PROCEEDINGS OF THE NUTRITION SOCIETY

The Three Hundred and Forty-fourth Scientific Meeting was held in the Barnes Lecture Theatre of the Royal Society of Medicine on 22 May 1980 when an address was given by the Retiring President Dr E. M. Widdowson

Adventures in nutrition over half a century

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My half a century begins at the beginning of the 1930s and continues until the present time. In order to set the stage, however, I must go back to the mid 1920s when R. A. McCance went to King's College Hospital as a medical student. He had already done some research work in the Biochemical Laboratory in Cambridge, and he was given a very small grant—about £30 a year—to lend a hand in the newly created Biochemical Department in his spare time. R. D. Lawrence, a diabetic, and one of the first to be treated with insulin in this country, was starting it up. The control of diabetes at that time depended on a prescribed diet with known amounts of carbohydrate in it. There were two difficulties about calculating the carbohydrate value of fruit and vegetables. First, although the figures for protein and fat given by different authors were uniform enough, the values for carbohydrate, nearly all determined 'by difference', might vary by several hundred per cent. Secondly, almost all previous analyses had been made on raw materials, but people, including diabetics, eat cooked foods, and the carbohydrate content of many foods must be altered by cooking.

In 1925 the Medical Research Council gave Dr McCance a grant to enable him to analyse cooked and raw fruits and vegetables for available carbohydrate, and he began to try to separate the available readily soluble sugars and starch from the relatively unavailable pentosans, hemicelluloses and lignins. He dealt with 109 different plant materials, each on six separate occasions, and he did all this analytical work single handed, along with his medical duties. He hydrolysed the fruit or vegetable with hydrochloric acid, then neutralized and determined the total reducing sugars. The results of the work were published as a Medical Research Council Special Report, *'The carbohydrate content of foods'* (McCance & Lawrence, 1929). This report contains not only values for the available carbohydrates in fruit and vegetables but also a long review on the food value of vegetable carbohydrates, available and unavailable, and those concerned with dietary fibre would do well to read this classic, which is as true today as when it was written over 50 years ago. Before this report was published the manuscript was submitted to E. P. Cathcart, Professor of Physiology of Glasgow University.

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Professor Cathcart was interested in the whole subject of nutrition and he at once said that from the point of view of general nutrition protein and fat were more important than carbohydrate. He thought it was a great pity that these constituents had not been determined as well as carbohydrate. Dr McCance told Professor Cathcart that he was willing to analyse foods for protein and fat, and also for minerals, if he could be provided with some assistance, but he really could not go on increasing the scope of this analytical work as well as learning to be a doctor, all alone. The result was the H. L. Shipp joined Dr McCance at King's College Hospital in 1930 and a detailed study of the composition of meat and fish began. The results of this study were published in 1933 under the title *'The chemistry of flesh foods and their losses on cooking'* (McCance & Shipp, 1933).

Shipp left in 1933 and was replaced by L. R. B. Shackleton, the plan being that fruits and vegetables should now be analysed for protein, fat and minerals, their available carbohydrate having already been measured.

Just at this time, however, I appeared on the scene. My ambition was to make biochemical research my career, but research jobs were even harder to come by in the 1930s than they are today, and I was advised to train as a dietitian. I was all set to begin the first Postgraduate Diploma Course in Dietetics at King's College of Household and Social Science, now Queen Elizabeth College, under Professor Mottram, and before the course started I was sent to King's College Hospital for training in a general hospital kitchen. I have told before how I frequently saw a thin white coated gentleman come down to the kitchen and put food into the oven or steamer, and after a while reappear and carry it away. I was told by the cook that he was Dr McCance and he was doing 'research' on cooking. One day I plucked up courage and spoke to him and he invited me to visit his tiny laboratory. There I learned about his previous studies and the one just beginning. It so happened that I knew something about the determination of carbohydrate in fruit, for I had just finished my PhD thesis on the carbohydrate content of developing apples. I told Dr McCance that his previous values for available carbohydrate in fruit were undoubtedly all too low for much of it is present as fructose, and this would have been partially destroyed by boiling with acid. Instead of being annoyed, as he might well have been, he at once asked me if I would join him and Shackleton in the new analyses of fruit and vegetables and repeat the determination of carbohydrate by more accurate methods. This I did, and we have been together ever since.

We eventually analysed all common fruits, vegetables and nuts for water, nitrogen, fat, minerals and for starch and dextrin, sucrose, glucose, fructose and the fraction which we called 'unavailable carbohydrate' but which has now been rechristened dietary fibre. Vegetables that are generally eaten cooked were analysed after cooking. The results were published in 1936 under the title *'The Nutritive Value of Fruits, Vegetables and Nuts'* (McCance *et al.* 1936). The stocks of all three publications were destroyed in a fire started by enemy action during World War II and they have been out of print ever since.

Long before this I had finished the Dietetics Diploma course. I did my Diet kitchen training at Barts under Margery Abrahams and I would like to pay tribute to her and say publicly how I still appreciate all she did for me in the early part of my career. She sent me to America, for one thing. In those days we had to travel by sea, and it seemed much more of an adventure than it does today. I met many people whose names were well known in the field of nutrition, which in the mid 1930s was already a subject in its own right in the United States. That trip was a tremendous adventure for me and I still have many happy memories of it.

While I was working at Barts I realized how much we needed comprehensive tables on the composition of British foods. The thought came to me one day that, since meat, fish, fruit and vegetables had now been extensively analysed, there remained only cereal foods, dairy products, and items such as beverages, jams and sweets. If these were also analysed we should have all the material available for making a practical set of food tables. This we did, and the first edition of *The Chemical Composition of Foods* was published in 1940 (McCance & Widdowson, 1940). The values in the tables represented about ten thousand separate determinations, each one made in duplicate, and we had no automated or electronic equipment in those days—burettes and pipettes and visual colorimeters were the tools of our trade. Yet it may surprise you to hear that in the latest edition of *The Composition of Foods* about half the values are still those we obtained by our primitive methods over 40 years ago.

Because we had all this information about the composition of foods we were in a strong position to calculate the intakes of energy and various nutrients in the diet. 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 total family's intake of energy and of nutrients 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 in this method of approach in the first number of *Nutrition Abstracts and Reviews*, published in 1931 (Cathcart, 1931). We clearly needed information about the intakes of individuals within the families, and I started my individual dietary surveys—the first two on sixty-three men and sixty-three women of the English middle class. This was when I discovered the astonishing variation in the intake of energy and of nutrients between one individual and another. There was no *British Journal of Nutrition* at that time, and when it came to publishing the results of these studies I did not know where to submit the papers. They appeared in the *Journal of Hygiene* in 1936 (Widdowson, 1936; Widdowson & McCance, 1936). I followed this up with a study of the individual dietary intakes, over a period of a week, of more than 1000 children between 1 and 18 years, and again I found that in any group of twenty or more individuals of the same age and sex there was always one getting twice as much energy as another (Widdowson, 1947). I concluded by saying 'The present work has revealed the fact that these individual differences among healthy people really exist and it has, therefore, formulated the problem which future students of individual physiology will have to solve. It is clear that much more research lies

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ahead of us before we can begin to understand why one person can live on half the calories of another and yet remain a perfectly efficient physical machine'. I wonder how far we have moved forward in the past 30 years.

Because of our knowledge about the composition of foods people came to us from time to time for help in sorting out results of dietary surveys they had made in various parts of the world. Among them was Dagmar Wilson. Dagmar, whom some of you will remember, was a most remarkable woman. She had worked with the Women's Medical Service in India for many years and among her other activities she had recorded the food intakes of various Indian communities having wheat or rice or millet as their staple cereal. She was particularly interested in the relation between diet, growth and incidence of disease and she made a special study of the cause of the rickets and osteomalacia which were prevalent in some parts of India. Her results showed clearly, as Dame Harriette Chick's had done 20 years earlier (Chick *et al.* 1923), the vital importance of sunshine falling on a bare skin. Where diets based on whole cereals were devoid of vitamin D and low in calcium, rickets did not occur if the women and children exposed their bodies to the sun; in areas where they shut themselves indoors and covered their bodies with clothing rickets and osteomalacia were widespread (Wilson & Widdowson, 1942).

Another visitor was Audrey Richards. She was an anthropologist working in what was then Northern Rhodesia, and among her investigations she had collected information about food intakes of people of the Bemba tribe. She also needed help, and again I managed to sort out her records so that we could calculate the nutrient intakes. Where we did not know the composition of an important food, samples were obtained and we analysed them. This was one of the first collaborations between an anthropologist and one who would nowadays be called a nutritionist (Richards & Widdowson, 1937). The full story was published in a book entitled *'Land, labour and diet in Northern Rhodesia'* (Richards, 1939).

All this 'research by observation' that I have so far described was adding to knowledge, but the multitude of analyses and calculations with a slide rule were very laborious and in some ways rather dull. However, another type of research was going on in our laboratory at King's between 1933 and 1938, and this was a study of experimental salt deficiency. Dr McCance began these investigations because of his interest in diabetic coma. He noticed that patients in coma had very little chloride in their urines. Addison's disease also involved salt and water problems, and he decided that the way to study all this experimentally was to create a deficiency of sodium and chloride in normal people. Salt deficiency was brought about by a salt-free diet and sweating in a hot air bath. The subjects lay on a macintosh sheet inside the warmed up apparatus for 2 h every afternoon, keeping their temperatures between 100 and 101°. The amount of salt they lost was measured by washing them down, and the macintosh sheet as well of course, with a jet of distilled water after each session, and analysing the washing; their water loss was measured by their loss of weight (McCance, 1936*a*). I was roped in to help with these experiments (Plate (a)) and they added considerable light relief to the food analysis and dietary calculations. This work gave me an insight into

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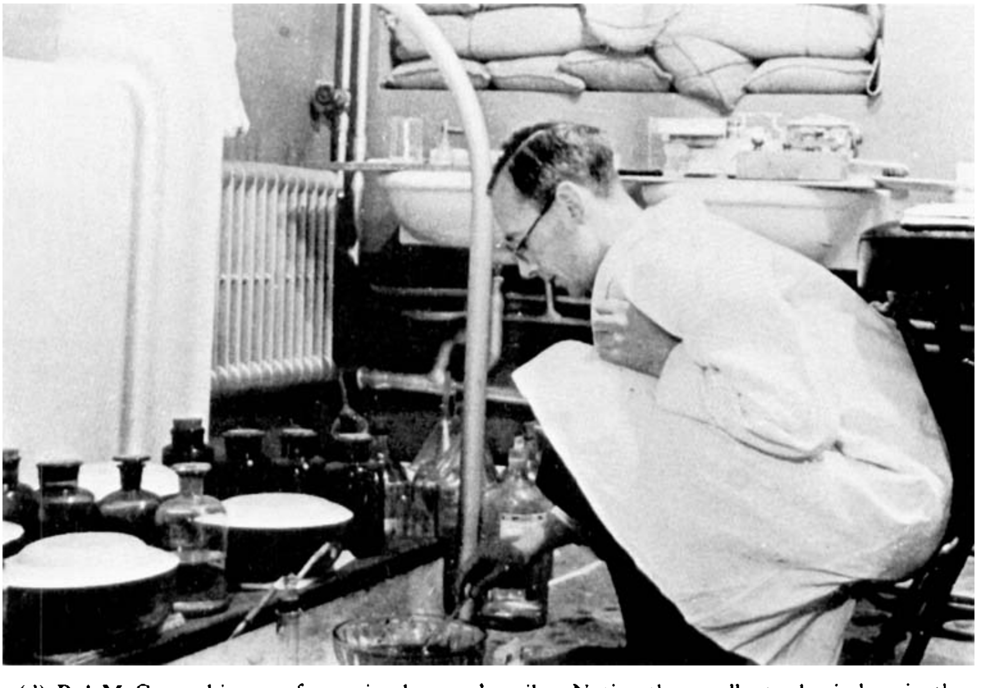
(a) Washing the salt off the skin after a session in the hot bath (1934).

(b) E.M.W. ready to start injecting herself with salts of iron, calcium and magnesium (1937).

facing page 296

(c) E.M.W. January 1940.



(d) R.A.McC. mashing up faeces in the gent's toilet. Notice the sandbagged window in the background (1940).

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experiments on man, not only their interest and importance but also their potential danger; on two occasions we had very alarming experiences but, fortunately, not utter disaster (McCance, 1936*b*).

The next thing that happened was the admission under Dr McCance of a patient, Mrs Harris, who was suffering from polycythemia rubra vera. This disease is characterized by a very high red cell count and haemoglobin concentration in the blood. One treatment was to administer phenylhydrazine. This leads to breakdown of the red cells and when this happens the iron in the haemoglobin is set free inside the body. We decided to measure Mrs Harris's intake and excretion of Fe while she was having the drug. We found to our astonishment that almost all the 5 g or so of Fe from the broken down red cells disappeared, and virtually none was excreted either by the intestine or by the kidneys (McCance & Widdowson, 1937*a*). This led us to look carefully at the literature on the absorption and excretion of Fe, and we came to the novel conclusion that the intestine excretes practically no Fe, and the amount of Fe in the body is maintained by controlled absorption; if the body contains excess iron it is unlikely that this will ever be lost except by loss of blood (McCance & Widdowson, 1937*b*). We found the same thing a few years later when we measured the absorption and excretion of Fe in a patient who was being treated by repeated blood transfusions (McCance & Widdowson, 1943). We put these ideas to the test experimentally by injecting into ourselves and other subjects, iron ammonium citrate every day for 14 d and again we found that very little of the iron injected was excreted (McCance & Widdowson, 1938). The same thing happened with medicinal doses of Fe by mouth. Some of this was absorbed, and once inside the body it did not come out again (Widdowson & McCance, 1937).

It seemed a pity to be making these studies which involved the collection of urine and faeces and duplicate samples of food and analysing them for Fe alone, so we injected calcium and magnesium gluconates at the same time as the Fe (Plate (b)) and we measured our intakes and excretions of these two elements as well. We found that most of the injected Ca and Mg was excreted in the urine and, like Fe, none of the excess appeared in the faeces (McCance & Widdowson, 1939).

In 1938, during the Munich crisis, we moved to Cambridge, and a year later the war started. We all had the feeling that we ought to do something to further the war effort and what we did was to put ourselves and other people on a diet which was based on the assumption that the importation of foodstuffs would be curtailed, and to adopt the principles of rationing that were in force between 1914–1918, but to make the actual restrictions more severe. The result of this was to produce a diet which was in many respects very restricted indeed, but it was not limited in energy and, as far as we could foretell, would not be prejudicial to health. All animal foods, fats and sugar were allowed in very small quantities, but bread made from 92% extraction flour and potatoes and other vegetables were unrationed. Eight people lived on this diet for $3\frac{1}{2}$ months. Food intakes were weighed and for part of the time urine and faeces were collected and analysed for nitrogen and minerals.

At the end of the experimental period, just after Christmas 1939, we went to the

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Lake District to test our physical fitness. Two members of the party started by cycling to Langdale, the last 100 miles over snow. During the two weeks we were there we took some very strenuous exercise, still living on our rations. One day for example Dr McCance and Andrew Huxley walked 36 miles, involving 7000 feet of ascent and descent at an average speed of 3·2 miles an hour. I managed 22 miles and ascents and descents of 5250 feet another day (Plate (c)). There was no doubt that we were physically fit and we all agreed this had been a splendid experiment. It was described in detail in an MRC report entitled *'An experimental study of rationing'* (McCance & Widdowson, 1946). All good things come to an end, however, and we came back to Cambridge and considered where our war effort should lead us next. We had not made a proper metabolic study during the experimental study of rationing, but there was a suggestion from the urinary and faecal excretions that we might have been in negative Ca balance. Our rations of milk and cheese were very small, but we had taken the precaution of having a small amount of calcium carbonate added to the flour used for making our bread. I remember showing the results to Dr Chick, and how she urged us to investigate the matter further.

We knew about Mellanby's work with puppies which had been going on for many years. He had discovered that rickets was due primarily to a deficiency of a fat soluble vitamin, but he also found that cereals precipitated rickets if the intake of the antirachitic vitamin was low. Some cereals were worse than others in their rachitic properties, oatmeal being particularly bad and white wheat flour being fairly innocuous. He next showed that the anticalcifying effect of oatmeal was partially removed either by boiling with dilute hydrochloric acid or by germination followed by heating, and he described the unknown substance as an anticalcifying toxamin (Mellanby, 1926). It was not until 1939 that this substance was identified as phytic acid (Harrison & Mellanby, 1939). Whole cereals contain most of their phosphorus as potassium and magnesium salts of phytic acid, and since calcium phytate is less soluble than the potassium and magnesium salts Ca is precipitated as phytate and its absorption prevented.

Mellanby had since 1933 been Secretary of the Medical Research Council, and he was naturally keen for us to test this out on man. So in the Spring of 1940 we embarked upon a long series of studies on healthy men and women to investigate their absorption and excretion of Ca, Mg, K and P from diets containing a great deal of bread: white bread, brown bread made from 92% extraction flour, white and brown bread containing added calcium carbonate or calcium phosphate, white bread containing added sodium phytate and brown bread made from flour from which the phytate had been removed by hydrolysis with the phytase naturally present in it. We also investigated the effect of taking additional vitamin D. We had all our meals at the laboratory and all food and drink was measured, sampled and analysed and urine and faeces likewise. The whole experiment lasted for well over a year and since all the subjects were sleeping in their own homes and going about their normal activities, a certain amount of organization and provision of portable bottles was necessary when, for example, visits to London had to be made. We had no blenders in those days so all mixing of food and faeces had to be done by hand (Plate (d)). We found that the absorption of Ca was considerably less when the diet contained much brown bread than when it contained much white bread. Adding Ca either as carbonate or phosphate to the flour improved the absorption of Ca (McCance & Widdowson, 1942*a*). Adding sodium phytate to white flour interfered so much with the absorption of Ca that the average balance became negative, whereas the removal of phytate from brown flour had just the opposite effect (McCance & Widdowson, 1942*b*). The decision to add calcium carbonate to the wartime flour was the direct result of these experiments, and the chalk is still in the flour today.

Our work on the effect of high extraction flour on the absorption of Ca brought us a minor adventure during the war in the shape of a trip to Dublin. Due to a shortage of wheat 100% wholemeal flour was being used for bread making and the incidence of rickets started to increase in the cities in Eire. Somehow Professor Jessop got to hear of our experiments and we were invited to Dublin to describe them to a group of doctors and politicians, including Mr de Valera. As a result it was decided to lower the extraction rate of the flour and later to add calcium phosphate to it and the incidence of rickets in children over one year decreased.

When the war ended in 1946 we realized that food supplies would be short in many countries, including Germany, for some time to come, so off we set in the Spring of 1946 to find somewhere in Germany where we could set up headquarters and have laboratory and hospital facilities so that we could study the effects of undernutrition on a civilian population. We found just what we needed in the I. G. Farben Industrie Laboratories in Wuppertal-Elberfeld, and in the Municipal Hospital in Wuppertal-Barmen. We were particularly fortunate in finding Dr Dorothy Rosenbaum in the hospital. Her mother was English and father German, and she spoke fluent English as well as German. She was very anxious to help us in every possible way and this was of immense value to us. A number of us settled in Wuppertal among the ruins of the bombed buildings in June 1946 and started work. Food was indeed scarce and the situation became worse during the next 6 months. The rations for an adult provided between 1000 and 1500 kcal/d. There was a considerable amount of undernutrition and oedema, particularly among the older members of the population. Professor McCance's primary interest was in the oedema, renal function and water metabolism, and in the effect of undernutrition on the volume and composition of the body fluids. Sheila Sherlock did liver biopsies and Sheila Howarth heart catheterization. These were both new procedures at that time and were rather alarming both to the patients and to the German nurses responsible for them. Dr Berridge was busy X-raying bones, gastrointestinal tracts and hearts, and Derek Russell Davis was particularly concerned with emotional disturbances. Besides making myself generally useful I made a special study of the response of twenty undernourished men between 26 and 80 years old to unlimited food. One thing this taught me was that age is no bar to rehabilitation and catch-up in weight. The 60–80 year olds put on 10 kg weight in 8 weeks on an average intake of 5600 kcal/d.

Mavis Gunther was with us for much of our time in Wuppertal and she and Jean Stanier studied the volume and composition of milk secreted by women who had had their babies in the Landesfrauenklinik in Wuppertal. R. F. A. Dean, who had worked with us in Cambridge for about a year before we went to Germany, made a detailed study of the effect of undernutrition of the mother on the size of the baby at birth and the yield of breast milk. All these investigations were published in 1951 as an MRC Special Report Entitled '*Studies of Undernutrition, Wuppertal* 1946–9'.

Now we come back to bread. Up to the post-war period there was no question in the minds of nutritionists that high extraction flour was more nutritious than white—all agreed about that—but there was a question as to whether white flour enriched with B vitamins and Fe was the nutritional equivalent of the corresponding higher extraction flour. In December 1946, while I was home on leave from Germany, Sir Edward Mellanby called a meeting to discuss the post war loaf, and in the course of it he said to me in his abrupt way, '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 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. Our results are well known. The children were under height and under weight at the outset. They gained equally rapidly in weight and height on all five types of flour: wholemeal, 85 and 72% extraction, and 72% extraction enriched with B vitamins and Fe to the amounts in 100 and 85% extraction flour. All the flours contained added calcuim carbonate. Bread provided 75% of the energy and the diets contained only 8 g of animal protein/d. The experiment lasted for 18 months and the children improved physically too, and it was impossible for the outsider to tell which kind of bread a child was eating (Widdowson & McCance, 1954). During the latter part of the experiment the BMA held its annual conference in Cambridge so I brought five of the girls, one from each bread group, from Wuppertal to Cambridge, so that the audience could see for themselves the results I was describing.

We had several other investigations going on in Germany at the same time as the bread experiment. Dean was busy feeding infants a food based on soya, wheat and barley. The children did not do very well at first, and I remember well Dr Lugg from Australia coming to visit us and telling us about the trypsin inhibitor recently discovered in soya flour; when steps were taken to remove this the children did much better (Dean, 1953).

By the time we returned to England in 1949 Sir Harold Himsworth had succeeded Sir Edward Mellanby as Secretary of the Medical Research Council, and I am sure it was Dean's experiences in Germany that led Sir Harold Himsworth to suggest that he go to Uganda to investigate the malnutrition among the children there. The possibility of improving their nutrition with plant foods was very much in everybody's mind. After a while the Infantile Malnutrition Research Unit was

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set up in Kampala, and we all know how much knowledge has come about as a result of its activities.

Now I have to back-track a little to an investigation that we started before we went to Germany and continued after we returned. This concerned the chemical composition of the human body, both of the foetus (Widdowson & Spray, 1951) and the adult (Widdowson *et al.* 1951), and we extended the investigation to cover chemical development in several other species, the mouse, rat, rabbit, cat and pig (Spray & Widdowson, 1950). This taught me that the human infant is exceptional in having 16% of fat in its body at birth, whereas most species have only 1–2% (Widdowson, 1950). The guinea pig, however, has about 10%, and one newborn grey seal, which we found recently dead on a beach on the Isle of Oronsay, Scotland, and brought to Cambridge in the boot of my car, had 9%. Fat was the great variable in the newborn as well as the adult, and it was essential to express the amounts of the other constituents per unit weight of fat free body tissue to get a true picture of chemical development before and after birth.

In the early fifties John Dickerson and David Southgate joined us and they stayed with us for many happy years. David's first job was to help with the preparation of the third edition of *'The composition of foods'* (McCance & Widdowson, 1960). We have been delighted more recently to entrust the fourth edition entirely to him and to Alison Paul (Paul & Southgate, 1978). John Dickerson extended the earlier work on the composition of the body (Dickerson & Widdowson, 1960*a*; Widdowson & Dickerson, 1960; Dickerson, 1962; Widdowson & Dickerson, 1964), but soon after he arrived he came with us on our adventure at Sandhurst. Dr Edholm came to see us one day and told us that the General who had recently inspected the cadets, aged 18½ to 20, was convinced that they were not gaining weight as they should, and this was because they were not getting enough good red meat. We agreed to collaborate with Dr Edholm in an investigation of the food intakes and energy expenditures of the cadets. We made two visits to Sandhurst, each lasting for a week or so. We found that the cadets got a large ration of meat, which they ate. The total energy from the food supplied amounted to 3714 kcal/d, but the cadets ate only 68% of this, and most of the missing 32% was accounted for by uneaten bread. They preferred to go to the canteen and buy cakes and pastries providing almost the same amount of energy as would have been supplied by the bread they did not eat. More interesting perhaps were the discoveries about their energy expenditure. We had been led to believe that they lived lives of ceaseless strenuous activity. In fact we found that they spent 8½ h out of the 24 in bed, 9¼ h sitting, some of it at lectures, and these two together accounted for 50% of their total energy expenditure. Dressing, cleaning uniforms and getting about the grounds accounted for another 28% and drill, sport and parades, which were regarded as so important in their training took up only 7% of the time and 12% of the energy (Widdowson *et al.* 1954).

Since the late 1930s Professor McCance had been investigating the renal function of infants. This arose out of his work on experimental salt deficiency, for it was discovered by Winifred Young, his house physician at King's, that newborn

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infants, like salt-deficient adults, have no chloride in their urine. He and Winifred Young pursued this and they found that the renal function of full term infants, and even more so of premature infants, was immature compared with that of adults, whether the basis of comparison was surface area or body-weight (McCance & Young, 1941; Young *et al.* 1941). They had hypotonic urines, low urea clearances and low excretion of sodium and chloride. Newborn rats, kittens, puppies and pigs were used for subsequent investigations, and the story was always the same. The renal function of the newborn appeared to be very inefficient compared to that of the adult. Although I had collaborated in several of these investigations I never had Professor McCance's enthusiasm for the physiology of renal function. However, it had always puzzled us that in spite of the failure of the newborn to excrete nitrogen and electrolytes like an adult the serum chemistry remained normal. It became clearer and clearer as time went on that growth was by far the most important influence in maintaining a normal internal environment. Inefficient though the kidneys were by adult standards they were quite capable of maintaining homoeostasis provided the animals were growing while being fed on food of exactly the right composition—that is mother's milk. Newborn rabbits, puppies and pigs for example retain 90% of the nitrogen in their food for purposes of growth, so the kidneys are never required to excrete an amount of nitrogen equivalent to more than 10% of the intake, and this they are perfectly capable of doing (McCance & Widdowson, 1957). As soon as we realized the importance of growth rather than the kidneys in maintaining a stable volume and composition of the body fluids everything fell into place and, moreover, I at once became keenly interested in the physiology of the newborn of all species.

Another person who joined us during the 1950s was Gordon Kennedy and it was he who introduced us to the idea of rearing rats in large and small groups (Kennedy, 1957). If two litters of rats born on the same day are mixed and three returned to one mother and the remainder, 16–20 to the second, those suckled in the small group get more milk per rat and grow faster than those in the large group so that by weaning at 3 weeks they are 2–3 times as heavy. We confirmed Kennedy's observation that, even though all the rats had access to unlimited food from weaning onwards, those that were small at weaning remained small, and showed no sign of the catch-up growth so characteristic of rehabilitation after undernutrition at older ages (Widdowson & McCance, 1960). We ourselves used these large and small litter rats for a variety of studies (Dickerson & Widdowson, 1960*b*; Lat *et al.* 1960; Widdowson & Kennedy, 1962), and since that time they have been used all round the world. It was they that helped Myron Winick to develop his ideas about cell division and catch-up growth (Winick & Noble, 1966).

The fifties also saw the beginning of our studies of the effects of severe undernutrition on growing animals. After many difficulties we eventually achieved our severely undernourished pigs weighing only 3% as much as their littermates when they were 1-year-old. (McCance & Widdowson, 1962). We made many investigations on these pigs, continuing into the 1960s and they formed an important part of our lives for about 10 years. Every part of their bodies was

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studied in one way or another and we extended the work to cover protein deficient animals too. We eventually kept the undernourished animals for 2 and 3 years and found that when they were rehabilitated they gained weight even though they had passed the age when the normal pig ceases to grow. They soon stopped growing, however, and the longer the period of undernutrition the shorter the period of catch-up growth and the smaller the ultimate size they attained and also the fatter they became (Widdowson, 1973).

We had always been interested in trace elements, and in the fifties we measured the absorption and excretion of iron, copper and zinc by young babies. We found that, unlike adults, breast fed babies one week old excreted far more Fe and Zn in their faeces than they received in their food, so that they were losing more than 1% of the body's total Fe and Zn each day (Cavell & Widdowson, 1964). This could obviously not go on indefinitely, and we thought the Fe might have come from unabsorbed bile and the Zn from pancreatic secretions, but unfortunately we had no way of getting any further with this at that time. We also became interested in the ability of young babies to absorb Ca and fat from various kinds of milk. We found that there is a fairly well defined limit to the amount of fat infants can absorb. This varies from baby to baby, and the level depends on the nature of the fat and the age of the baby. Breast milk fat is absorbed in the greatest quantity, cows' milk fat in the least, and a milk with vegetable and animal fat came in between (Southgate *et al.* 1969).

When the British Nutrition Foundation's Research Conference in Cambridge on Nutritional Problems in a Changing World was being organized in 1973 I became involved in the planning of the session on Infant Feeding. It was this that inspired us to analyse infant milks on sale in different European countries, and in the course of this we discovered that the Dutch formula Almiron, which is used for nearly all babies in Holland who are not breast fed, has all the cows' milk fat replaced with maize oil (Widdowson *et al.* 1974). This has such a very different fatty acid composition, with 60% as linoleic acid compared with 1% in cows' milk fat, or even 8% in the fat of breast milk, that it made us wonder what was happening to the fatty acid composition of the fat of babies living on it. We investigated this and found that it was indeed having a remarkable effect. In 3 months the linoleic acid in the body fat of the Dutch infants had risen to 46% of the total, whereas in the artificially fed British infants who were having cows' milk fat, it remained at about 1% (Widdowson *et al.* 1975). In breast fed infants linoleic acid contributed 3–4% to the total. These results are expressed as a percentage of the total fatty acids, but to appreciate the magnitude of the difference between the composition of the bodies of Dutch and British babies brought about by the kind of fat in the milk they receive we must remember that by 3 or 4 months 25% of the baby's weight may consist of fat. If this fat has 40% of linoleic acid in it, then 10% of the weight of the baby fed on the Dutch infant food will consist of this polyunsaturated fatty acid. The corresponding figure for breast fed babies is less than 1%. So the body composition of babies has been altered on a national scale in a remarkable way. The question is does this matter? Are the Dutch people any the better or worse off for having such a highly unsaturated body fat in infancy? This question is relevant to UK too, for the fat of our babies is becoming more unsaturated with the newer type of infant foods that are now being used. The composition of the depot fat may not matter very much but what about the other lipids of the body? We have had to go to animal experiments to look into this further. We have used guinea pigs because, as we saw earlier, the guinea pig is one of the few small mammals that deposits fat in its adipose tissue before birth. We fed a group of pregnant guinea pigs on a diet containing maize oil and therefore much linoleic acid, and another group on the same basic diet with beef dripping, which has very little linoleic acid in it. At birth the body fat of the young of the maize oil mothers was similar in fatty acid composition to that of the Dutch infants, while the fat in the young of the beef dripping mothers was like that of British infants. We then looked at the fatty acid compostition of the phospholipid fractions of red cells, muscle and liver and found that they were also very different (Pavey *et al.* 1976). Not only so but the myelin in all parts of the brain was affected by the nature of the dietary fat consumed by the mother during pregnancy (Pavey & Widdowson, 1980). The lipids of the brain are not immutable but can be altered quite readily at the time when myelination is proceeding rapidly. Although much of the myelination of the human brain has occurred before birth it continues for some time afterwards, and if I was able to have another adventure in nutrition I should use this ready made experiment to look further at the implications of feeding such very different fats in infancy.

As you will have gathered from what I have said I have worked on many different topics over the years. All have been connected in some way with nutrition, though nutrition as a subject did not exist when I began. I believe it is good to have several different investigations running at the same time. The trouble about being a Jack of all Trades is that you are regarded as an expert on all the subjects you have ever covered, so that I get invited to sit on expert Committees, speak at conferences and write chapters for books on topics such as bread, infant feeding, growth, undernutrition, obesity, and body composition.

If I were asked what has been most important to me in all my adventures in nutrition over half a century I would say without hesitation that it has been the companionship of my partner, Professor McCance. He has taught me, inspired me, supported and helped me all the way along, even to the preparation of this lecture. I am sure that in research, as well as in other things, 'Two are better than one: because they have a good reward for their labour' (*Ecclesiastes* 4.9). One of my rewards has been to serve as President of the Nutrition Society over the past 3 years.

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