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FROM CHICK NUTRITION TO NUTRITION POLICY

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■ **Abstract** It is difficult to abstract a summary of a lifetime of work. I have chosen to discuss research on protein, calcium, and the effects of dietary fat and cholesterol on serum cholesterol and on my activities that led to the publication of the *Dietary Guidelines for Americans*. Among the conclusions from studies on protein and calcium is that reasonably healthy people are adapted to their current diets. People all over the world, for example, are raised on relatively low calcium intakes yet have less osteoporosis than those who consume western-style diets. They also appear to do reasonably well on low-lysine intakes. Attempts to define requirements need to allow for adaptation, and we need to determine whether such adaptations are beneficial or detrimental to health. The studies on serum cholesterol defined the role of the various classes of fatty acids. The publication of the *Dietary Guidelines for Americans* introduced a new era of nutrition and has radically altered nutrition policy, nutrition standards, and education.

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UNIVERSITY OF IDAHO

The general direction of my career was probably determined during my first few days at the University of Idaho. I had been awarded a Union Pacific Scholarship that paid a small amount of money—I do not recall how much—and a round-trip ticket to the University. This seemed too good to pass up, so I arrived at the University with $75 in my pocket. I had little idea of the fields available, but I was sure I did not want to be a farmer. The County Agent, Don Bolingbroke, and Lou Williams, who taught Agriculture in the high school, were largely responsible for

my being there, so I thought I would probably do something similar. I had an appointment with Dean Iddings on the third day. He pointed out that all potential students had received a letter saying that a year at the University would cost about $300 and "that there wasn't much point in providing such advice if it was simply ignored." But he must have thought I needed and deserved some support, so a couple of days later, I was told to report to the Experiment Station Laboratory to wash dishes. Don Bolin was the Experiment Station Chemist and he took an interest in me. After I had washed dished for a few months, he began to give me other jobs and opportunities. A major activity in the laboratory was the proximate and calcium and phosphorus analysis of forages collected from around the state. I worked in the lab year round, and by the time I graduated I thought I was a pretty good analytical chemist.

The next summer I was also asked to help Dr. Ella Woods, the Experiment Station Home Economist, who was estimating the vitamin C content of strawberries. My job was to walk out to the farm every morning, pick a few strawberries, weigh out the appropriate samples, and feed them to the guinea pigs. After a few weeks they and the control animals, who received different levels of lemon juice, were killed and Dr. Woods scored them for signs of scurvy. After that summer I usually had something to do for Dr. Woods, and she also took me under her wing. During those years, Don Bolin and Dr. Woods advised me on course work. I took more math, chemistry, and biology than the usual agricultural student. The Dean said I was trying to get a pre-med degree in the College of Agriculture, but he didn't really object.

By the time my senior year rolled around, it was somehow clear that I should go to graduate school, even though I had no money. I had a rough idea of what was going on in various schools and knew the names of some of the prominent people in nutrition and biochemistry or agricultural chemistry, but Don Bolin and Dr. Woods advised me on where to apply. In those days, nutrition studies were the major effort in many agricultural chemistry and biochemistry departments. In February, I was offered a teaching assistantship by Henry Sherman at Columbia University. I was thrilled and immediately accepted. In a few weeks, however, I received offers from all but one of the other schools I had applied to. After a week of turmoil—Don was from Wisconsin and Dr. Woods was a Columbia graduate—I finally apologized to Dr. Sherman and accepted the offer from Wisconsin. New York seemed far away and a little intimidating for an Idaho farm boy.

UNIVERSITY OF WISCONSIN

At Wisconsin, I was assigned to WH Peterson, a fermentation chemist, to work on AIV silage, which was named after AI Virtanen, the Finnish Nobel laureate. He had shown that alfalfa and other forages that contained little sugar could be made into silage if they were treated with hydrochloric acid or other strong acids. They were higher in protein and carotene than corn silage and might eliminate the need

for protein supplements and improve the quality of the ration and the milk. There were many questions about performance of the animals, costs, etc, being studied in animal husbandry. My job was to look at the preservation of carotene and the vitamin A and carotene content of milk from cows fed various rations. It was already known that the standard method for measuring carotene did not work in silages: The silage appeared to have more carotene than the original forage. Over the year, I made all kinds of silage—using different kinds and amounts of acids, from all kinds of green materials, in everything from quart bottles to crocks—and sampled the silos on the farm. I missed an opportunity. Although I could easily set up chromatographic columns that demonstrated various carotenoids and separated them to varying degrees, I never succeeded in getting columns that were easily reproducible. We did develop an improved method for carotene determination (29).

It was becoming clear that AIV silage would not revolutionize the Wisconsin dairy industry. Although I was not privy to the discussions, Professors Elvehjem and Peterson thought I should change problems and considered either putting me in Peterson's fermentation lab, where Frank Strong, Wayne Woolley, Ez Snell, and others were working, or putting me with Professors Elvehjem and Hart to work on new growth factors for chicks. Somehow or other I ended up in the animal work, replacing Herb Bird, who had just graduated.

I cannot imagine a better graduate experience than we had at Wisconsin in those days. The faculty of Professors Hart, Elvehjem, Steenbock, and Link—with Frank Strong, Paul Phillips, Carl Bauman, and others coming on strong—read like a *Who's Who*. There were probably 30–40 students working on almost every current nutritional-biochemical topic. We learned more from bull sessions than from classes. Because practically no one had money, we were, and were expected to be, at the lab from about 7:30 AM to 10–11 PM most of the time. We expected every FASEB meeting to announce something exciting. That was where discoveries were announced—not in the public press. I recall hearing Du Vigneau report on transmethylation, Roger Williams on pantothenic acid, Sam Leprovsky on B6-xanthurenic acid, and of course the Wisconsin group on nicotinic acid in 1938. We searched the literature for every significant advance to report in Professor Hart's Saturday morning seminar. During those years, which have been called the "heyday of nutrition," the vitamin story was nearly completed. I managed some 15 publications, which included studies on silage, vitamin B_6, pantothenic acid, choline, biotin, inositol, arginine, glycine, and the *Lactobacillus casei* factor that became folic acid.

I did not find a suitable job in 1940. After an interview with an industrial company, Professor Hart told me that such companies "prostituted his boys," so I continued to work another year at Wisconsin. The next year I went to the Abbott Laboratories as a research chemist. I did not find it all that stimulating and made no significant contributions. I did try to convince the Director of Research that Abbott should put some effort into the isolation of what we called the *L. casei* factor, that this would be the next important vitamin. The Lederle Laboratories succeeded in isolating folic acid soon after.

Fortunately, Fred Stare recruited me and Jack McKibbin—all Wisconsinites—to be the faculty at the new Division of Nutrition in the Harvard Schools of Medicine and Public Health. Fred described these beginnings in his contribution to this *Review* (51), so I do not discuss them. Fred built one of the better departments of nutrition, provided us with good facilities, and gave us a rather free hand in research. We were fortunate to grow during the time when the National Institutes of Health (NIH) was expanding and NIH funds were usually available. It seems remarkable now that when I was chairman of the Nutrition Study Section, 80%–90% of applications were approved and paid.

Before discussing some of the issues that have interested me over the years, I would note the transformation that occurred in nutrition just after the war. As I indicated, in the 1930s, the major interest was in identifying the essential nutrients and trying to determine their requirements. When World War II was over, however, the nutrition community was surprised to find that deficiency diseases had become rare in the United States, Europe, and in some other areas. Interest in nutrition in medicine and public health fell accordingly. Why teach medical students about diseases they would never see? During the first few years, our group in nutrition had a fair block of teaching time in the biochemistry department at the medical school, but this was gradually whittled down until I had one lecture to teach, "Nutrition," which seemed less than useless. The situation is probably best exemplified by what happened at Oxford. Funds had been obtained to develop a nutrition department just after the war, but the University refused to accept it. Sir Donald Acheson notes that, "[f]or most students in Oxford in those days there were no remaining unsolved problems in nutrition. All of the accessory food factors had been identified. All that was necessary was to eat a good mixed diet, preferably three square meals daily, avoid obesity, and all would be well" (see 50a). Of course the functions of the nutrients had to be elucidated, but this would be done by the biochemists. Enzymology bloomed. Many erstwhile nutritionists, perhaps the more enlightened ones, became enzymologists. I never found sitting in front of a Warburg apparatus very appealing, but my guess is that had the decision been delayed a few years, there would have been no nutrition department at Harvard.

My bibliography reads like a potpourri of nutrition topics, including nutrition of ducks, gerbils, agoutis, cats, dogs, monkeys, and a few other species, with papers on most of the B vitamins, ascorbic acid, vitamin D, vitamin A, inositol, iron, fluoride, and others. I regret that space does not permit a discussion of some of these here.

PROTEIN AND AMINO ACIDS

Protein requirements were of concern during the war years because of the possibility that war-time rationing might lead to deficiencies. Our early studies (30) (which now look meaningless to me), but more important the data in the literature, convinced me that nitrogen balance was possible at relatively low intakes, even

with largely vegetarian diets, and that the role of protein had been generally overemphasized. Various issues related to protein have been a continuing interest over 45 years.

It had long been obvious that proteins varied in nutritional value. This was a function of their amino acid content since it could be modified by amino acid supplementation. The traditional method of estimating the nutritional quality of proteins with young rats was the protein efficiency ratio (PER), originally proposed by Osborne et al (43). This was the gain in weight divided by the grams of protein eaten, and there was a rather voluminous literature, some of it indicating obvious problems. We could rather easily show that this was an unsatisfactory method, for several reasons (31), but it took many years of thought and effort to devise a more satisfactory approach (47). We eventually developed a slope-ratio assay similar to those used in microbiologic assays. This was never accepted because it required larger numbers of animals and was, thus, more expensive. Much later we applied a similar technique to estimate available calories and could show that the calories in fats are more efficiently used than those in carbohydrates (9). It is particularly interesting that even when the total energy intake was not sufficient for maximal growth, the animals receiving more calories from fat had more body fat than those receiving comparable levels of carbohydrate. I believe fat helps make you fat.

It turned out that a completely satisfactory assay for protein quality is not possible because the dose-response lines do not have a common intercept. The reason for this became apparent when we investigated the amino acid requirements of adult rats. Various levels of each essential amino acid were fed to define the maintenance requirement (46). When a threonine-free diet was fed, they lost weight rapidly, similarly to those fed a protein-free diet. In contrast, a lysine-free diet caused a very slow loss of weight; some of the animals maintained their original weight for weeks. The response to other amino acid deficiencies fell between these extremes. Obviously, the animals had the capacity to adapt to a low-lysine diet and conserve their body lysine but little capacity to conserve body threonine. Sue Chu (6) then demonstrated that liver lysine-ketoglutarate reductase—the first enzyme involved in lysine catabolism—fell markedly in lysine-deficient animals, whereas liver theonine dehydratase fell little during threonine deficiency. Both enzymes increased on a high-protein diet. Thus, animals have varying capacity but can adapt to different levels of protein and amino acids in the diet.

These data also undermined other basic nutritional tenets (17, 18). The Biological Value and Amino Acid Score (4), measures of protein quality, had been based upon the idea that because all essential amino acids are required for protein synthesis, a deficiency of any essential amino acid should be the nutritional equivalent of a protein-free diet. Clearly, that is not true.

Rapidly growing animals have limited capacity to adapt to nutritional deficiencies because new tissue is being formed. Thus, it is easy to demonstrate differences in protein quality with growing rats. However, even these animals demonstrate adaptation when fed diets that only allow maintenance of weight. I believe the conclusion must be that in adult animals or in very slowly growing species, such

as man and other primates, protein quality is of much less significance than is generally believed. It is true that differences in protein quality have been repeatedly demonstrated in nitrogen-balance trials with adults, but these have been in short-term studies, which may not allow ability to adapt. How long this may take is unknown.

We do not know how similar man may be to adult rats, but some adaptation to low-protein diets must occur. This would explain why amino acid supplementation has generally failed to improve performance, as well as the ability of populations to perform reasonably well on largely cereal, low-lysine diets. All future studies on either amino acid or protein requirements should be designed with this in mind. Because adaptation is a universal phenomenon, the requirement to maintain the metabolic status quo is simply the content of the current diet of the individual. Measuring the requirement to maintain what is provided by the current diet has little or no nutritional significance.

In 1957, I (15) concluded that growth was a minor determinant of protein or other nutrient needs after the first months of life. The amount of new tissue protein deposited per day in growing children or during the adolescent growth spurt is very small compared with the total maintenance requirement of protein. This distinguishes humans and other primate species from common laboratory and domestic species. The young of most common species require diets higher in protein than adults, but this is not true of humans. Breast milk is a very low-protein diet but obviously nutritionally adequate. During the rapid growth of the first few months of life, 6%–7% of calories as protein is adequate and there appears to be no reason to believe that needs later in life are higher.

Brock & Autret (5) reported the wide-spread occurrence of infant malnutrition in Africa, and it was soon apparent that similar problems were common throughout the developing world. Because edema occurred in some infants and plasma albumin levels were low, it was soon labeled "protein deficiency" (54a). I was skeptical (16, 17). There followed a long period of study and debate, sometimes acrimonious debate. A number of committees, such as the Protein Advisory Committee of the Food and Nutrition Board (FNB), were developed for advice and guidance. To make a long story shorter, these extensive efforts to promote everything from protein to amino acid supplementation reached their peak with the UN conference on Averting the Coming Protein Crisis (45) and almost disappeared after McLarin (38).

During this period, amino acid fortifications was rather widely recommended. Although I was skeptical, the department participated in field trials testing the effects of lysine supplementation of wheat in Tunisia (10) and threonine-lysine supplementation of rice in Thailand (14). The results were essentially negative. As discussed above, it appears that such negative results are explained by adaptation to low-protein diets.

Another tenet of protein nutrition that has confused the issue over the years has been the conclusion that energy restriction increases protein needs. This seemed obvious from the repeated and common finding in nitrogen balance trials that urinary nitrogen excretion increases when energy intake is restricted. Thus, the

conclusion was reached that body protein is burned to provide energy and therefore protein needs should be increased when energy intakes are low, Numerous "high-protein mixtures" were developed for infant and child feeding.

We (48) studied baby monkeys under three dietary conditions. In one group (protein deficient), the protein content of the diet was continually adjusted to a level that prevented growth and just allowed maintenance of weight, but energy intake was ad libitum. The second group (energy deficient) had a normal diet, but the total intake was restricted to just maintain weight. The third (combined deficiency) received the amount of protein required for maintenance, as defined by the first group, but energy was also restricted to a maintenance level. This study required practically round-the-clock monitoring of the animals and diets. For example, the level of protein in the diet of group 1 had to be adjusted slightly up or down on an individual basis, as did the total intake of each animal in the other groups. A dedicated staff was essential. We could concluded that (*a*) the protein-restricted group wasted energy. Although they were not growing, they ate more food (energy) than the energy-restricted group of similar weight. This had been previously demonstrated in swine by others (40). Is ATP formation uncoupled? (*b*) A few of the animals in the protein-restricted group developed moderate edema, but none did in the combined-deficiency group. Other signs of protein deficiency, serum albumin levels, anemia, etc, were not more severe in the combined-deficiency group. Hence, there was no evidence that energy restriction increased protein needs, and in fact, excess energy intake appeared to exacerbate protein deficiency. (*c*) Food intake, and perhaps protein and energy need, varied somewhat from day-to-day. If either energy or protein intake fell below the maintenance level and was not immediately corrected, the animals deteriorated rapidly and died. This is probably the crucial issue in infant malnutrition. Our animals were presumably free of infection, but the situation with malnourished infants is complicated by infections, fevers, etc, that influence both intake and requirements.

Obviously, infants and children must have a nutritionally adequate diet to maintain health, but there appears to be no benefit from high protein intakes. The idea that restricting energy increases protein needs has been based upon inadequate experimental protocols.

I am aware of the difficulty of defining adaptation (3). Probably no one is perfectly adapted to his or her current diet or environment. Yet reasonably healthy people must be considered to be "adapted" to their current diet. If the requirement of any nutrient is to be defined, the subjects must be allowed the time to adapt. Otherwise one simply estimates the nutrient supply in the current diet, which has little nutritional significance.

CALCIUM

In 1950, I found myself in Peru, assigned to the National Institute of Nutrition, to establish a nutrition laboratory. We interpreted the assignment as (*a*) developing the methods available for the determination of nutrients in foodstuffs, (*b*) establishing

a nutrient data base for Peru, and (*c*) conducting dietary surveys in various locals. Once the laboratory was functioning, we invited Emma Rey of the Food and Agricultural Organization to train the dieticians to collect the food-consumption data. I looked for some additional activity.

I had been trained at Wisconsin and knew milk was unique and practically an essential food. Mitchell & Curzon (41) had summarized the available calcium-balance data and concluded that the calcium requirement of adults was 10 mg kg^{-1} day^{-1} (700 mg/day). Most of these data had been collected in the United States and Europe. This led the FNB to establish the adult Recommended Dietary Allowance (RDA) of 800 mg/day for calcium. About the only milk most Peruvians consumed was in their morning cafe con leche. I thought they must be deficient in calcium.

Because all estimates of calcium requirements were based upon balance studies, we arranged with the Central Penitentiary to set up a small kitchen area down three flights of stairs in the bottom floor of the prison. The Director simply selected 10 "volunteers," who had little idea of what they would be expected to do, and I hired Irma Moscoso and another young lady as the dieticians. We conducted balance trials at three levels of calcium intake. The basal diet consisted of most of the foods in the prison diet, but much better prepared, with the small amount of milk and a few green vegetables eliminated. This diet supplied about 300 mg of calcium/day. The higher intakes were provided by graded levels of milk.

Our "volunteers" were a rather remarkable group. They included some of the most notorious murderers in Peru. But they could sit down, be served well-prepared food, and converse with a couple of pretty young women—the best thing that ever happened to them. My principal activity, along with the chauffeur, was lugging the bottles of food and excreta up the stairs, transporting them to the lab, and determining the calcium content.

We estimated the mean daily requirement of these men to maintain calcium equilibrium to be about 350 mg/day (27), and the results are compared with those presented by Mitchell & Curzon [Figure 1 (41)]. A few of the men were not in negative balance even at the lowest level of intake, so we could not estimate their requirement. It is interesting to note that these appeared to be the men from the lowest social classes and had probably been raised on extremely low calcium intakes.

This paper (27) has been widely debated and criticized, but the conclusions must be correct because they make sense. It is obvious that the people in Peru, and in many places around the world where calcium intakes are low, grow up, work hard, and reproduce. This would not be possible if these populations were seriously deficient in calcium. They must be "adapted" to their usual intake and must make more efficient use of dietary calcium than do those of us who consume high intakes. It is equally obvious that those of us who consume milk must be "adapted" to higher intakes. If we utilized dietary calcium as efficiently as do populations accustomed to low intakes, we would be storing unacceptable amounts of body calcium. In 1958, When I returned to Boston, Stan Gershoff (13) demonstrated that the amount

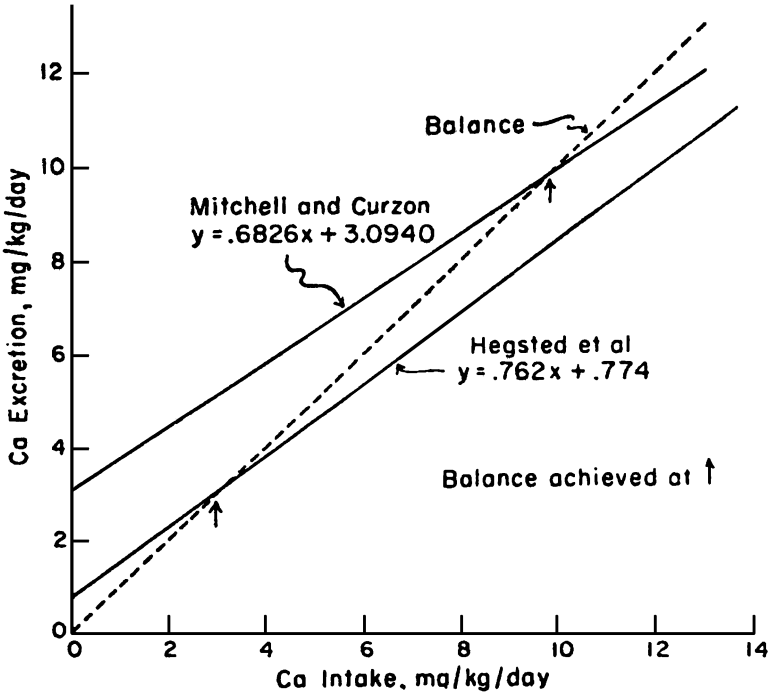

Figure 1 Comparison of the data by Mitchell & Curzon (41) and the data from the Peruvian study (19).

of calcium required to maintain calcium balance in dogs was directly related to the amount of calcium in the diet they had been reared.

Malm (34) demonstrated that adult men with a usual calcium intake of 900 mg/day could adapt to an intake of about 450 mg/day, but that this might require weeks or months. Thus the amount of calcium required for calcium balance is the amount of calcium an individual usually consumes. Whether the hundreds of calcium-balance values that have been (35), and are still being (32), published have anything to do with requirements is dubious. Such data, however, are the primary evidence for the RDA and usual recommendations for calcium intake (42).

When this work was done there was practically no interest in osteoporosis. The message was that high calcium intakes were necessary in infants and children to "build strong bones and teeth." Osteoporosis has now become of great interest and current recommendations are up to 1500 mg/day, based largely upon balance data. It is amazing that although high intakes have been recommended for years, there is still little evidence that high intakes reduce bone fractures. And although quantitative data are still limited, it is abundantly clear that fractures are more common in the United States and in North European societies, where calcium intakes are relatively higher than in most of the world (20). Much of the recent

data have dealt with bone density or bone mineral content. It appears that calcium supplements cause a modest increase in bone mineral, but Japanese women, for example, have both low bone mineral content and fewer fractures than do American women. Although the amount of bone must be a factor, it cannot be the whole story. The crucial issue is fractures, not bone mineral content.

It appears to me that osteoporosis may be another chronic disease associated with the western-style diet, as is coronary heart disease and some types of cancer. High protein intakes, which increase urinary calcium excretion, might be involved, and I have speculated about other possible reasons (22). In any event, if we are to understand the etiology of osteoporotic fractures and true calcium requirements, I believe we must understand the basis of the geographic distribution of the disease. Calcium intake cannot be the whole story.

As discussed above with regard to protein and amino acid requirements, adaptation is a real phenomenon and a primary, and perhaps the most difficult, problem in defining nutrient requirements. This is particularly true for calcium because it is clear that it may require very long periods for adaptation to a new intake to occur.

FATS, CHOLESTEROL, AND SERUM LIPIDS

Experimental work on atherosclerosis in our department was started by George Mann and Lou Fillios, who demonstrated that atherosclerosis could be produced in rats if they were fed diets containing cholesterol and cholic acid (11, 55). I spent considerable time feeding different fats to rats fed cholesterol-cholic acid diets (24). The results appeared to make no practical sense, and I conclude that if you use the wrong animal model, you are not likely to get meaningful results.

Our most important contribution in this area was the Danvers study. Twenty men, at the Danvers hospital, who had mental problems but appeared to be physically normal, who would likely be both cooperative and long-term residents, and whose serum cholesterol levels were between 200 and 300 mg/dl, were selected. This was generally considered to be the "normal" range of serum cholesterol. A separate dining room and kitchen were set up. The men were fed diets containing different levels and kinds of fat and different levels of cholesterol. The fats were provided by Proctor and Gamble, and the dieticians incorporated them into various foods. Filled milk and ice cream were provided by a local dairy. Each experiment period was a month, and two blood samples were obtained for analysis during the last few days of each trial. Every few months a basal diet—representing our best guess as to the average American diet—was given. Changes in serum cholesterol were calculated as the difference between the experiment period and the two basal samples on either side of the experiment month.

Although at the time we did not appreciate it, in the light of current difficulties in conducting controlled trials, this was a rather noteworthy study. Except for a month-long vacation each summer, the study lasted nearly 5 years, and at the

end, 17 of the original men were still participating. Over 50 different diets were given. Although the study would be illegal now, because informed consent was not obtained, this was a great experience for the subjects. They could eat in a separate room, with better food and with concerned cooks and dieticians always in attendance. In fact, at the end of the first year, many were sent home and we thought we would have to recruit a new group. But in September, all were back. These kinds of studies, however, where every meal day in and day out must be prepared and everyone monitored, take their toll on researchers. We eventually tired of the operation. I regret now that we did not anticipate some of the issues being debated at this time.

The results (26) obtained with the first 36 diets are shown in Figure 2. The best description of the results were provided by the regression equation, $\Delta\text{Ch} = 2.16S - 1.65P + 6.77C - 0.53$, where ΔCh is the change in serum cholesterol

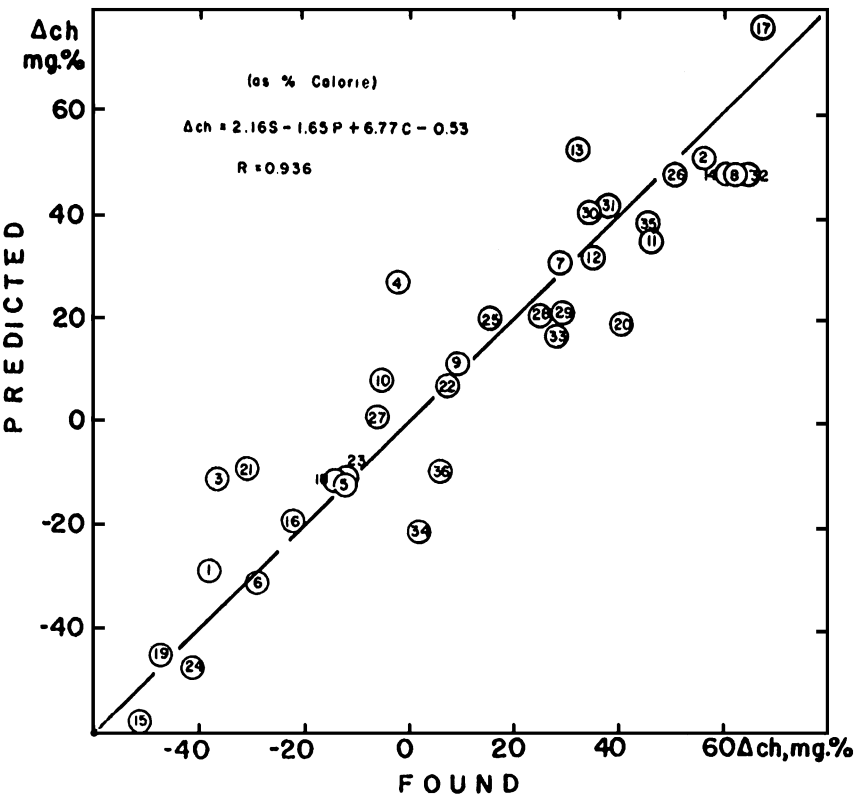

Figure 2 A scatter diagram indicating the correlation between the change in serum cholesterol found and that predicted by the regression equation. (*Numbered points*) The oils and mixtures of oils described by Hegsted et al (26).

expressed as milligrams per deciliter, *S* is change in saturated fatty acid intake and *P* in polyunsaturated fatty acid intake, expressed as percentage of calories, and *C* is change in cholesterol intake expressed as 100 mg/day. This equation explains 88% of the total variance in the data. When monounsaturated fatty acid intake, *M*, was included in the regression equation, there was no improvement in predictability and the coefficient of *M* was not significant. Thus, changes in the three dietary parameters, *S*, *P*, and *C*, were responsible for practically all the dietary effects, with saturated fatty acids and dietary cholesterol elevating and polyunsaturated fatty acids lowering serum cholesterol levels. Monounsaturated fatty acids could not be shown to influence serum cholesterol.

This equation was similar to that produced by Keys et al (33) but, I believe, was somewhat superior because Keys et al did not include dietary cholesterol in their original calculations. It is highly significant, however, that in these 36 diets and in the 30 reported by Keys et al, the intake of monounsaturated fatty acids was varied widely but no effect could be demonstrated. We also reported that myristic acid and palmitic acid were the saturated fatty acids that elevated cholesterol levels, with myristic acid being substantially more potent. I have been skeptical of this finding because the saturated fats most often varied in these diets were butter and coconut oil—both major sources of myristic acid—so the intake of myristic acid was highly correlated with the intake of total *S*. More recent evidence (44), however, supports the conclusion that myristic acid is, in fact, the most hypercholesterolemic fatty acid.

It is important to note that in these men, who had an average serum cholesterol of about 225 mg/dl when eating the average American diet, we were able to shift the cholesterol level about 100 mg/dl by modifying dietary fat and cholesterol. That is, the diet with the highest level of saturated fat and cholesterol and the lowest level of linoleic acid produced an average serum cholesterol 100 mg/dl higher than the most unsaturated, low-cholesterol diet.

The conclusion that the monounsaturated fatty acid has no significant effect on serum cholesterol has been challenged, and this has led to many studies, still with no definite conclusion. The original challenge was done with a cholesterol-free formula diet (36). Considerable data support the conclusion that with such diets, the distinction between the effects of mono- and polyunsaturated fatty acids is minimal (28) and the results may not be generally applicable. Some confusion is also caused by the fact that replacement of saturated fatty acids with monounsaturated fat reduces serum cholesterol, but this is not evidence of a specific cholesterol-lowering effect of monounsaturated fatty acids.

Hegsted & Kritchevsky (21, 25) have discussed some of the limitations in recent studies of the effect of dietary fats on serum lipids. All the studies conducted in recent years have compared a limited number of fats, usually two or three. The variability in the data presented by Keys et al (33) and Hegsted et al (26) should make it abundantly clear that one cannot generalize from such limited comparisons. Even the better recent studies involve provision of food but are without continual supervision. This does not provide good dietary control, and numerous studies

provide no dietary control at all. Dietary information collected by any method is relatively inaccurate, and individuals who have agreed to participate in such trials are likely to be especially unreliable (39). Olive oil, the monounsaturated fat most often studied, is very variable in composition, and there are numerous "other materials" in oils and diets that also affect serum lipid levels. Unfortunately, none of these effects has been quantitated. Thus, at this time, I believe that the data presented by Keys et al (33) and Hegsted et al (26) still provide the best prediction of the effects on total serum cholesterol when dietary fats and cholesterol are modified and are supported by much other data (23).

I would also note that when we compared lauric, myristic, palmitic, and stearic acid by transesterification in olive or safflower oil, they all appeared to have similar effects on serum cholesterol (37). Thus triglyceride structure—so far little studied—may be important.

It is most unfortunate that it is now nearly impossible to conduct well-controlled nutrition studies with groups of adequate size over reasonably long periods of time in this country. Nutrition research is severely compromised.

WASHINGTON, DIETARY GOALS, AND DIETARY GUIDELINES

Nutrition policy in the United States from World War II until 1980 was based almost entirely upon the RDA—assuring an adequate intake of the essential nutrients. The publication of the *Dietary Guidelines for Americans* changed this substantially, and I am pleased to have had a part in their development.

The beginning in the United States was the recommendation by the American Heart Association that Americans, in general, should reduce their consumption of saturated fat and cholesterol. This was rather unusual because most cardiologists at that time had relatively little interest in nutrition, epidemiology, or preventive medicine. I cannot review the background here, but I believe that the data provided by Keys et al (33) and Hegsted et al (26) were important, as accumulating evidence demonstrated the risk associated with elevated serum cholesterol levels. I do not recall, however, that the American Heart Association recommendation caused much public comment.

It was the report of the Senate Select Committee on Nutrition and Human Needs, chaired by George McGovern, that led more directly to the publication of the guidelines. The Committee was established in response to information that indicated extensive poverty and malnutrition in children, particularly in the South and in Appalachia. It was a rather impressive committee and included prominent senators, such as Kennedy of Massachusetts, Humphery of Minnesota, Dole of Kansas, and Taft of Ohio. Obviously, doing something to combat malnutrition was considered politically advantageous. Over a number of years, the Committee held many hearings, and it seems to me that practically everyone in academia, industry, or government who had anything to contribute testified. Although select

committees do not write legislation, this one was responsible for a great expansion of programs, such as the school lunch, food stamps, and WIC (women, infants and children program).

Although I was not privy to the inner workings of the Committee or its staff, it seems to me that by the middle 1970s, the Committee was beginning to run out of steam. The nutrition programs had become large and expensive and could not be expanded indefinitely. Large programs begin to attract opposition and questions about their effectiveness. The staff no doubt looked for other issues that might be tackled, and the Committee then held hearings on what they called killer diseases. I do not have a copy of those hearings but my testimony was as follows:

> The Diet of the American people has become increasingly rich—rich in meat, other sources of saturated fat and cholesterol, and in sugar. There will be people who will contest this statement. It has been pointed out repeatedly that total sugar use has remained relatively constant for a number of years. We would emphasize, however, that our total food consumption has fallen even though we still eat too much relative to our needs. Thus, the proportion of the total diet contributed by fatty and cholesterol-rich foods and by refined foods has risen. We might be better able to tolerate this diet if we were much more physically active, but we are a sedentary people.
>
> It should be emphasized that this diet which affluent people generally consume is everywhere associated with a similar disease pattern—high rates of ischemic heart disease, certain forms of cancer, diabetes and obesity. These are the major causes of death and disability in the United States. These so-called degenerative diseases obviously become more important now that infectious diseases are, relatively speaking, under good control. I wish to emphasize that these diseases undoubtedly have a complex etiology. It is not correct, strictly speaking, to say they are caused by malnutrition but rather that an inappropriate diet contributes to their causation. Our genetic make-up contributes—not all people are equally susceptible. Yet those who are susceptible, most of us, are those who would profit most from an appropriate diet. Diet is one of the things we can change if we want to. There will undoubtedly be many people who will say we have not proven our point: We have not demonstrated that the dietary modifications we recommend will yield the dividends expected. We would point out to those people that the diet we eat today was not planned or developed for any particular purpose. It is a happenstance related to our affluence, the productivity of our farmers and the activities of our food industry. The risks associated with eating this diet are demonstrably large. The question to be asked, therefore, is not why we should change our diet but why not? What are the risks associated with eating less meat, less fat, less saturated fat, less cholesterol, less sugar, less salt, and more fruits and vegetables, unsaturated fat and cereal products—especially whole grain cereals? There are none that can be identified and important benefits can be expected.

> Ischemic heart disease, cancer, diabetes and hypertension are the diseases that kill us. They are epidemic in our population. We cannot afford to temporize. We have an obligation to inform the public of the current state of knowledge and to assist the public in making the correct food choices. To do less is to avoid our responsibility.

From these hearings, the staff prepared a document entitled *Dietary Goals for the United States*, which was published in February of 1977. I do not have a copy of that document, but the recommendations were similar to those made in the next-to-last paragraph of my testimony. There was, however, a flood of comment, both favorable and critical, but mostly critical. McGovern requested the views of many experts, most of whom opposed or provided only partial support (49). I am not very clear on the exact timetable, but my recollection is that I received a draft of a revision in May or June of 1977. My first thought was that the draft should be sent to the FNB for their comments, but it was announced at that time that the McGovern committee would not be reappointed the next year. It was clear that if the document was to be published, it would have to be before the Committee was dismissed and that the FNB could not respond before that time. In retrospect, we were fortunate that the Board could not respond rapidly. Although I was not too impressed with the document, I strongly supported the dietary goals. I am uncertain who the other reviewers were but the second edition was published in December 1977 (50).

The major goals were as follows: (*a*) Avoid becoming overweight; (*b*) increase consumption of complex carbohydrates; (*c*) reduce consumption of refined and processed sugars; (*d*) reduce overall fat consumption from approximately 40% to about 30% of energy intake; (*e*) reduce saturated fat consumption to about 10% of energy and balance with polyunsaturated and monounsaturated fats, which should account for about 10% of energy intake each; (*f*) reduce cholesterol consumption to about 300 mg a day; and (*g*) limit the intake of sodium by reducing the intake of salt to about 5 g a day.

Suggestions were made as to dietary changes needed to achieve these goals. It seemed to me that practically no one agreed. I believe that the nutrition community, generally, was opposed to recommendations that failed to mention the RDA and that they also thought these kinds of recommendations should come from nutrition scientists, not a group of senators. The meat, milk, and egg producers, the erstwhile producers of protective foods, found the goals to have no merit whatsoever. I was told by Phil Handler, then president of the National Academy of Sciences, that he "was sure that the FNB would never accept this kind of nonsense." The American Medical Association opposed it.

Meanwhile, the Agriculture Committee of the US Senate, of which McGovern was chairman, produced the Farm Bill, which specified that nutrition in the US Department of Agriculture (USDA) be elevated to a recognizable position, and that the USDA be given primary responsibility for nutrition research, except for clinical nutrition research. I believe that McGovern was provoked when the chairman of

NIH stated that he thought the NIH should not take a position on the goals, that this might bias research at NIH.

The Farm Bill required the USDA to develop a higher-level position for nutritrion, and I was offered the job of Administrator of Nutrition. Nearing retirement, I thought this might be an interesting opportunity.

The first week after I arrived in Washington, there was a meeting at the NIH—attended by practically every government agency that had anything to do with nutrition—to consider the official response to the dietary goals. It was immediately proposed that the FNB be asked for their appraisal and recommendation. I knew this would mean the end of the goals and argued that we in the government had the expertise needed and, furthermore, that the FNB had no responsibility for implementing their recommendations and that this was the responsibility of government agencies. The upshot was the appointment of a small committee of six to develop recommendations.

Some of this group thought that the appropriate response was a thorough review of all the data available. I argued that this had already been done several times and that all we had to do was prepare some recommendations. No one was prepared to undertake a comprehensive review, so after a few unproductive meetings, we decided that we would accept the conclusions of a task force that had been already convened by the American Society of Clinical Nutrition. The task force reported in December 1979 (2). Although there were large differences in the opinions of the members of the task force, the average scores on the topics that were considered favored—sometimes barely—the issues stated in the dietary goals. A writer was hired and various drafts were circulated through the NIH, the HHS (Health and Human Services), and us at the USDA. Eventually *Dietary Guidelines for Americans* was published jointly by the HHS and the USDA (54).

If the *Dietary Goals for the United States* were controversial, the *Dietary Guidelines for Americans*, for many, were even worse. The meat, milk, and egg producers felt that they had been betrayed, that the USDA, in particular, should be on their side, supporting agricultural efforts. The FNB published *Toward Healthful Diets* (12), which was not supportive. Hearings were held by the Subcommittee on Domestic Marketing, Consumer Relations and Nutrition of the House (53) and by the Committee on Appropriations of the Senate (52), where the dietary guidelines were both praised and lambasted. Space does not permit discussion of the specifics here, but it is unlikely that any Secretary of Agriculture, before or since, would have approved the guidelines.

I believe the real break finally came when the conclusions of the Committee on Diet, Nutrition and Cancer of the National Academy of Sciences in 1982 (7) supported the dietary guidelines. Eventually, the FNB provided general support, in 1989, with a report entitled *Diet and Health* (8).

I recognize that this summary ignores many activities, such as those of many heart associations and departments of health around the world, and the gradual acceptance by various institutions and associations in this country, but these were the primary developments as I saw them. As should be expected, there have been,

and will continue to be, debate about and modifications in dietary recommendations. Overall, I think we did as good a job as was possible at the time and the guidelines have held up well.

The *Dietary Guidelines for Americans* represented a primary change in nutrition standards. Space does not permit a discussion of dietary standards, but I would emphasize that we have some ability to modify the kind of diet people eat through education, marketing, etc, but very little capacity to control the amount of food individuals eat, which is largely controlled by body size and physical activity. Thus, I expect guideline-like standards to eventually displace most standards like the RDA, or their recent modifications. To assume that a single value can be developed for the age and sex groups seems unreasonable because total energy intake (food intake) may vary 100% within a group.

I regret that in this brief look at a lifetime in nutrition I have not been able to identify most of the students, post docs, faculty, and nutritionists throughout the country and the world who have been important to me. There have been many, and I want them to know that it has been a privilege to have been associated with every one of them.

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