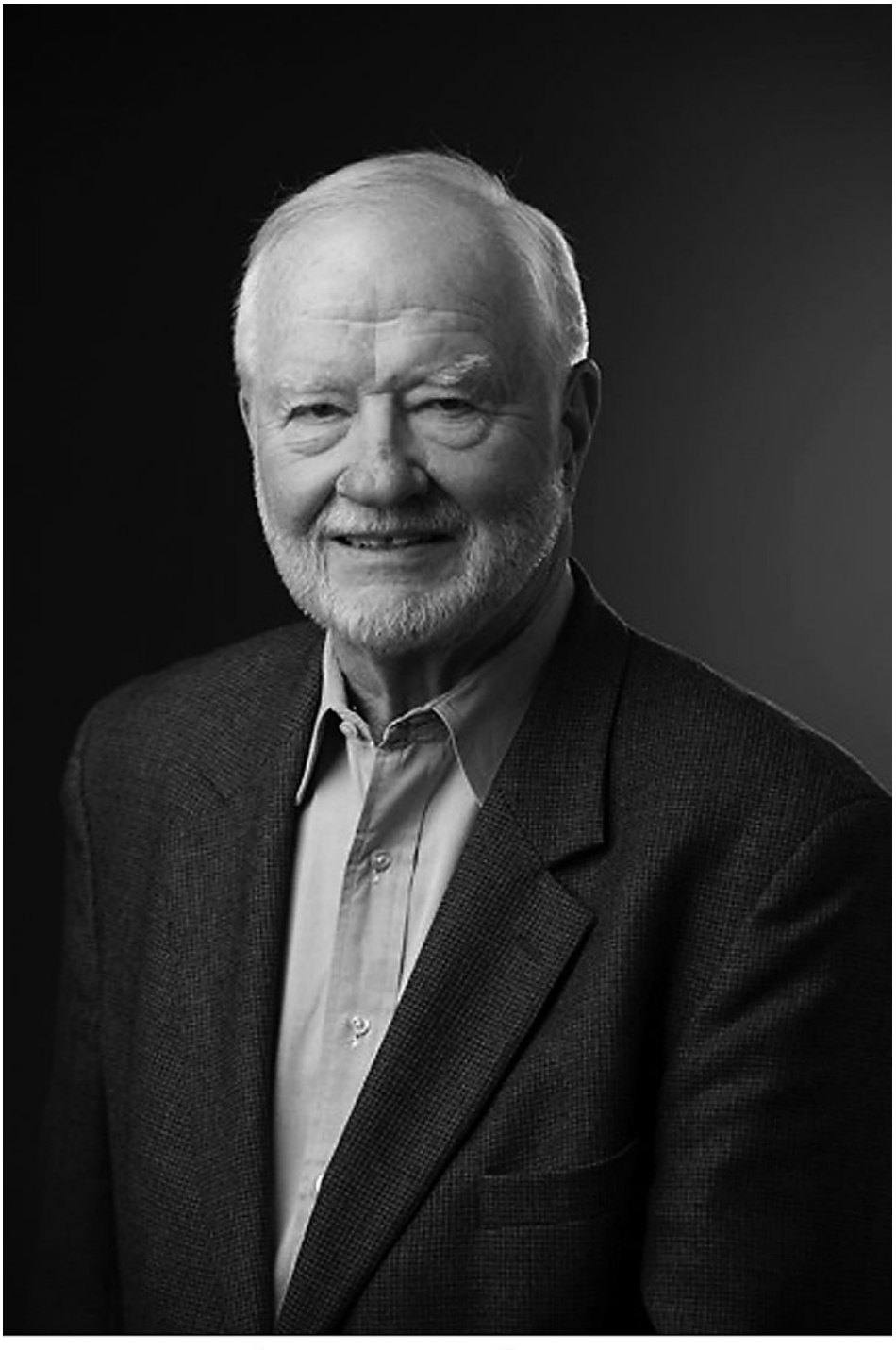

An Unexpected Life in Nutrition

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Abstract

In this biographical article, I describe the evolution of my career in nutrition from an early period as an animal nutritionist interested in amino acid metabolism and genetic variation in nutrient requirements to an involvement in human nutrition and international public health. The career changes were in some respects a mirror of the evolution of nutritional science in my lifetime. I spent my entire career at Cornell University in what I think of as three distinct phases. As a researcher and teacher in the Poultry Science Department, I was able to do research in animal nutrition and witness the rapid industrialization of the production of poultry meat and eggs, helped by the findings of the era of nutrient discovery in nutritional science. Later I had the opportunity to lead the reorganization of human nutrition at Cornell during a period when research in nutritional science turned away from identifying new nutrients and became increasingly concerned with the roles of diet and chronic disease. During this period my research focus evolved as I became interested in aspects of international nutrition problems, particularly the influence of parasitic infections on child health and nutrition. I also became involved nationally in nutrition issues through participation in organizations such as the National Nutrition Consortium, the Food and Nutrition Board, and National Institutes of Health study sections at a time of great ferment in nutrition about the relationship of dietary patterns to health. Finally, I became provost of Cornell University and involved in the administration of a major research university. I describe my career in the context of my origins and early education springing from life on a sustainable family farm in rural Illinois.

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INTRODUCTION

It never occurred to me while growing up on a farm in rural Illinois that I would become deeply involved in the science of nutrition and spend my life at an Eastern university. But I have had an unusual opportunity to be part of nutritional science and to observe its evolution for nearly 60 years. I began my academic career as an experimental animal nutritionist, but over time my focus changed as I became interested in human nutrition and its role in public health. My career also included a variety of administrative roles that expanded my skills and perspective. Perhaps my experience can help those beginning in the field now to recognize the ways in which careers in nutritional science can and do evolve.

ORIGINS

I am the seventh of eight children of a farm family from near Rochelle, Illinois. My father was a Norwegian immigrant who arrived in the United States in 1904 at the age of 14 after completing the schooling mandated in Norway at that time. My mother was the daughter of German immigrants who came to this country right after the American Civil War. She completed high school, which was sufficient education for her to become an elementary school teacher in a one-room rural school near her father's farm. In 1929 my parents bought a 170-acre farm, where I was born in 1931.

By today's standards, the farm would be considered a model sustainable farm. Although we had no electricity until 1938, the farm produced most of the food for the family—crops of corn, oats, and hay that were fed to cattle and lambs brought from Western ranges to be fattened prior to being sent to the Chicago stockyards. We had three or four cows for our milk, cream, and butter and always had a few pigs. The manure went back on the land. My mother cared for a flock of chickens that provided eggs and meat. We exchanged excess eggs for staple foods such as flour and sugar at a local grocery store. We tended an apple orchard and a large garden. We preserved food by home canning and by home curing ham and bacon. Our 170 acres produced sufficient food and income to support our large family.

I worked on the farm as I grew up, and I drove a tractor from the age of 8. The farm is no longer in the family. Today it produces corn and soybeans, has no livestock, and is farmed as part of a large commercial operation. When I visited the farm recently, there was no garden or orchard; the barn, silo, corn crib, and feedlot were gone; and there was no food in sight—a prime example of the vast change in Midwestern agriculture in the past 60 years.

Education was important in my family, perhaps because of my mother's background as a teacher. Six of the eight children completed college, three obtained PhDs and the other three earned master's degrees. I attended grades 1 through 8 in the same one-room rural school where my mother had taught. When I graduated from the eighth grade, the total enrollment of the school consisted of two eighth-graders and one second-grader. In earlier years there had been as many as 18 pupils among the eight grades. I graduated from Rochelle Township High School in 1949 as one of a class of 66. I was a good student (second in my class academically), and I was senior class president.

In the fall of 1949 I enrolled at the University of Illinois in Urbana-Champaign, which seemed to me to be the only college to consider. I won a county scholarship to study agriculture. This paid my tuition of $40 per semester for four years. I worked summers for a local contractor as a carpenter, constructing farm

buildings in the local area and saving enough to pay most of my college expenses. My parents provided very little financial help. This experience is in sharp contrast to the challenges faced by today's students and families, as support for public higher education has waned and public universities have become much more expensive. The University of Illinois at Urbana-Champaign listed 2011–12 tuition for Illinois residents as $14,414 to $19,238, depending on the major, with total estimated costs of up to $33,028. It is hard to imagine working one's way through college today without substantial family help or without incurring considerable debt.

I initially followed a general agriculture curriculum, having little experience beyond the farm and no clue as to what I wished to do with my life at that stage. It seemed only natural for a farm boy to study agriculture. Fortunately for me, my older brother, Robert Nesheim, who had served in the United States Army in World War II, enrolled as a graduate student in animal nutrition at Illinois in 1950. I had grown up with Bob, an admired big brother. Observing his graduate study opened up possibilities that were new to me. He was working with B. Connor Johnson on studies of vitamin B12 requirements in baby pigs. Some of his research required use of a purified diet designed as an artificial milk. I would help him from time to time as he made late-night trips to the lab to feed his pigs, and I was exposed to the processes and excitement of research. At that time I also had a part-time job helping to summarize research records for Jim Craig, a geneticist in the Animal Science Department.

These experiences led me to consider graduate study for myself, and I changed from general agriculture to a science-based curriculum. This required me to catch up on chemistry and math courses that were missing in my early college education. Unfortunately, the curricula for agriculture students in those days included special basic courses in chemistry and biology that were considered good enough for farmers. I had to retake some more comprehensive introductory courses to provide a better background for graduate study. This experience always reminds me of the folly of underestimating one's students and short-changing them by providing watered-down instruction that limits options. In the spring of 1953, I entered graduate school at Illinois to pursue a master's degree in animal nutrition, working with Gene Becker and Stanley Terrell in the Animal Science Department.

Illinois was a wonderful place to get a basic knowledge of nutrition and biochemistry. Carl Vestling taught basic biochemistry, Herbert Carter and Lavelle Henderson taught advanced courses, and nutrition pioneer W. C. Rose was still on the campus, though retired. Tom Hamilton and Harold H. Mitchell taught a graduate-level course in comparative nutrition that met five days a week for two semesters. They covered the whole field of nutrition in depth during that year. I took the course during the last year in which Mitchell taught before he retired. His was an extraordinary course in classical energy metabolism and nutrition that has stayed relevant and important to this day. Hans Fisher was in the same class that year, and he relates a similar experience in his own career reflections in the 2009 volume of the *Annual Review of Nutrition*. Mitchell had received his PhD in chemistry from the University of Illinois in 1915. He provided a perspective on nutrition dating from the days of Atwater, Armsby, Benedict, Osbourne, and Mendel that provided wonderful insights for a young graduate student. At Illinois, I studied the tryptophan requirements of young pigs and received an M.S. degree in animal nutrition in 1954.

In July that year I reported for duty in the United States Air Force. I was in the ROTC as an undergraduate and was obligated to spend two years on active duty. I was stationed at Wright-Patterson Air Force Base in Dayton, Ohio as a staff officer for an engineering group that was standardizing equipment used to service aircraft from the U.S., Canadian, and British air forces. Not much nutrition was involved, but I learned a lot about screw threads and grease nipples.

While in the Air Force, I made plans to continue studying animal nutrition when my

military service was complete. My advisor at Illinois, Gene Becker, was a Cornell PhD. He advised me to apply to Cornell to get a background in poultry nutrition as a means of broadening my experience to make me more employable in the animal feed industry upon graduation. I was admitted to Cornell's doctoral program in animal nutrition in September 1956. At the time I enrolled, I had no idea I would spend the rest of my life associated with Cornell.

ANIMAL NUTRITION YEARS

The science of nutrition from the early days of the twentieth century was preoccupied with the identification and characterization of the essential nutrients. By the early 1950s, the nutrient-discovery era was coming to an end as vitamin B12 and folic acid were finally characterized. But the identification, structure, synthesis, biochemical function, and requirements of the essential nutrients were still the predominant themes of nutrition research. I had been fascinated at Illinois as Carl Vestling unveiled the wonders of the Krebs cycle and the roles of vitamins as cofactors in intermediary metabolism in my first biochemistry course. Most nutrition teaching programs everywhere at that time had laboratory courses where various vitamin- or mineral-deficiency diseases were produced in rats or chicks. Richard Forbes taught such a laboratory course at Illinois and Clive McCay taught a similar course at Cornell. They constructed diets made up of highly purified ingredients to produce these deficiency diseases. In some cases, nutrient-deficient diets had to be fed to pregnant rats or flocks of laying hens to produce offspring with nutrient stores low enough to allow deficiency symptoms to develop. Few such courses exist today, and most modern nutrition graduates have never seen a nutritional deficiency in an animal. In my view, without such training, they have little appreciation about how hard it is to produce some of these nutrient deficiencies and how rare nutrient deficiencies are in diets made up of a variety of unprocessed ingredients.

In the nutrient-discovery era, findings were rapidly turned into practical use in feeding animals, which promoted the development of commercial feeds. The feed industry supported the intensive raising of young chickens, turkeys, and pigs on simple diets made up primarily of corn and oil-seed meals supplemented with essential nutrients.

I arrived at Cornell when the leadership of nutrition on campus was changing. Leonard Maynard, the key figure in nutrition at Cornell since 1915, had just retired. McCay was still active in the Animal Science Department. Richard Barnes had arrived on campus to replace Maynard in leading the School of Nutrition. Strong animal nutrition programs were centered in the departments of Animal Science and Poultry Science.

The nutrition group in the Poultry Science Department was led by Leo Norris (Maynard's first graduate student). His colleagues were Milton Scott and Fred Hill, who were both former graduate students of Norris. This group had long been involved in the discovery of the essential nutrients, the characterization of deficiencies, and the estimation of requirements in avian species. These themes still characterized much of the ongoing research in the department. In 1956, Norris's group was trying to identify a component (later identified as zinc) present in the ash fraction of several food materials. When the ash was added to a purified diet, it stimulated chick growth. Hill was interested in energy metabolism and fat utilization in chickens.

Scott was investigating a factor that prevented an edema, termed exudative diathesis, that developed in young chicks fed a semipurified diet containing an unusual protein source, torula yeast. The condition could be prevented by adding to the chicks' diets either vitamin E or some unidentified component of brewer's yeast. Scott had studied these dietary conditions because Klaus Schwartz at the National Institutes of Health (NIH) was trying to purify and identify a substance in brewer's yeast, which he called Factor 3, that would prevent liver necrosis in rats fed a similar purified diet low

in vitamin E. In collaboration with Schwartz, Scott found that concentrates of Factor 3 would prevent exudative diathesis in chicks.

I chose Scott as my PhD advisor, and he strongly suggested that I work on the vitamin E problem. Vitamin E–deficiency diseases in young chicks were very complex. It appeared that three distinct syndromes—exudative diathesis, muscular dystrophy, and encephalomalacia—could be produced in chicks, depending on the composition of the diet. All of these syndromes could be prevented by adding vitamin E to the diets. The muscular dystrophy, however, was also prevented by the amino acid methionine; the exudative diathesis could be prevented by that unknown factor in brewer's yeast; and encephalomalacia could be prevented by synthetic antioxidants.

Some aspects of this complex situation became clearer one day in 1957 when Schwartz mailed Scott three small glass vials containing tiny amounts of white powders. He suggested that we find out whether the material in the vials could prevent exudative diathesis in chicks. It was my job to do the experiment. Much to our surprise, none of the chicks fed diets to which the Schwartz materials had been added showed any sign of exudative diathesis. Schwartz then revealed that the three vials contained sodium selenite, seleno-methionine, and elemental selenium. Given the earlier history of selenium as a toxic element, this was an astounding revelation.

The Schwartz discovery came at the same time as Patterson and his colleagues at Lederle Laboratories made a similar finding. The Patterson and Schwartz groups published the results in the same issue of the *Proceedings of the Society for Experimental Biology and Medicine* in September of 1957 (25, 26). The data from my experiment with the Schwartz material are included in the Schwartz paper. The thrill of having played a small part in the discovery of an essential nutrient hooked me on the rewards of nutrition research. My PhD thesis research involved further study of the selenium–vitamin E relationship, and I published my findings in 1958 (21).

As I completed graduate study, I considered what I wished to do next. I had never considered a university career as an objective. Brother Bob was now working in the feed industry at the Quaker Oats Company, and I had assumed that some career in industry was my most likely next step. In any case, few university positions were open at that time. When my graduate program was nearly complete, I began interviewing for positions at several companies. As I was about to decide among several offers, my department chair, J. H. Bruckner, told me that the department wanted me to join the faculty as an assistant professor. This is an example of how academic hiring was done at that time. There was no search committee, I had no formal interview, and to my knowledge no other candidates were considered. I accepted the offer and joined the Cornell faculty on February 15, 1959 with an appointment in the Poultry Science Department as well as in the Graduate School of Nutrition.

The Cornell Poultry Science Department offered many advantages for nutrition research at that time. Experimental facilities were good, and large numbers of young chicks were available to use in experiments at low cost. Department funds supported the research as well as graduate student stipends, and the Graduate School of Nutrition provided funds for a research technician to support my work. I had light teaching loads and good colleagues doing interesting nutrition research. But as a new faculty member in the same department as my graduate mentor, I felt that I needed to develop an identity and a research program of my own. Doing this meant that I had to abandon the vitamin E and selenium field, which at Cornell was the domain of Milton Scott.

I decided to examine the effects of other amino acid deficiencies, including arginine, on the development of muscular dystrophy. Chickens, as well as other birds, excrete nitrogen as uric acid rather than urea, and they do not have a functioning urea cycle. Arginine is not synthesized and is an essential amino acid. Purified diets for chickens that contain casein as the source of protein are deficient in arginine,

and this amino acid must be added to make the diet complete. Since dietary methionine affected the development of muscular dystrophy in chicks fed vitamin E–deficient diets, I wondered if arginine had the same effect. I found a marked variation in the response of individual chicks to an arginine deficiency. This intrigued me and made me wonder whether there could be large genetic differences in some nutrient requirements.

I had a department colleague, Professor F. B. Hutt, a distinguished geneticist who along with his colleague R. K. Cole had selected strains of chickens resistant or susceptible to Marek's disease, a viral tumor that could cause high mortality in poultry flocks. During our discussions of genetic variations in nutritional needs, Hutt suggested that we try to find out whether his distinct strains of chickens would respond in similar ways to diets deficient in arginine. Much to our surprise, chicks from the strain susceptible to the virus tumor grew only half as well on the arginine-deficient diet as those from the resistant strain. When arginine was adequate, they all grew at the same rate. We published these observations in *Science* in 1962 (20).

On the basis of these observations, Hutt and I decided to see if we could select for chicken strains with different arginine requirements. Over the next few years we developed strains of chickens with low or high arginine requirements, and we studied the metabolic differences that distinguished the two groups (10). We obtained NIH support. Along with several of my students over the next few years, we studied arginine metabolism in chickens. Eventually, we figured out that we were really studying an arginine–lysine relationship. Excess lysine, which is high in a protein such as casein, induced elevated levels of kidney arginase when fed to chicks (17). The strains that had a high arginine requirement did not degrade excess lysine well and displayed elevated blood levels of lysine and kidney arginase activity (30). We studied the biochemistry of these differences, but the techniques that are now available to study the genetics did not yet exist.

At the time we carried out these studies, nutrition scientists were interested in how much genetic variation might occur in nutrient requirements. The studies with animal models provided an opportunity to examine this variation experimentally. In further studies, we explored the degree of variation in other requirements for other nutrients. In most chickens, the response to nutritional deficiency showed little variation among individuals, but the conversion of tryptophan to niacin showed considerable variation. We developed strains of chickens that differed in their ability to convert tryptophan to niacin (15). Not surprisingly, it became evident that nutrients with more complex metabolic fates showed the most variability in the response of animals to a deficiency.

In my time in the Poultry Science Department, my graduate students and I worked on a variety of other problems. We studied the effects on digestion of trypsin inhibitors in soybeans and other beans. We carried out studies on essential fatty acids, bile acids, choline requirements, and sodium, potassium, and chloride relationships. I worked with Roland Leach, now at Pennsylvania State University, who developed strains of chickens to study a bone disorder of growing chickens—tibial dyschondroplasia—which affected certain strains of rapidly growing chickens used in the broiler industry (12).

Growing chickens were a useful experimental animal for nutrition studies. Chickens grow rapidly, making them highly sensitive to nutrient deficiencies. They readily consume highly purified diets. Large numbers were available for study at low cost. It was easy to design well-controlled experiments with large numbers of experimental birds. The disadvantage was that chickens are not mammals. They were not considered a good model for nutrition problems relevant to humans; therefore, results obtained were out of the mainstream of nutritional science.

In 1965 Leslie Card, head of the Animal Science Department at the University of Illinois, asked me to help him revise his book *Poultry Production*, first published in 1914 by

W. A. Lippincott. The book's authors became Lippincott and Card in 1934, and Card and I published the tenth edition in 1966 (1). Introductory courses in poultry production were common in agricultural colleges at that time, and the book was quite widely used. Having coauthored the textbook, I was asked to teach the introductory course for several years in the 1960s, early in the era of industrial-scale poultry production. I continued working on the book through its twelfth edition in 1979, when I asked my colleague Richard Austic to be a coauthor. He continued the book with me as a coauthor through the thirteenth edition in 1990. The book has not been revised since, but it had a remarkable run of some 76 years.

In 1971, I collaborated with my colleagues M. L. Scott and R. J. Young to publish *Nutrition of the Chicken* (27). We published this basic text in three editions, the last in 1982. During those years, this was the standard reference for feeding chickens.

Ironically, the current local-food movement has brought back interest in small-scale backyard chicken-raising as a reaction against industrial-scale agriculture. Some of the older out-of-date books on raising poultry are now back in vogue. This is an interesting development given that I started out on a family farm raising backyard chickens but began working in poultry science as industrialization came to full flower.

CHANGE OF DIRECTION

In 1966, with the help of a Rockefeller Foundation Fellowship, I took a sabbatical leave at the University of Cambridge in England to work with Kenneth Carpenter. Since I had done my graduate study at Cornell, I wanted to gain perspective by working in a new environment. I also wanted to expose my young family to an overseas experience. Carpenter was interested in protein quality and had developed techniques to examine amino acid availability and the effects of heat treatment on proteins used for animal feed. He had been especially interested in meat meals and fish meals. I helped carry out some digestibility studies on heat-damaged proteins (18). We studied the extent to which poor-quality proteins could adequately support growth when they were fed at higher levels.

An unexpected benefit of the sabbatical leave was meeting a young parasitologist, David Crompton, who was working in the Molteno Institute for Parasitology at Cambridge. Crompton was interested in Acanthocephala worms inhabiting the intestinal tract of animals. He postulated that the behavior of these parasites in the gut depended on the nutritional environment at the specific sites at which they lived. Our common interest involved aspects of intestinal physiology, and we considered that host-parasite interactions might be modified by host nutrition. Crompton made plans to come to Cornell in the summer of 1968 and again in 1970. We began studies to examine the habitat of intestinal parasites at various sites in the intestinal tract. At Cornell we initially investigated amino acid and bile acid gradients at various levels of the intestinal tract of ducks. We chose ducks because they hosted the Acanthocephala parasite *Polymorphus minutus*. This began a 30-year research collaboration and lifetime friendship that changed both of our research directions.

I spent a year-long sabbatical leave at the University of Cambridge from 1972 to 1973, this time working in the Molteno Institute with David Crompton. I was supported by an NIH senior postdoctoral fellowship and an overseas fellowship at Churchill College, Cambridge. During this period, we showed that we could affect the growth and location of the parasite *Moniliformis dubius* in the rat intestine by manipulating the carbohydrate content of the host's diet. In this anaerobic environment, the parasite obtained its energy through glycolysis. By using carbohydrate-free diets in the host rat, we could manipulate a worm's growth, change its location in the intestine, and determine which monosaccharides it could metabolize (19). These studies confirmed our idea that intestinal parasites are sensitive to intestinal ecosystems that can be manipulated

by changing the host's nutrition. I returned to Cambridge one more time in 1983 for another five months, this time as a visiting fellow of Sidney Sussex College.

A NEW OPPORTUNITY

When I returned from Cambridge in 1973, I was still in the Poultry Science Department, although a few years earlier I had begun to teach part of a senior-level course in general animal nutrition offered by the Animal Science Department. By this time, the poultry industry had moved to full-scale industrialization. The major nutrition problems associated with large-scale production of poultry meat and eggs had largely been solved. Few poultry farms remained in New York State, and I had little sense that applied research in the area would help the small farmers of the state to survive. I was ready to broaden my professional horizons, and I considered leaving Cornell.

Cornell's nutrition programs were in great ferment. Its Graduate School of Nutrition had been transformed from an intercollege unit with faculty drawn from other university departments to a small but very strong unit with about 15 core faculty appointed directly to the School. Richard Barnes had come to Cornell in 1956 to become Dean of the School, replacing its first director and Cornell nutrition pioneer, L. A. Maynard. Barnes had recruited several senior nutrition researchers and added young nutritional biochemists to the faculty to strengthen Cornell's biochemistry program. In the meantime, the College of Home Economics had changed its name to the College of Human Ecology in 1969. A new Dean was looking to strengthen its various programs, which included a Department of Human Nutrition and Food. There also were strong nutrition programs in the Animal Science and Poultry Science Departments. In 1971–1972, several faculty committees had examined how to combine the various nutrition programs into a single unit, but there was little consensus or faculty enthusiasm for a merger. In 1972, Barnes asked to be relieved as Dean.

Just as I returned from Cambridge in 1973, the University administration made the decision to combine the Graduate School of Nutrition and the Department of Human Nutrition and Food into a single academic unit, reporting to both the Dean of the College of Human Ecology and the Dean of the College of Agriculture and Life Sciences. That fall, the University made an unsuccessful attempt to recruit a Director for the new program from outside of Cornell. In early 1974, after meeting with various faculty groups, I was asked to become the Director of the new unit called the Division of Nutritional Sciences. Given my changing research interests, the offer came at a key time. I could make a major change in my career but without the disruption of moving to a new institution. I accepted the position, and the new unit became official on April 1, 1974.

The task of forming the new nutrition program at Cornell was formidable. The Graduate School of Nutrition faculty members who were transferred to the new unit were mostly male and oriented to biochemistry and metabolism or to international and public health. They were concerned about being associated with a former home economics program. The faculty of the Department of Human Nutrition and Food were all female. They had diverse research interests but were strongly oriented to undergraduate teaching and extension work. They felt unappreciated by their male colleagues from the School of Nutrition. Also, some of the existing nutrition faculty in the Animal Science and Poultry Science Departments were given joint appointments in the new Division.

The combined resources of these units made the Division the largest academic unit devoted to nutrition in the United States at that time. But the cultures of the two major units were very different and very unlike that of the family-oriented Department of Poultry Science. It was a challenge to establish a new identity that did not merely incorporate one unit into the other. Fortunately, several faculty vacancies made it possible to recruit new faculty who did not have loyalties to either of the old organizational structures.

The Division had responsibility for undergraduate teaching, including a dietetics program, cooperative extension work in nutrition throughout New York State, and graduate and research programs in human nutrition. Substantial resources were available from New York State, some endowments, and the income from research grant overhead. These resources mainly remained with the Division and provided its programs with more autonomy than most other departments at Cornell.

In the early 1970s, nutritional science was in the midst of a major transition. The age of nutrient discovery was essentially over, and the role of dietary patterns in the development of chronic disease was becoming a central issue. Faculty were beginning to view nutrition not as a specific discipline, but rather as a broad field of study that explored the relationship of the food environment to health. With the large faculty of the Division, it was possible to maintain strength in nutritional biochemistry but also to add strength in social sciences and economics in order to support population- and community-based research. We recruited faculty with expertise in nutritional epidemiology, toxicology, physical anthropology, and food economics to complement the interests of faculty trained in biochemistry and nutrition who formed the core of most nutrition departments. Recognizing that many of the world's nutrition problems were in the developing world, we expanded our international expertise. Research on populations plagued by malnutrition was considered essential to meet humanitarian goals, provide training for students, and gain access to important areas of inquiry. By the end of 1975, we had recruited several faculty to develop these new directions. My own teaching shifted to an upper-level course in the biochemistry and physiology of human nutrition.

NUTRITION AND PARASITES

The transition to a human nutrition unit also transformed my research. Although my laboratory continued to work on arginine and lysine metabolism for several years, we also began to develop the work on nutrition and parasitic infections. The work that I began with David Crompton could now shift from a concern about the influence of host nutrition on the parasite to an investigation of the influence of the parasite on host nutrition. And Crompton's interests could turn from laboratory models to study human helminth infections in the field. One of my graduate students, Lani Stephenson, wanted to study infection with the common roundworm *Ascaris lumbricoides* on the nutritional status of its human hosts. The prevailing public health view was that roundworm infection was not a major public health problem in spite of its wide distribution. At Cornell, we had a facility where we could study growing pigs infected with *Ascaris suum*, a parasite nearly identical to the human roundworm. In these initial experiments, Stephenson showed that roundworm infections reduced protein and fat digestion and markedly affected the intestinal morphology of young pigs (29). Working with Crompton and Michael Latham, she carried out a study of *Ascaris* infection in school children in Kenya, demonstrating that deworming with an anthelmintic drug resulted in improved growth of previously infected children (28). Stephenson and Latham, along with Crompton, continued to work on the relationship of nutrition to infection, principally with schistosomiasis and hookworm in Kenya. Crompton came to Cornell for various periods each year to teach a special course on parasitic infections to graduate nutrition students.

In 1981, I was asked to go to Burma (now Myanmar) to help design a deworming study that was to be supported by USAID. I asked Crompton to accompany me, and we helped colleagues at the Burmese Medical Research unit undertake a very large study of the effect of deworming on the growth of school children. We continued to do work with *Ascaris suum* in pigs at Cornell. One of our findings was that the infection reduced intestinal lactase in pigs (7). Later, in Panama, we observed that worm-infected children were lactose intolerant and that they recovered their ability to digest lactose after deworming (2). In Indonesia we studied

the influence of *Ascaris* infections on the ability to use plant food beta-carotene as a source of vitamin A (11).

We extended work to other parasitic infections as our graduate students examined the effect of iron supplementation on malarial infection in laboratory mice as well as in school children in Papua New Guinea (8).

I found my animal nutrition background very helpful in appreciating the problems of appropriate design, execution, and interpretation of human studies. I often had my graduate students carry out controlled animal studies with rats or pigs related to the field research they were going to conduct. It gave them an opportunity to test hypotheses and gain an appreciation of the importance of appropriate design as well as the level of detail needed to make inferences about the problem they were studying.

My students at Cornell and Crompton's students at Cambridge and later in Glasgow continued to collaborate on studies of nutrition–parasite relationships throughout the period I was Director of Nutritional Sciences at Cornell. After our work and that of others showed that parasitic infections were an important part of the environment of childhood malnutrition, we felt that insufficient attention was being given to this problem and that more effort should focus on controlling parasites as a means to reduce the malnutrition that affected so many of the world's children. Crompton and I decided that we needed to bring these results to a wider audience. We organized a symposium on parasitic infections at the annual meeting of the American Institute of Nutrition in 1982 and three international meetings on this topic cosponsored by the World Health Organization (WHO). These meetings, held in 1984, 1988, and 2000, brought together researchers and public health workers to consider the consequences of parasitic infections, the relationship of nutrition and infection, and strategies for control. We published papers from these meetings in three books (3, 5, 6).

The meeting in 2000, held in Indonesia, published the Bali Declaration, which in part stated that:

> Benefits accrue from deworming in childhood growth, development and cognition, in adult productivity and in the course and outcome of pregnancy. The Bali Conference declares that the World Health Organization, as a matter of urgency, should call on governments of the developed countries to contribute to relieving poor people worldwide of this unnecessary burden of disease.

In May 2001, the World Health Assembly adopted a resolution calling on member states to take steps to reduce the burden of intestinal helminthiasis and schistosomiasis in school-age children (31). It was gratifying to see the broad recognition of an important public health problem that we had investigated for many years. Crompton and I reviewed much of the research on parasitic infection and nutrition for the *Annual Review of Nutrition* in 2002 (4). WHO now has a program for neglected tropical diseases that includes the soil-transmitted helminths and schistosomiasis, and there is now significant recognition that the control and elimination of these diseases needs international support (9). WHO reported in 2008 that more than 205 million children were treated worldwide for soil-transmitted helminths that year (32).

PUBLIC SERVICE

Becoming Director of Nutritional Sciences at Cornell gave me considerable visibility in the greater nutrition community, and I became involved in a number of activities outside of Cornell that included committees at the U.S. Department of Agriculture and the NIH. The nutrition community was undergoing some major changes in the 1970s and 1980s. The Senate Select Committee on Nutrition (the McGovern Committee) had published *Dietary Goals for the United States* in 1977, and the first edition of *Dietary Guidelines for Americans* was released in 1980. These guidelines differed from standard micronutrient-based advice and instead were based on the idea that macronutrient patterns may influence chronic disease.

I became a member of the Board of the National Nutrition Consortium in 1976 and chaired the Board in 1982. This was a consortium of major nutrition societies of the time: the American Institute of Nutrition, the American Dietetic Association, the American Society of Clinical Nutrition, the Institute of Food Technologists, and the Society for Nutrition Education. The consortium was originally formed with support from the Nutrition Foundation (an industry-funded group) to comment on major nutrition-related issues of the day using the expertise of the professional nutrition community.

In 1980, the Board of the Consortium issued a statement outlining guidelines for a national nutrition policy. It endorsed the principle that guidelines for healthy diets should deal with intake of nonessential nutrients such as saturated fats, sugar, cholesterol, and fiber as well as the essential vitamins and minerals. The statement went on to say that the evidence for these guidelines was limited. It also indicated that the board was not fully in agreement about the value of guidelines in preventive medicine. The cautious statement was a reflection of the brewing controversy about the Dietary Guidelines in the nutrition community.

The National Nutrition Consortium eventually went out of business in the next few years, largely due to lack of financial support but also to the problem of scientific societies making statements on controversial issues. Though the consortium failed, its history is a cautionary note for professional societies. Attempting to develop consensus statements on topics for which research is ongoing is inevitably political and divisive for societies that were originally founded to promote the publication of research and exchange of ideas. The recent foray of the American Society for Nutrition into front-of-package food labeling is a good example of the inevitable problems of conflict of interest that can arise (24).

In 1983 the Food and Nutrition Board of the National Research Council (NRC) issued a report entitled "Toward Healthful Diets." The report maintained that the evidence supporting the dietary changes—to avoid too much fat, saturated fat, cholesterol, salt, and sugar—recommended in the 1980 Dietary Guidelines was limited. Instead the Board suggested that research was too premature to recommend major changes in U.S. dietary patterns to prevent chronic disease.

The report was controversial and led to major changes at the NRC and the Food and Nutrition Board. The Board's industry liaison group was disbanded amid charges of untoward industry influence on its recommendations. The NRC appointed a new executive officer to the Food and Nutrition Board along with several new members and a new Chair, Kurt Isselbacher, from Harvard. I was one of the new appointees in 1983, and I served on the Board for nine years.

We were immediately confronted with a difficult problem. A committee appointed by the pre-1983 Board had drafted a new tenth edition of the *Recommended Dietary Allowances* (RDAs) for publication in 1985. The report was reviewed by the post-1983 Board and the National Academy of Sciences (to which the NRC belonged), and it was rejected, partly because it reflected the views of those who questioned the evidence for the 1980 Dietary Guidelines and the relationship of some nutrient levels to chronic disease (13). I opposed the rejection and supported working with the committee to revise the draft, as I felt it would result in a major rift among members of the nutrition community. As it turned out, the rejection was highly controversial, and the NIH, which had funded the development of the new edition, was especially concerned.

The issue was eventually resolved when the NIH and the National Academy agreed that a new committee of the Food and Nutrition Board would revise the manuscript. I was on the committee to revise the original draft along with Richard Havel, Doris Callaway, Walter Mertz, and Joan Gussow. Amid threats of lawsuits and charges of copyright violations and plagiarism (14), the tenth edition of the RDAs was published in 1989. This was the last of the single-volume RDA reports from the Food and

Nutrition Board. The more recent *Dietary Reference Intake* reports now take up more than a foot of space on my bookshelves.

The controversies of the early 1980s reflected the differences in opinion as to whether dietary macronutrient patterns had a role in the development of chronic disease. Was good nutrition defined primarily by the essential nutrient content of a diet or should it include the additional need to define appropriate macronutrient levels provided by patterns of relatively unprocessed foods? By 1989, the Food and Nutrition Board had published a report, *Diet and Health*, that outlined evidence for food and chronic disease relationships, and the Surgeon General published a report on nutrition and health that helped resolve some of the controversies in the field at that time.

I chaired the 1990 Dietary Guidelines committee that drew heavily on the evidence reviewed by these two reports. Because the committee was concerned with maintaining consistency of dietary advice for the public, the 1990 guidelines were similar to those issued in 1980 and 1985, but with some more quantitative recommendations. The 1990 committee was the source of the still current alcohol recommendations that allow two drinks a day for men and one drink for women. We thought we would be accused of blatant sexism for the recommendation, but it was supported by evidence. At the time, the Dietary Guideline committees actually wrote the guidelines themselves as opposed to the present practice whereby the advisory committee produces a report but the government agencies write the actual guidelines. By 2005, it seemed the guidelines had become too political to leave in the hands of an expert committee.

From 1981 to 1986, I served on the NIH Nutrition Study Section, which I chaired from 1983 to 1986. One of the things that struck me about my time on the study section was where proposals were coming from. More grant proposals were submitted by cardiologists, endocrinologists, and oncologists than by traditional nutrition scientists. The proposals reflected the new interest in nutrition and chronic disease and a change from the traditional studies of metabolism and nutrient requirements that used to characterize nutrition research. I also served on several special study sections to examine proposals on nutrition and cancer that at that time were handled separately from the Nutrition Study Section.

I was elected to the Council of the American Institute of Nutrition (AIN) in 1977 and was President of the Society from 1985 to 1986. The AIN and the American Society for Clinical Nutrition had some difficult relationships during that period, reflecting the general turmoil in the nutrition community at that time. The AIN was growing very slowly while other nutrition societies such as the American Dietetic Association, the Society for Nutrition Education, and the American Society for Parenteral and Enteral Nutrition showed rapid growth. I worked with the AIN council to make meetings and society membership more reflective of the range of interests and approaches to nutrition that then characterized the field. The AIN was considered a society for those interested in nutritional biochemistry or laboratory-based studies as opposed to more population-based nutrition research. In response, the AIN reorganized its meetings and recruited members that embraced nutritional epidemiology and community- and population-based approaches to nutrition.

In an effort to help nutrition departments respond to the changing nutrition scene, I obtained a $5 million grant from the Pew Charitable Trusts of Philadelphia for a program we called the Pew National Nutrition Program. We asked universities to submit proposals to bring together various nutrition interests on campuses to respond to the new directions of the field. With advice from an advisory committee, we awarded several large grants to universities that were trying to unite their resources in traditional nutrition and food science areas with those in medicine and other fields interested in nutrition research.

The Pew program had some successes, but changing academic directions and organization

in universities is not easy and probably required even more resources than we could provide. The program organized the first Nutrition Department chair meetings at the Experimental Biology meetings, and these have continued to the present day. It took many more years before the AIN and the American Society for Clinical Nutrition could unite to form the present American Society for Nutrition.

ANOTHER CORNELL CAREER

By the mid 1980s I had been Director of Nutritional Sciences at Cornell for more than 13 years, and I began to feel the need to move on and let someone new provide new ideas and energy to the Division's programs. After 13 years, more than half of the Division's faculty members were new, and the program had grown dramatically in terms of students and research activities. Work was underway to remodel our existing facilities and to build a new laboratory building. One of the problems that results from administrators holding positions for a long time is that they tend to look back and focus on what has been accomplished, whereas a person new to the job tends to look ahead and imagine what needs to be done.

I began to get offers to be considered for positions outside of Cornell, and I seriously considered a move. But at that time I was asked to join the central administration at Cornell as a Vice Provost for Planning and Budgeting. I accepted the offer and began my new duties in August of 1987, thus beginning a third career at Cornell with new colleagues and new challenges. Two years later I was named Cornell Provost, which is the position of chief academic and chief operating officer for the Ithaca campus. I could not help but reflect on the contrast between the rural, one-room school I attended for eight grades and the large Ivy League university I would now need to manage. As Provost, I was fortunate to serve with Cornell President Frank H. T. Rhodes. He is a remarkable man and was an exceptional Cornell University president for 18 years. Because of the demands on campus as Provost, I greatly reduced my involvement in nutrition-related activities outside of Cornell.

Being Provost of Cornell gave me the opportunity to know the issues and culture of an enormous range of academic disciplines and to work with a broad range of distinguished academics. I was Provost for six years, from 1989 to 1995. In the early 1990s the United States was in a recession that resulted in stressed budgets and intense pressures for slowing the growth of the costs of higher education. I had to reduce the growth of Cornell's budget and tuition rates while maintaining Cornell's policy of need-blind admission and financial aid for undergraduates. This meant making some difficult choices as to important programs, and I worked closely with the deans of the 12 colleges and schools on the Ithaca campus to reduce expenditures but maintain the academic strengths expected at Cornell. Ironically, while I was Provost, the Department of Poultry Science was closed and faculty distributed to other University units. Cornell embarked upon a major fund-raising campaign in that period. We were confronted with the rising expectations of a diverse student body, which resulted in building occupations and intense discussions with students and faculty about expanding ethnic studies programs. It was both a challenging and exciting time.

When Frank Rhodes retired as Cornell President in 1995, I decided to leave the administration as well. In the early 1990s, laws took effect eliminating mandatory retirement at age 70 for academics. As Provost, I had been concerned that this would result in an aging faculty and would reduce the opportunity to bring new ideas and energy to the University faculty through new appointments of young scholars. I did not want to be one of the aging professors that I was concerned about as Provost, so I retired from Cornell and became Professor of Nutrition Emeritus and Provost Emeritus in 1997.

During my retirement years, I have had a number of opportunities to keep active academically. President Bill Clinton appointed

me Chair of a Commission on Dietary Supplements. This was mandated by the Dietary Supplement Health and Education Act of 1994, and our Commission spent two years preparing a report on the Act. I chaired an Institute of Medicine study on the risks and benefits of seafood, and I had an opportunity to travel and lecture in Japan and Indonesia at the invitation of former students and colleagues. I spent 12 years as Chair or a member of the Board of Trustees of the Pan American Health and Education Foundation, which assists the Pan American Health Organization with its work in the Americas.

After my first marriage ended in divorce in the mid 1970s, I married Diva Sanjur, a Professor of Nutrition at Cornell who was originally from Panama, and we collaborated on a number of studies on nutrition and parasitic infections there. Our retirement plans included spending time in Panama, especially to escape the depths of Ithaca winters. Sadly, shortly after her retirement in 2000, she became ill and died in 2002, a loss that made another major change in my life.

An academic life can be quite wonderful even in retirement, in that you can continue to learn, to write, and to interact with colleagues at your own pace and with little pressure. Cornell has been generous to me, providing me office space and support that has enabled me to keep connected to my academic roots through my retirement years. I have been fortunate to have found a new partner in Marion Nestle, still active as a Professor at New York University and author of several books on nutrition and food policy. We coauthored a book on pet foods and the pet food industry entitled *Feed Your Pet Right* in 2010 (22). That took me back to my animal nutrition roots. Another collaboration has resulted in a book entitled *Why Calories Count: From Science to Politics*, published by the University of California Press in early 2012 (23). I have also prepared a history of nutrition programs at Cornell, which I published online in 2011 (16).

All of this has indeed ended up being an unexpected life in nutrition for a farm boy from Illinois.

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LITERATURE CITED

1. Card LE, Nesheim MC. 1966. *Poultry Production*. Philadelphia, PA: Lea & Febiger. 10th ed. See also 11th ed. (1972), 12th ed. (1977), and 13th ed. (1982).
2. Carrera E, Nesheim MC, Crompton DWT. 1984. Lactose mal-digestion in Ascaris-infected preschool children. *Am. J. Clin. Nutr.* 39:255–64
3. Crompton DWT, Montresor A, Nesheim MC, Savioli L, eds. 2004. *Controlling Disease Due to Helminth Infections*. Geneva: World Health Org.
4. Crompton DWT, Nesheim MC. 2002. Nutritional impact of intestinal helminthiasis during the human life cycle. *Annu. Rev. Nutr.* 22:35–59
5. Crompton DWT, Nesheim MC, Pawlowski ZS, eds. 1985. *Ascariasis and Its Public Health Significance*. London: Taylor & Francis

6. Crompton DWT, Nesheim MC, Pawlowski ZS, eds. 1989. *Ascariasis and Its Prevention and Control*. London: Taylor & Francis
7. Forsum E, Nesheim MC, Crompton DWT. 1981. Nutritional aspects of *Ascaris* infection in young protein-deficient pigs. *Parasitology* 83:497–512
8. Harvey PWJ, Heywood PF, Nesheim MC, Galme K, Zegans M, et al. 1989. The effect of iron therapy on malarial infection in Papua New Guinean schoolchildren. *Am. J. Trop. Med. Hyg.* 40:12–18
9. Hotez PJ, Pecoul B. 2010. "Manifesto" for advancing the control and elimination of neglected tropical diseases. *PLoS Negl. Trop. Dis.* 4(5):e718
10. Hutt FB, Nesheim MC. 1966. Changing the chick's requirement of arginine by selection. *Can. J. Genet. Cytol.* 8:251–59
11. Jalal F, Nesheim MC, Agus Z, Sanjur D, Habicht JP. 1998. Serum retinol concentrations in children are affected by food sources of beta-carotene, fat intake, and anthelminthic drug treatment. *Am. J. Clin. Nutr.* 68:623–29
12. Leach RM Jr, Nesheim MC. 1965. Nutritional, genetic and morphological studies of an abnormal cartilage formation in young chicks. *J. Nutr.* 86:236–44
13. Marshall E. 1985. The Academy kills a nutrition report. *Science* 230:420–21
14. Marshall E. 1990. Academy sued on "plagiarized" diet report. *Science* 247:1022
15. Nesheim MC. 1972. Genetic variation in nicotinic acid requirements of chicks. *J. Hered.* 63:347–50
16. Nesheim MC. 2010. *The Division of Nutritional Sciences at Cornell University: A History and Personal Reflections*. Ithaca, NY: Cornell Univ. Library. **http://ecommons.library.cornell.edu/handle/1813/14711**
17. Nesheim MC, Austic RE, Wang S-H. 1971. Genetic factors in lysine and arginine metabolism of chicks. *Fed. Proc.* 30:121–26
18. Nesheim MC, Carpenter KJ. 1967. The digestion of heat-damaged protein. *Br. J. Nutr.* 21:399–411
19. Nesheim MC, Crompton DWT, Arnold S, Barnard D. 1977. Dietary relations between *Moniliformis* (Acanthocephala) and laboratory rats. *Proc. R. Soc. Lond. B Biol. Sci.* 197:363–83
20. Nesheim MC, Hutt FB. 1962. Genetic differences among White Leghorn chicks in requirements of arginine. *Science* 137:691–92
21. Nesheim MC, Scott ML. 1958. Studies on the nutritive effects of selenium for chicks. *J. Nutr.* 65:601–18
22. Nestle M, Nesheim MC. 2010. *Feed Your Pet Right.* New York: Simon & Schuster/Free Press
23. Nestle M, Nesheim MC. 2012. *Why Calories Count: From Science to Politics.* Berkeley: Univ. Calif. Press
24. Neuman W. 2009. Food label program to suspend operations. *N.Y. Times*, Oct. 23
25. Patterson E, Mistrey R, Stokstad ELR. 1957. Effect of selenium in preventing exudative diathesis in chicks. *Proc. Soc. Exp. Biol. Med.* 95:617–20
26. Schwartz K, Bieri JG, Briggs GM, Scott ML. 1957. Prevention of exudative diathesis in chicks by factor 3 and selenium. *Proc. Soc. Exp. Biol. Med.* 95:621–25
27. Scott ML, Nesheim MC, Young RJ. 1969. *Nutrition of the Chicken*. Ithaca, NY: ML Scott & Assoc. See also 2nd ed. (1976) and 3rd ed. (1982).
28. Stephenson LS, Crompton DWT, Latham MC, Schulpen TWJ, Nesheim MC, Jansen AAJ. 1980. Relationships between Ascaris infection and growth of malnourished preschool children in Kenya. *Am. J. Clin. Nutr.* 33:1165–72
29. Stephenson LS, Pond WG, Nesheim MC, Krook LP, Crompton DWT. 1980. *Ascaris suum*: nutrient absorption, growth, and intestinal pathology in young pigs experimentally infected with 15-day-old larvae. *Exp. Parasitol.* 49:15–25
30. Wang S-H, Nesheim MC. 1972. Degradation of lysine in chicks. *J. Nutr.* 102:583–96
31. World Health Org. 2001. Schistosomiasis and soil-transmitted helminth infections. Agenda item 13.3. *54th World Health Assembly, Geneva, 22 May 2001*
32. World Health Org. 2010. Soil-transmitted helminthiasis. *Wkly. Epidemiol. Rec.* 85:141–48