It seems like only yesterday . . .  

A ‘not-so-brilliant’ New Zealand cadet’s tale

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I did not perform well at Wellington College. I believed what I was told, namely, that brilliant students got there without working hard, and that the not-so-brilliant, to which group I belonged, could not succeed. At university, however, I discovered that brilliant students actually worked very hard. I then did the same and found that I could compete favorably with them.

I later discovered Tsu, who, in ancient China, wrote about waging war. I read Tsu’s book and learned that the Emperor Wu won every battle as did his successors in years to come. This gave the following insight: If you know yourself but not the enemy, you will lose as often as you win; if you know neither yourself nor the enemy, you will lose; if you know yourself and your enemy, you can proceed with confidence.

I suggest that in all activities, an enemy that needs to be understood stands in the way of achievement. Combine this with the Popperian philosophy that we progress by falsification of existing concepts and you have a few tools for success. For the following article, I have selected a few highlights from a wide range of research described in some 300 contributions, and have made reference to a few key papers.

The beginning
In 1927, I joined the staff of the Agricultural Chemical Laboratory in Wellington as a cadet. Cadets then did technician work while serving their apprenticeships. They were selected from undergraduates and allowed up to seven hours off per week for lectures at Victoria University College. In general, their academic performance was superior to that of full-time students.

We had a class of 11 graduate students working toward the honors M.Sc. degree. Only three of us, all from the Agricultural Chemical Laboratory, achieved first-class honors. Two of us—Frank Denz and myself—shared the Sir George Grey Scholarship given for the highest marks overall in subjects taken for the Bachelor of Science examination. Exams were then set and marked by the University of London. Unfortunately, the cadet system has long since been abolished.

My interest in fats arose when Frank Denz and I evaluated meal from freshwater eels for pig feeding. The meal was very oily, and the project was abandoned. However, the oil, when tested with antimony trichloride, proved to be rich in vitamin A. This prompted us to test the liver oils from New Zealand (NZ) fish species, which differed from those of the North Sea.

We found these were rich in vitamin A. In our spare time we produced a paper which we showed to Chief Agricultural Chemist B.C. Aston, who remarked, “They will never publish that rubbish.” We thought there was no harm in trying. Much to our surprise and to the dismay of the chief, it was accepted by the editor of New Zealand Journal of Science and Technology, which published “The composition and vitamin A value of some New Zealand fish liver oils” [N.Z. J. Sci. Technol. 15:327–331 (1934)].

My colleague went on to study medicine in London. Returning to NZ after serving as a medical major in World War II, he took charge of the NZ MRC Toxicology Unit.

Research projects
A Jacob Joseph Scholarship at the university enabled me to take on an unsupervised research project involving the reactions between dibasic acids and glycol. The reaction was expected to result in the formation of the di-β hydroxyethyl ester of the dibasic acid or in a linear polymer in which the dibasic acids were joined by glycol. Both reactions, in fact, did occur, with the polymers forming above 160°C. The monomers when acetylated could be purified by distillation in vacuo.

The polymers were like high melting-point waxes, as reported in “Glycol esters of dibasic acids: the di-β-hydroxyethyl esters of dibasic acid” published in 1935 in the Journal of the American Chemical Society [57:115–116 (1935)]. In the case of adipic acid, had I used hexamethylene-1,6-diamine in place of glycol, I would have produced nylon, as discovered later by Carothers.

Work at the laboratory included studies on the mineral content of pasture, soil analyses, iodine surveys relating to goiter in livestock, and checking sheep dips and other products used by farmers. A cadet received good training in analytical chemistry. On graduating and becoming a chemist, one could develop projects appropriate to agricultural research and, in my case, I published jointly with colleagues or on my own.

I decided to study for a Ph.D. at Liverpool under the direction of Professor T.P. Hilditch, then the world leader in fats research. Before leaving at the end of 1934, I helped prepare 14 papers. Four dealt with bush-sickness, an anemic condition in ruminants in the pumice country of the Waikato. Two were associated with pampas grass, then used as a supplementary fodder for ruminants. Another three concerned fish liver and fish oils, including “New Zealand fish liver oils,” published in Nature [140:223 (1937)], and still another was to do with aluminum in pasture as an index of soil contamination. Two other papers dealt with geothermal heat utilization for agriculture and power production. Thus, as an inexperienced unsupervised graduate, I already had
published in leading overseas journals. Funded by a DSIR Scholarship and a University Free Passage, I prepared fish liver oils and fats from farm animals to take with me to Liverpool on the Port Fairy via Cape Horn at the end of 1934.

At the Department of Industrial Chemistry

The Hilditch research school started in 1927. By 1935, the fatty acid composition and, to some extent, the glyceride structure of many fats from a wide variety of plant and animal sources had been determined. Previously, fats had been a neglected chapter in chemistry since the discovery of their nature by Chevreul in 1823.

Hilditch used known technologies to separate fatty acids into saturated and unsaturated acids and to separate them further into fractions by fractional distillation of the methyl esters under high vacuum in a Willstatter flask in which the bulbs were packed with metal eyelets supported on a gauze plate. The composition of the fractions containing esters of adjacent carbon numbers was calculated from the saponification equivalents and iodine values. When more than two component unsaturated fatty esters were present, the mixture was shown, for example, as unsaturated C18 (-5.1 H), with the figure in parentheses indicating the mean unsaturation.

At that time, Prof, as Hilditch was generally known, was writing his first edition of the Chemical Constitution of Natural Fats, published by Chapman and Hall in 1940. The fourth edition, published in 1964, remains, as ever, the most complete description of the existing papers dealing with fatty acid and triglyceride analyses. The laboratory then had about a dozen doctoral and one or two postdoctoral students awaiting permanent positions.

In talking to Prof, I once referred to the technician as Mr. Burke. I was severely reprimanded. I had to call him Burke. Prof was friendly but against familiarity. Harold Jasperson, from the oils and fats firm of Bibbys, spoke at length to Prof during morning visits but found he could go only so far before he was discouraged.

Herbert Longenecker from The Pennsylvania State University brought with him the technique of making Fenske packing. This involved taking a glass rod inserted into a cork drilled to fit tightly onto a steel rod. The steel rod was suitably mounted so that the glass rod, when heated, could be wound into a fine spring, which was then broken into whole or three-quarter rings, packed into a 15-mm diameter column heated and equipped with a condenser and a reflux. Herbert would say to Prof, “The column has 12 plates.” Prof would reply, “Where are they? I can’t see them.” I am sure that Prof was teasing Herbert because the system was adopted by the Department.

Prof was meticulous in taking precautions against fire risk. After work, all inflammable solvents were stored outside and no burners were lit near workbenches, which carried many large separating funnels holding flammable solvents. Paradoxically, when Prof visited us about 11 a.m., he would say, “How goes it?” We would recount any successes or failures in our work. Sometimes he would then strike a match to set his pipe going before replying.

Having achieved my objective of studying the samples which I had brought with me, I was awarded a Ph.D. degree in absentia in 1937. (A Doctor of Science degree was to follow in 1950.) The results were published either jointly with Prof or on my own. My examiner was Professor Jack (later Sir Jack) Drummond of the Biochemistry Department of the University of London. Based on my thesis, he asked me but one question: “What is the hydrolysis product of sphingosine?” I replied, “You mean of sphingomyelin? In which case it is sphingosine.” I do not know whether or not the question was a mistake or deliberate. That finished the exam.

The discussion then turned to Prof Drummond’s visit to Russia where he thought that the development of biochemistry had been minimal, and that parts of the country he had seen when traveling by rail were reminiscent of what might have been expected in medieval England.

During vacations, I worked in the analytical laboratory of the British Drug Houses Ltd. (BDH) in London. We had already supplied BDH up to 144 gallons per annum of ling (Genypterus blacodes) liver oil produced from a small-scale plant set up by my father. The oil was 15 times richer in vitamin A than cod liver oil. The demand was much greater than we could ever hope to supply. The opportunity was used to determine spectrophotometrically the vitamin A content of my samples. I am very grateful for all of these experiences.

Returning to the Agricultural Chemical Laboratory

When I returned to the Agricultural Chemical Laboratory, I learned R.E.R. Grimmett had become chief agricultural chemist. As his adviser on chemical matters, I could select appropriate research projects.

This happy turn of events did not last. Instead, our laboratory came under the power of J.F. Filmer, who was director of the Animal Research Division and responsible for its closure. He had come from Western Australia to solve the facial eczema problem in sheep and cattle which occurred in certain parts of the North Island around 1939 with high levels of mortality. The basic problem was a toxin which caused liver damage, and was associated with high temperatures and rainfall following a drought resulting in lush growth.

Filmer was certain that the toxin was produced in the pasture. Many chemists including myself worked
endlessly on the problem without success. Several decades later, Royd Thornton showed that a fungus, believed to be *Sporidesmin bakeri* but later identified as *Pithomyces chartarum*, was involved. Eventually the toxic substance sporiesmen was isolated by Nobel Prize winner Dr. Synge, who came to NZ for a period, and by Dr. E.P. White.

At that time, we were studying the fats of the bacon pig in reference to carcass quality. The composition of pig fats affects hardness and therefore meat quality. Pigs fed soybean meal produce soft pork. Earlier theories suggested that hardness depended on the temperature of deposition, hence kidney fat was harder than outer back fat. Callow in 1935 developed the growth rate theory according to which the hardest fats were found along the shoulder where the growth rate was at a maximum. Our work, published as “Studies on the fats of the bacon pig with reference to carcass quality 3: the relation between growth rate and chemical composition of pig depot fat,” [Journal of Agricultural Science 35:39–43 (1945)] indicated that neither of these theories was tenable.

In 1938, Lovern showed that the oil content of the freshwater eel in the Northern Hemisphere increased with its length. Subsequently, in 1944, McCance considered that the oil content could not be foretold from the length. We examined our NZ species *Anguilla aucklandii* and *A. australis* from a variety of sources and distinguished between migrants and nonmigrants. The oil content of migrant eels, regardless of their length, was more or less constant as recorded by McCance, but in agreement with Lovern, the oil content of the nonmigrant (immature) eel increased with its length. That the oil content of the migrant eel is more uniformly distributed. Amusingly, McCance, then head of Dunn Nutrition Laboratory at Cambridge, wrote to say he was a bit put off by eels. Studies on forage grasses and clovers, meanwhile, showed total lipid content of about 7% on a dry matter basis, some 14% being waxes and phospholipids. The acetone-soluble remainder comprised triglycerides and free fatty acids. Little was then known apart from the work of Smith and Chibnall in 1932, who found that the main components were octadecadienoic and α-linolenic acids in the ratio of 1:3, with small amounts of palmitic acid. We generally confirmed these results except that we found much higher levels of α-linolenic acid and credited their low value of 0.2% phospholipid on a dry matter basis to enzymic action. Freshly cut pasture dropped into boiling alcohol gave values of 1.5–1.7%.

In our later studies, Weenink showed that the acetone-soluble fraction of pasture lipids consisted of galactolipids and not of triglycerides. A detailed fatty acid analysis of the acetone-soluble fraction of rye grass was then carried out.

From the lipids of rape leaf (*Brassica napus* L.) used as sheep fodder, we discovered that hexadecatrienoic acid constituted 17% of the total fatty acids. Its structure showed it to be the C16 homolog of α-linolenic acid not previously found in plants [“The constitution of pentadecatrienoic acid from the glycerides of rape (*Brassica napus* L.) leaf,” Biochem. J. 49:503–506 (1951)].

The Agricultural Chemical Laboratory closed down in 1946 and its staff transferred to the Ruakura Animal Research Station. The staff included young promising research workers such as Peter B. De la Mare, F.D. Collins, and Walter Metcalf. McMeekan’s specialty was animal production with little interest in chemical careers. De la Mare and Collins did routine nitrogen determinations in pasture and in feces, while Walter Metcalf was to isolate estrogen from sheep’s urine, a very poor source. The research workers did not stay long. Peter De la Mare went to London to study under Ingold the mechanism of chemical reactions and then became professor of organic chemistry at Bedford College, University of London.

Establishment of a fats research lab
Unable to continue with fish oil studies at Ruakura, I switched to DSIR Dominion Laboratory to take charge of a fats research section. Britain was then imposing a food-labeling order that required every pound of butter to be labeled with its vitamin A content. As I was the only person experienced in the determination of vitamin A, Dr. Marshden (later Sir Ernest), head of the DSIR, negotiated a position for me as officer in charge of the Fats Research Laboratory to investigate NZ fats of actual or potential economic value, concentrating at first on butterfat. Fortunately, Britain withdrew the food labeling order from butter. The persistent conscientious work of staff including especially that of my deputy, Roy P. Hansen, ensured successful outcomes. Our staff of 20 included ten chemists.

We began with a detailed analysis of the seasonal variations in the fatty acid composition of butterfat. The samples came from the Rangitaiki Plains Dairy Co. and were representative of the bulk churning from numerous herds in the district over a one-year period. The analyses in triplicate were subjected to statistical evaluation. From July through November, the content of C6–C14 saturated acids increased from 18.6 mole percentage to 26.9 mole percentage and thereafter slowly declined; the variations in content of these acids were balanced mainly by C18 unsaturated acids. The maximum content of lower saturated fatty acids in November was coincident with the greatest growth of pasture and hence with the maximum production of “acetate,” the precursor of the lower saturated fatty acids. Seasonal changes in ewe milk fat followed the same pattern as for butterfat even though the lactation period ended in November, compared with July for cows. Thus, changes in milk fat composition are not due to stage of lactation but are diet-related.

Oxidation with potassium permanganate in acetone of concentrates of the C18 methyl esters of butterfat’s unsaturated fatty acid fractions yielded an oily fraction and not the expected traces of methyl stearate. This led to our discovery in 1951 of branched-
chain fatty acids in natural fats; see "The branched-chain fatty acids of butter fat 1: The isolation from butterfat of branched-chain fatty acids with special reference to the C17 acids" in the Biochemical Journal [51:207 (1951)]. As described in The Chemical Constitution of Natural Fats and other textbooks, apart from isovaleric acid in dolphin oil, natural fats are made up of n even-numbered-carbon saturated and unsaturated fatty acids.

We established by isolation of the pure acids the occurrence in ruminant fats and other natural sources of the iso series of branched-chain fatty acids, odd- and even-numbered carbon from C13 through to C18 and the (+)-anteiso C13, C15 and C17 acids as reviewed in Annual Review of Biochemistry [25:101–122 (1956)]. In addition, we found multibranched-chain fatty acids including a C20 tetramethyl acid later identified by Dutch and Swedish workers by mass spectrometry as phytanic acid related to phytol. The branched-chain fatty acids constituted about 1.5% of the total fatty acids. The result was treated with some skepticism by Hilditch. As the waxes from bacteria were known to contain branched-chain fatty acids, he wrote to me to suggest that the source could be of bacterial origin.

Also, by 1954, we had established the presence of an n-odd-numbered fatty acid in natural fats by the isolation of pure heptadecanoic acid in mutton fat. This was followed by the isolation of a range (C3–C25) including some monounsaturated representatives from ruminant and other fat sources, as reviewed in the previously cited article in Annual Review of Biochemistry. These results were before the advent of gas chromatography which enabled the presence of such acids to be readily confirmed.

Our successes, in part, were due to efficient laboratory fractional distillation columns. I devised such columns by winding two closely coiled springs—one of copper wire and the other of stainless steel—around a central core of sealed Pyrex tubing. The packing was inserted into a close-fitting tube attached to the distilling flask. On careful heating with a blow torch and the application of a slight negative pressure, the outer tube shrank onto the metal coils. The copper coil was dissolved in nitric acid. The packing provided low back pressure and low hold-up, enabling separation of branched-chain isomers.

In 1952, over a period of six months, I visited fats research laboratories in Britain, Europe, and the United States. I also attended the Second International Conference on Biochemistry in Paris. Between sessions, I held discussions with Prof. Raymond Reiser from the Biochemistry Department at Texas A&M at College Station and also with Prof. Walt Lundberg, head of the Hormel Institute in Minnesota. These experiences increased my opportunities in the United States to visit organizations and give lectures.

In addition, I was invited by Prof. H.P. Kaufmann to give a plenary lecture at the German Society for Fats Research Annual Conference. There I met Prof. H. Dam of Copenhagen, who received a Nobel Prize for his work on antihemorrhagic vitamin K. Not only did I visit his laboratory but he also took me through Denmark to Elsinore where some years later I attended a special meeting on the occasion of his 70th birthday.

Our discovery of branched-chain fatty acids as well as our unraveling of the nature of ruminant fats began to make an impact. I was invited to write a chapter in the previously cited Annual Review of Biochemistry on the "Chemistry of the lipides" as they were then called. At the time, only leading researchers in the United States, England, and Europe had been invited. It appears that no other work carried out in NZ has been thus recognized.

To explain the unique presence of substantial amounts of trans and cis isomers of oleic acid, trans conjugated isomers of linoleic acid, and high stearic acid contents in ruminant fats, we turned to Reiser's 1951 discovery that sheep rumen contents hydrogenated linolenic to linoleic acid. Prior to Reiser's discovery and contrary to what was then believed (Armstrong and Allen, 1924; Hilditch, 1947), namely, that pasture-fed horses lay down hard fat like that of mutton or beef tallow, we found that the depot fats were oily and contained about 20% linolenic acid. We concluded therefore that the relative absence of dietary linolenic acid in ruminant fats was because linolenic acid is either...
metabolized or hydrogenated [Bio-
chemistry Journal 46:80–85 (1950)].
We found that in the depot fats of
nonruminants, such as those of the rat,
pig, horse, rabbit and pheasant, trans
acids were absent unless present in the
diet whereas, consistent with ruminal
hydrogenation, ruminants invariably
possessed such acids amounting to
as the real dietary fat of ruminants and
adds that to the endogenously pro-
duced fat comprising oleic acid with
25% palmitic and 7% stearic acid, the
composition of ruminant fats is large-
ly explained [The effect of rumen on
dietary fat, Nature 175:1129 (1955)].
Using fat-free sheep rumen con-
tents, we found that linoleic acid was

Although these examples are entirely correct,
they do not invalidate the Shorland hypothesis,
since neither the pig nor the horse can be
classified as a ruminant.

3.5–11.2% of the total fatty acids. Certain marsupials, though monogas-
tric, set aside part of their stomach to
accommodate rumen microorganisms,
according to Professor Waring. Through his courtesy, we obtained
marsupial depot fat samples which showed that those of the quokka
and the wallaby, possessing ruminant-like
functions, did indeed yield high levels
of trans acids (up to 21%) but had also relatively high levels of α-
linolenic and linoleic acids, suggest-
ing lower hydrogenation activity. Trans acids were absent from the
depot fats of other marsupials, such as
the opossum and koala, which did not
possess rumen-like activity [The trans
unsaturated fatty acid contents of
ruminants and nonruminants,
We compared the fatty acid
compositions of the dietary pasture
with those of rumen contents and of
ruminant depot fats. Whereas pasture
lipids constituted mainly unsaturated
fatty acids (76.5% with 58.9% α-
linolenic acid), the rumen lipids con-
tained mainly saturated fatty acids
(72.1% with high levels of stearic
acid), the unsaturated fatty acids
(27.9%) having been extensively
hydrogenated to form mainly
monoenoic acids including trans
(11.9% of total fatty acids) and posi-
tional isomers of oleic acid. If one
regards the fats of the rumen contents
not the main ruminal hydrogenation
product of α-linolenic acid as reported
by Reiser, but was mainly stearic acid
with trans and cis isomers of oleic
acid and traces of dienoic acid (mainly
trans dienoic acid).
To explain the fatty acid composi-
tion of animal fats, Hilditch and
Lovern in 1936 used the evolution
theory as a background, noting the
gradual simplification in fatty acid
composition as one proceeds from the
lower forms of life to those that are
more highly organized. In particular,
fish depot fats covered a wide range
of fatty acids compared with those of
highly evolved land mammals. In 1952,
I put forward a different theory in
"The evolution of natural fats"
[Nature 170:92 (1952)], that the fatty
acid composition is determined by
dietary fat and stage of development
of their fat metabolism as follows: (a)
In the early stages of evolutionary
development, the organism, as exem-
plified by fishes, is unable to make fat
from the nonfatty constituents of the
diet but largely derives its depot fat
directly from the dietary fat; (b) In the
next stage, as exemplified by amphib-
ians, reptiles and most mammals,
there is, in addition to deposition of
dietary fat, an ability to synthesize fat
from the nonfatty components of the
diet. Such synthetic fat is of simple
composition, composed of palmitic,
stearic and oleic acids; (c) In the final
stage, as exemplified by ruminants
and certain marsupials, the influence
of dietary fat is largely reduced
because unsaturated fatty acids in the
diet are hydrogenated by rumen
microorganisms to give a fatty acid
composition resembling that of the
endogenous fat.
Hilditch accepted my conclusions
regarding progressive changes in the
capacity of animals to produce com-
paratively simple synthetic fats. How-
ever, pasture-fed horses containing
notable amounts of α-linolenic acid
apparently absorbed from the grass
glycerides seem not to fit in my gener-
alization. The disagreement between
my self and Hilditch is summarized by
Deuel [Lipids, Interscience Publishers,
New York and London, 1955] as fol-
lows: "Hilditch, in commenting on the
Shorland article, advances the fact that
the composition of pig fat from an
animal fed on whale oil may resemble
the latter fat to a great extent. More-
ever, reference is made to the fact that
α-linolenic acid appears in the fat of
gas-fed horses; this apparently origi-
nates from the grass glycerides.
Although these examples are entirely
correct, they do not invalidate the
Shorland hypothesis, since neither the
pig nor the horse can be classified as a ruminant."
Fish liver oil research, as reported
in some 20 publications, was followed
in the early 1940s by full-scale pro-
duction of liver oil by Karitane Prod-
ucts in Wellington and Greenwells
Ltd. in Auckland. An important factor
was to prepare vitamin A concentrates
by molecular distillation of fish liver
oils, a process previously practiced
only in the United States. I greatly
improved the efficiency of the falling
film still by disturbing the film surface
with a stainless steel helix (NZ Patent
93490, filed April 9, 1947), enabling
the commercial production of vitamin
A concentrates which were sent to the
Nicholas Propriety in Melbourne.
Meanwhile, DSIR Physics and Engi-
neering Laboratories manufactured a
spinning disc still based on the East-
man Kodak design.
Distillation Products then wrote to
Greenwells stating that it held world
patents on the preparation of vitamin
A concentrates and asked for the
that the menu included wild game in the form of pheasants traditionally tenderized by being hung for a considerable time. Professor Kaufmann, president of the German Society for Fats Research, again invited me to give a plenary lecture.

In 1966, I was again overseas. With the late Sir Charles Burns, I helped set up the Nutrition Society of New Zealand. I became secretary, later chairman, and then a life member. I attended the Seventh International Congress on Nutrition held in Hamburg where our society had added its name to the other nutrition societies of the world so that nutrition could be represented on the International Council of Scientific Unions. I gave a paper on the fate of phytanic acid (the metabolite of phytol associated with chlorophyll) in rats. Whereas 0.1 g/rat/day was metabolized with no obvious adverse effects, 0.5 g/rat/day was fatal in the course of a few days. At this level, there was a marked effect on fatty acid metabolism that varied in different tissues. In the kidney, the lauric acid content increased markedly but in the liver the palmitic and stearic acid contents were reduced to about 2% of the total fatty acids. It was shown that part of the phytanic acid was converted to pristanic and lower branched-chain fatty acids.

Following the United Nations publication in 1968 of “International Action to Avert the Impending Protein Crisis,” I was misled into turning wool into food protein, a feat which we achieved. When incorporated into conventional recipes, the tasteless protein showed that the product was readily acceptable. However, the United Nations assessment was wrong; it was not a protein but a calorie shortage problem.

Fats research in NZ is not generally supported by universities. It was therefore remarkable that the ICI medal of the NZ Institute of Chemistry, awarded to that investigator who had by way of research during the past three years contributed most to the advancement of chemistry, should come to me the year after its inception in 1949 and later to my colleagues, Drs. R.P. Hansen and L. Hartman, in succession. Further recognitions followed with FRSNZ, and the premier award of the Royal Society of New Zealand, the Hector Medal in 1955 which rotates over six areas of science, each of which comes up at six yearly intervals. That year I not only contributed my invited chapter to the Annual Review of Biochemistry, as previously mentioned, but also gave the prestigious 11th Liversidge Lecture of ANZAAS in Melbourne, Australia, being the third New Zealander to be thus invited. Many other awards followed, including the Order of the British Empire some ten years prior to my retirement. Even more rewarding was to have Fulbright Scholars work in our laboratory. They included Professors Walter Dunkley and Lloyd Smith from the University of California at Davis and Professor Dave Cramer from Colorado State University.

Dissolution of the Fats Research Division followed its change of name to the Food Chemistry Division. This came following my retirement after 40 years of service and the desire of the Applied Biochemistry Division at Palmerston North to do fats research. Dr. Hansen was put in charge of a fats section and carried on unabated for several years prior to retirement with investigations into animal fats and notably those of earthworms—a remarkable source of branched-chain fatty acids. Fats research slowly ceased to exist as did DSIR.

Retirement or a new career?

My postretirement career thus far covers some 30 years. An appointment at Victoria University of Wellington as honorary lecturer to the Department of Biochemistry and then as honorary fellow in the School of Biological Sciences has provided me office space, facilities, and numerous contacts enabling me—without funding—to publish in local and overseas journals about 100 research papers and reviews. I also have been honorary editor of New Zealand Science Review.

On considering my nutrition lectures in the Biochemistry Department, I decided that much of the contemporary approach to human nutrition was

(continued on page 1005)
ridiculous, being based largely on assertions lacking in experimental evidence. The result was my article "Human nutrition is an ass" published in 1989 in the Proceedings of the Nutritional Society of NZ [14:180–182 (1989)]. In addition, data assiduously collected worldwide in FAO Food Balance Sheets were not interpreted as to their significance. This gave me a rich source of information for my invited review, "Is our knowledge of human nutrition soundly based?" published in the World Review of Nutrition and Dietetics [57:126–213 (1988)]. I categorized the diets of countries into meat–milk, wheat high- and low-calorie, maize, and rice groups. From life expectancy data of Keyfitz and Fluger, I showed that some developing countries with relatively high infant mortality on low-calorie wheat or maize diets had abnormally high life expectancies after the age of 60 years. My graphs appear on the outside cover of Volume 57 of World Review on Nutrition and Dietetics.

Human protein requirements have been based on conjecture or erroneously on rat models; valid work on humans, such as that of Hindhede, has been ignored. Consequences include: (a) through adoption of Voit's unfounded assumption that 12 ounces of beef/day were necessary for the army, Germany lost World War I through starvation; (b) in 1968, the United Nations published "International Action to Avert the Impending Protein Crisis" to meet a nonexistent deficiency; and (c) the division of proteins into first class and second class has no bearing on humans. In practice, protein intake has varied from the Hindhede potato diet of 21 grams per day to 377 grams per day in the Eskimo diet without incurring obvious adverse effects.

In 1917, federal authorities made recommendations concerning dietary fats and sugar. Limits for fat were first announced in 1977. By 1988 it was conceded that chronic diseases are linked to diets too high in fats and too low in dietary fiber. That Eskimos in their native state are free from chronic diseases has been overlooked. Their diet included nearly 50% fat calories and no dietary fiber, showing that fat composition which includes high levels of n-3 polyunsaturated fatty acids is the key factor. The acceptance of recommended dietary allowances as a guideline to good nutrition is likewise on very shaky ground.

My World Review publication brought together my thoughts on nutrition and unexpectedly drew favorable comments from the late Hugh Sinclair, then director of the International Nutrition Foundation at Cambridge.

Prof. Raymond Reiser, in writing an invited chapter on meat fats and fatty acids for Advances in Meat Research, invited me to collaborate. I was honored to be the only non-American contributor [Meat fats and fatty acids, Advances in Meat Research 6:21–62 (1990)].

From October 1974 to February 1976 I was a senior Fulbright Scholar and visiting professor in the Department of Food Science and Nutrition at Michigan State University. I lived in Owen Hall with 1,000 postdoctoral students, half of whom were non-American. At my farewell in Owen Hall, I received a special bon voyage message: "A distinguished scholar, writer, scientist and Fulbright fellow has served unselfishly and energetically as O.G.A.'s Cultural Educational Committee's chairman for the past two years." The Owen Hall Quarterly to which I contributed articles had a caricature sketch of me on the front cover of its January 1975 issue. I had been accepted as "one of our fellow resident Owentites whose wit had been a delight to many, possibly vexing to some and of curiosity to others."

My main project during this visiting professorship was to determine the effects of dietary fat and vitamin E on the lipid composition and stability of veal during frozen storage. Because of my input, Professor Al Pearson decided to put my name first. The calves were fed corn oil or coconut oil as milk fat replacers until nine weeks of age. The corn oil-fed animals showed poor growth. Supplemental vitamin E retarded oxidation in muscle tissue but not in omental or perinephric fatty tissue. Inhibition of oxidation by vitamin E was not clearly evident in intact tissue systems as in the extracted lipids.

From my involvement in a Royal Society of New Zealand report on coronary heart disease (CHD) in 1969, I found that the only highly positive but ignored result was that of Morrisson (1960) who studied 100 patients, of whom 50 remained on their high-calorie prethrombosis diet and the other 50 received a restricted 1,500 kcal/day diet which included lean meat, poultry, and fish. At the end of the 12-year period, all members of the control group were dead but 38% of the trial group had survived.

I then studied the role of fats in relation to CHD. My publications show that the nutritional and physiological roles of polyunsaturated fatty acids and their prostaglandin and leukotriene metabolites are often ignored or poorly understood by health authorities in their advice to the public. I reported to the Royal Society of New Zealand on the matter.

In reply, Sir John Scott, chairman of the National Committee on Nutrition, wrote, "I believe Dr. Shorland is correct in stating that a number of individual people and a number of organizations which appear to speak with authority on nutritional matters exhibit inadequate understanding of lipid chemistry and the physiology of lipids so far as human health and disease process are concerned . . . . Dr. Shorland's two-page document accurately portrays my own understanding of the situation. The way out of the situation is to maintain vigilance of the type Dr. Shorland had exhibited for decades."