959. A great part of my work since then has been with this book, and I owe an enormous amount to

Sir Stanley Davidson in my understanding of nutrition in relation to medicine and how to write about it. Sir Stanley was a very great man; he started life as a laboratory worker and he made his reputation by being the first person in this country to use liver for the treatment of pernicious anaemia; this in 1930 got him a reputation as a good scientific physician and then the post of Professor of Medicine in Aberdeen. There he made epidemiological studies on iron deficiency anaemia in women, which used to be known as chlorosis, then rather a mystery disease; many people thought it was a neurosis. Davidson was assisted critically by Dr Isabella Leitch and they wrote a long review, now a classic, on iron deficiency anaemia. I think it is largely as a result of their work that the term chlorosis disappeared from textbooks.

Stanley was not a great writer, but he was a good talker and also, unlike some good talkers, a very good listener. He always had the brightest of the young medical post-graduates round him in his department and a lot of distinguished visitors. He would listen to them and ask them questions and often get them to write drafts for him. He was an absolute master at picking out sense from nonsense, and putting the sense into a form that the ordinary person could understand. He never allowed any sentence to be printed which was obscure or a nonsense, because he read everything so carefully. He once said, 'No wonder my books are so good and so often read because I've read every word of them myself'. I have now produced eight editions of this book, four of them working with Sir Stanley and after that mainly with Stewart Truswell and now in the last edition with Martin Eastwood, an Edinburgh gastroenterologist. This book I think has been my main contribution to nutrition, because it has become a standard text-book for dietitians. The sales of it greatly exceed the number of dietitians, most of whom have got it, and I always wonder who actually buys it. I suspect the majority of sales are to the food industry. I think food industrialists like it because it gives an account of nutrition consistent with orthodox medicine, and because of Stanley Davidson's name; I deduce this because, whenever I go amongst food industrialists, they seem to know my name.

When I came back from India in 1947 the Nutrition Society was already well established. I at once became a member and I have been a member ever since; now I am very proud to be an honorary member and I have the highest opinion of the Society. I have never been an Officer of it, although I have been on the Scottish committee and I've always appreciated and enjoyed the fact that it has a separate Scottish element. I have published most of my most important papers in the *British Journal of Nutrition*, especially those dealing with the effects of overfeeding on thin and fat people. I was pleased to do this because I had the greatest respect for the Editor, Dr Kon, who took a great deal of trouble in editing papers. The British Nutrition Society owes its position very largely to the quality of the journal and it was Kon who built it up to international standards. Although he edited papers very strictly and I had

one or two clashes with him, we always remained good friends. He was a very fine Editor and the Society owes a great deal to him.

Recorded with Elsie M. Widdowson, 1986

Dr Alice M. Copping

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Elected Hon. Member 1983



I was born in Stratford, New Zealand, on 14th May 1905. My life began with good parents who had a strong belief in education; what was not learned at

school was learned at home. Being a sickly child, I did not start school until I was eight, when I began at the district school. Later, when I showed an interest in science, Father said that if I was going to do science, then I must learn German. As German was not taught in school in those days, after the 1914–1918 war, I had private tuition, and the knowledge of German later proved to be very useful. In 1923 I enrolled at Victoria University College where I studied chemistry, gaining a BSc second class Honours. I graduated in 1925 and added MSc in organic chemistry in 1926. I won a travel scholarship and came to London to study biochemistry with Professor J.C. Drummond at University College. Drummond set me to work on the problem of the 'bios' requirements and production of different strains of yeast. After trying to work out a method, I was sent to Berlin to work with Otto Warburg, and learn his method. After a short time I learned to use his manometer, and brought the first set of Warburg manometers back to London. There were no computers in those days and all the awful calculations were done by logarithms. Incidentally, I took a course in medical bacteriology in order to cultivate the yeasts, so that early on I learned the beginnings of nutrition both in man and organisms.

After two years I went to see Sir Charles Martin at the Lister Institute. He suggested that Harriette Chick needed a personal assistant, and I worked with Dr Chick from September 1929 to December 1930 on B vitamin problems. As my mother was ill I returned to New Zealand and took a temporary post as lecturer in physiology and nutrition in the Department of Home Science at the University of Otago. At the end of the year no definite job was available in New Zealand so I agreed to Dr Chick's request to come back to the Lister Institute. My journey back to London in 1932 took me on a cargo boat from Sydney round the south coast of Australia, and let me visit Sir Charles Martin, who was then in Adelaide. He showed me some of the research on cobalt deficiency disease in sheep; this led to identification of cobalt as part of the vitamin B₁₂ molecule.

My work with Dr Chick was as part-time research assistant and to help her with *Nutrition Abstracts and Reviews*. Research on methods for the estimation of vitamin D occupied me for a few months before I returned to work on biochemistry, biology and microbiology of B vitamins. Dr Chick was much involved with the Accessory Food Factors Committee of the Medical Research Council, and by the mid 1930s I was working on standardization of vitamin E tests. Sir John Ledingham, who had succeeded Sir Charles Martin as Director of the Lister Institute, considered it unsafe for experimental animals to be in Chelsea in wartime, so Sir Charles Martin, who had returned from Adelaide, took the Division of Nutrition as we had now become, to Roebuck House, Old Chesterton, Cambridge, in September 1939. There we worked on the nutritive value of all sorts of wartime foods, including Air Force meals, and continued work on vitamin values of different fractions of wheat and wheat flour, which formed the background of decisions about the National Loaf.

We returned to the Lister Institute, Chelsea, in 1946 and took the animals back at that time. The Lister now had a new Director who was not interested in nutrition, and in March 1949 the Division of Nutrition was closed. I worked for the next six months with a bacteriology group at the Lister on growth of *Proteus vulgaris* with limiting concentrations of nicotinic acid. In October 1949, I was appointed lecturer in the Department of Physiology, Kings College of Household and Social Science. At first I was teaching nutrition to students of dietetics, and this gave me time to carry on the animal work that I had been doing at the Lister Institute; Professor Yudkin allowed me to bring the Lister stock rat colony with me. These animals were the basis of nutrition research at the College until after I retired in 1968.

In 1953 the College was granted a new Charter and became Queen Elizabeth College. With Professor Yudkin I helped build up teaching of BSc Nutrition, and at the same time continued work on B vitamins with other members of the department. This included studying the effect of pyridoxine and pantothenic acid on the blood picture and on the gut bacteria of rats.

In 1957–1958 I went as visiting Professor in Nutrition to the University of Connecticut with a Fulbright Travel Grant. The Storrs Campus where I taught had a great historical interest as it was where Atwater carried out much of his early work on energy requirements. In 1959 I served as Consultant in Nutrition Education for FAO, preparing for a joint symposium with WHO in December, and as editor of the Report on Education and Training in Nutrition in Europe. In 1961 we had the International Congress of Dietetics in London for which I was Chairman of the Programmes Committee and Editor of the Report. At the end of the year I was the FAO Consultant for a special study of nutrition education at University level in six European countries, with Professor den Hartog of Holland and Professor Bigwood of Belgium. We visited France, West Germany, Belgium, Holland, Italy and the UK. In March/April 1963 I visited UNICEF in Bangkok, and the South Pacific Health Service in Fiji on my way to and from a visit to my family in New Zealand. In November/December 1964 I went on behalf of the International University Council to Sudan to help with setting up a Home Economics course in Khartoum University. This gave me an opportunity to see nutrition and food habits in Sudan, including some areas south of Khartoum.

In August/September 1966 I visited East Africa as Consultant to the Ministry of Overseas Development, to advise on a degree course in Home Economics and Nutrition at University College, Nairobi. All these jobs and travels were done incidentally to a full teaching programme. Queen Elizabeth College and the University of London gladly gave me permission as the overseas work improved my own nutrition knowledge. In 1968 I was offered a posting by UNICEF to study the training of women and girls in caring for children in South East Asia. This was too long an assignment to take on with my teaching work in London, so I chose early retirement. I then had a most

interesting few months in Indonesia, Thailand and East and West Pakistan, meeting people and their foods and uses of local supplies. Owing to trouble in East Pakistan I had extra time in Bangkok where I was able to get a visa for Cambodia to visit the wonderful remains at Ankor Wat and surrounding areas. Many of these were fragile, even in 1969, and in the horrific changes happening now in Cambodia they must have been badly damaged. This was a very fortunate experience, but there was a slight hitch in the arrangement because on my return flight from Phnom Penh to Bangkok I was off-loaded. I took a bicycle rickshaw into the town, and found a hotel. As it was Easter Sunday I tried to find a church, but when I finally talked to a French priest at a large church nearby, he said the only Anglican Pastor was with troops in Vietnam. However, he had a service in English early on Easter Sunday which I attended and found a large group of Australian engineers who were working on the Mekong River. They took care of me and I took them to the local museum and showed them the marvels of Ankor which they had not seen. Two of them came to the airport and managed to push me onto a flight, so I returned to Thailand and to the rest of my mission. The contrast between Indonesian and Pakistani people was incredible. In Indonesia, however poor the food supplies were, somehow the people found enough to eat and were always cheerful. In Pakistan people everywhere complained of lack of food, even when they were comparatively well supplied with basic necessities. Strangely, supplies of dried skimmed milk were greatly in demand. Sadly the dried milk was not always used to advantage when it was obtained. In East Pakistan (now Bangladesh) I had rather limited travel, working from Dakar, except for one amazing journey by road and ferry across the great rivers to Comilla, the centre of studies on rice culture. In West Pakistan my visits ranged through schools and markets from Peshawar on the Afghanistan frontier to Karachi, whence I flew back to Rome to report before returning to London.

When I returned to London in 1969 I still found demands for my teaching ability in preparing overseas students for special courses in nutrition which required extra physiology and background in nutrition that had not come in their regular training. In this I helped the Physiology Department at College and still carried on a little participation in research that began before I retired. In fact, in a sense I have never given up my interest in nutrition education. I have been fortunate in the teachers, colleagues and mentors with whom I have worked all my life. I began in a good chemistry department in New Zealand, then moved to Professor Drummond at University College. Sir Charles Martin, Dr Harriette Chick and many other Lister colleagues, allowed me to meet many scientists from all over the world who visited the Lister Institute and we usually met them simply as fellow workers.

The work on *Nutrition Abstracts and Reviews* encouraged this extension of knowledge of overseas workers in the field as I had an opportunity of reading their work and editing abstracts from it. With the work on *Nutrition Abstracts*

and *Reviews*, there was contact with Boyd Orr and the staff at the Rowett Institute, Aberdeen. Then in the university post the broad ideas of Professor Yudkin and the Principal of Queen Elizabeth College, who was a most understanding Head of College, encouraged the process of teaching nutrition to students for the new degree, and later to more mature seekers after nutrition and human welfare in the advanced courses we were able to develop. The endeavour to establish a Degree in Nutrition as a recognized faculty in the University led me to meet Sir Douglas Logan, the Principal of London University. Somehow between all these changes and different occupations some of my research went on, and my published papers allowed the award of DSc London 1962.

Sir Douglas Logan helped me in my other great interest, which was the welfare of women students at the residence, College Hall, Malet Street, London. College Hall was established as an independent Hall in 1882 to provide residence for the earliest women students in the University (see *Story of College Hall* by A.M. Copping). It was independent until 1962 when debts accumulated after bombing and extensive rebuilding became a nightmare. At this time I was the Executive Secretary of College Hall Council, and with a helpful Treasurer, persuaded the Council to offer the Hall to the University. Fortunately Sir Douglas Logan was entirely co-operative and took us, our debts, mortgage and all. I am still involved with College Hall and its affairs.

In my consulting work, and in other travels I learned much about people and food. Food markets all over the world tell wonderful tales of available foods and local food. In London we can now obtain summer fruits and vegetables at any time of the year at a price. In so many countries I visited this did not apply. It was particularly interesting in Sudan, where I met the unusual cereals characteristic of very arid areas, and where there were groups of people who had retained very old food habits. An expedition on my own account to Ethiopia showed me even more interesting foods, and some of my students worked there on the problems of the people and their diets. As a child in New Zealand I had often enjoyed food from the South Sea Islands brought home by a neighbour who sailed in a ship to the Islands and brought home avocado, breadfruit and other things; his own family did not like them so he gave them to us. In South East Asia, especially in Indonesia, there were many unusual fruits and vegetables and there I encountered the marvel of areas where not one, but two, and sometimes three, rice crops could be grown in a year. In Pakistan work is done by an Institute devoted to rice culture to try to improve rice production in areas where wheat cannot properly replace rice as a basic food. The use of local foodstuffs and the relation of foods to religion and social culture is something entirely basic to the understanding of good nutrition. Until about 30 years ago nutritionists in the United States tended to believe that if milk was not available the people of a country were undernourished, never

understanding that many eastern people lose the ability to digest milk after infancy. So the teaching of nutrition in any area must be related to a very wide knowledge of the subject.

In 1939 the suggestion was made that a Nutrition Society should be formed. Sir Charles Martin, Sir John Boyd Orr and Dr Leslie Harris were the prime movers and I was associated with the Society from its earliest beginnings. I have described the History of the Nutrition Society in *Proceedings of the Nutrition Society* 37, 105-38 (1978). Now I am the Society's Archivist and this, with all my activities, gives me a lively interest in nutrition in my advancing years.

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The Nutrition Society, 1941 – 1991: Presidents and Honorary Members: Their Stories and Recollections

Compiled by Elsie M. Widdowson

The Nutrition Society celebrates its fiftieth anniversary in 1991, and this book, presenting the stories and recollections of chairmen, presidents and honorary members, is published to mark the occasion. Written in a lively and popular style, the book provides a fascinating history of the careers of the leading figures in the development of the science of nutrition in the UK in the last half century. While the major scientific breakthroughs are well documented, readers will also find a great deal of information on the personalities and their formative years, research institutes and organizations that were behind these advances. All contributions include a photograph and most have been written by the presidents or honorary members themselves, but some have been contributed by other guest authors. The book should prove highly stimulating to all concerned with the science of human and animal nutrition.



www.nutrition society.org