



ADVENTURES IN DIET

PART II

BY VILHJALMUR STEFANSSON

Now that the experiments in diet which Karsten Andersen and I undertook at Bellevue Hospital have been accepted by the medical world, it is difficult to realize that there could have been such a storm of excitement about the announcement of the plan, such a violent clash of opinions, such near unanimity in the prediction of dire results.

The feeling that decisive controlled tests were needed began to spread after I told one of the scientific heads of the Food Administration in 1918 that I had lived for an aggregate of more than five years with enjoyment on just meat and water (as described in my article in last month's HARPERS). A turning point came in 1920 when I had an hour for explaining a meat regimen to the physicians and staff at the Mayo Clinic. The concluding phase began in 1928 when Mr. Andersen and myself entered Bellevue Hospital to give science the first chance in its history to observe human subjects while they lived through the chill of winter and the heat of summer, for twelve months, on an exclusive meat diet. We were to do it under conditions of ordinary city life.

At the beginning of our northern work in 1906 it was the accepted view among doctors and dietitians that man cannot live on meat alone. They believed specifically that a group of seri-

ous diseases were either caused directly by meat or preventable only by vegetables. Those views were still being held when, the autumn of 1918, an old friend, Frederic C. Walcott (later Senator from Connecticut), decided that my experiences and the resulting opinions were revolutionary in certain fields, and introduced me to Professor Raymond Pearl of Johns Hopkins, who was then with the U. S. Food Administration in Washington. Pearl considered several of the things I told him upsetting to views then held; he questioned me before a stenographer, and sent the mimeographed results to a number of dietitians. Their replies varied from concurrence with him (and me) to agreement with David Hume that you are likelier to meet a thousand liars than one miracle.

Pearl was convinced that neither fibs nor miracles were involved and proposed that we write a book on dietetics. I agreed. But cares intervened and things dragged.

In 1920 I had the above-mentioned chance to speak at the Mayo Clinic, Rochester, Minnesota. One of the Mayo brothers suggested that I spend two or three weeks there to have a check-over and see whether they could not find evidences of the supposed bad effects of meat. I wanted to do this but commitments in New York prevented.

Then one day while talking with the

gastro-enterologist Dr. Clarence W. Lieb, I told him of my regret that I had not been able to take advantage of the Mayo check-over. Lieb said there were good doctors in New York, too, and volunteered to gather a committee of specialists who would put me through an examination as rigid as anything I could get from the Mayos.

The committee was organized, I went through the mill, and Dr. Lieb reported the findings in the *Journal of the American Medical Association* for July 3, 1926, "The Effects of an Exclusive Long-Continued Meat Diet." The committee had failed to discover any trace of even one of the supposed harmful effects.

With this publication the Lieb and Pearl events merge. For when the Institute of American Meat Packers wrote asking permission to reprint a large number of copies for distribution to the medical profession and to dietitians, Lieb, Pearl and I went into a huddle. The result was a letter to the Institute saying that we refused permission to reprint, but suggesting that they might get something much better worth publishing, and with right to publish it, if they gave a fund to a research institution for a series of experiments designed to check, under conditions of average city life, the problems which had arisen out of my experiences and views. For it was contended by many that an all-meat diet might work in a cold climate though not in a warm, and under the strenuous conditions of the frontier though not in common American (sedentary) business life.

We gave the meat packers warning that, if anything, the institution chosen would lean backward to make sure that nothing in the results could even be suspected of having been influenced by the source of the money.

After much negotiating, the Institute agreed to furnish the money.

The organization selected was the Russell Sage Institute of Pathology. The committee in charge was to consist of leaders in the most important sciences that appeared related to the problem, and represented seven institutions:

American Museum of Natural History: Dr. Clark Wissler.

Cornell University Medical College: Dr. Walter L. Niles.

Harvard University: Drs. Lawrence J. Henderson, Earnest A. Hooton, and Percy Howe.

Institute of American Meat Packers: Dr. C. Robert Moulton.

Johns Hopkins University: Drs. William G. McCallum and Raymond Pearl.

Russell Sage Institute of Pathology: Drs. Eugene F. DuBois and Graham Lusk.

University of Chicago: Dr. Edwin O. Jordan.

Unattached: Dr. Clarence W. Lieb (private practice) and Vilhjalmur Stefansson.

The Chairman of the committee was Dr. Pearl. The main research work of the experiment was headed by Dr. DuBois, who is now Physician-in-Chief of the New York Hospital and was then, as he still is, Medical Director of the Russell Sage Institute of Pathology. Among his collaborators were Dr. Walter S. McClellan, Dr. Henry B. Richardson, Mr. V. R. Rupp, Mr. G. F. Soderstrom, Dr. Henry J. Spencer, Dr. Edward Tolstoi, Dr. John C. Torrey, and Mr. Vincent Toscani. The clinical supervision was in charge of Dr. Lieb.

After meetings of the supervising committee, the election of a smaller executive committee, and much discussion, it was decided that, while the experiment would be directed at strictly scientific problems, there might be side glances now and then toward common folk beliefs and the propaganda of certain groups. For instance, our definition of a meat diet as "a diet from which all vegetable elements are excluded" would permit us to use milk and eggs, for they are not

vegetables. But some vegetarians are illogical enough to allow milk and eggs; we agreed to be correspondingly illogical and exclude them. This forestalled the possible cry that we were being saved from the ill effects of a vegetable-less diet by the eggs and the milk.

The aim of the project was not, as the press claimed at the time, to "prove" something or other. We were not trying to prove or disprove anything; we merely wanted to get at the facts. Every aspect of the results would be studied, but special attention would be paid to certain common views, such as that scurvy will result from the absence of vegetable elements, that other deficiency diseases may be produced, that the effect will be bad on the circulatory system and on the kidneys, that certain harmful micro-organisms will flourish in the intestinal tract, and that there will be insufficient calcium. The broad question was, of course, the effect upon the general health as judged by the observations of the supervising doctors and by the testimony of the subjects themselves.

The test was originally planned on me alone, but I might be struck by lightning before conclusions were reached, or I might get run over by a truck, and that would be construed, by mixed-dieters and vegetarians, as showing impairment of mental alertness and bodily vigor through the monotony and poison of meat. It was difficult to find a colleague, for you cannot make this sort of experiment on just anybody. That appears if you consider two elementary cases.

Assume the news of a stock market crash that ruins them is conveyed to a number of people after they have eaten a good meal. Digestion may stop almost at the point of the mental shock. Obviously the sickness which follows that meal is not caused by the food, as such.

Or ask some impressionable friends to lunch. Serve them veal, of good quality and well cooked. When dinner is over, you inquire about the veal; they will answer with the usual compliments. Then you say that your case has been proved. Rover died and they have eaten him. If your stage setting and acting have been at all adequate, a few at least of your company will make a dive from the room. What sickens them is not the meat of a dog but the idea that they have eaten dog.

The Russell Sage experiment, then, could not be made upon anybody controlled by any strong dietetic belief, such as that meat is harmful, that abstinence from vegetables brings trouble, that you tire of a food if you have to eat the same thing often. But almost everyone holds these or similar beliefs. So we were practically compelled to secure subjects from members of one of my expeditions; they were the only living Europeans we knew who had used meat long enough to eliminate completely the mental hazards.

One man fortunately was available. He was Karsten Andersen, a young Dane who had been a member of my third expedition. During that time he had lived an aggregate of more than a year on strictly meat and water, suffering no ill result and, in fact, being on one occasion cured by meat from scurvy which he had contracted on a mixed diet. Moreover, he knew from the experience of a dozen members of the expedition that his healthful enjoyment of the diet was not peculiar to himself but common to all those who had tried it, including members of three races—ordinary whites, Cape Verde Islanders with a strain of negro blood, and South Sea Islanders.

But there were other things which made Andersen almost incredibly suitable for our test. For several years he had been working his own Florida

orange grove, spending most of practically every day outdoors, lightly clad and enjoying the benefits (such as they are) of sub-tropical sunlight. In that mental and physical environment he had naturally been on a diet heavy in vegetable elements, and had suffered constantly from head colds, his hair was thinning steadily, and he had developed a condition involving intestinal toxæmia such as would ordinarily cause a doctor to look serious and pronounce: "You must go light on meat" or "I am afraid you'll have to cut out meat entirely."

We could find no one but Andersen whose mind would leave his body unhandicapped. So, in January 1928, the test began with the two of us. It was under the direct charge of Dr. DuBois and his staff in the dietetic ward of Bellevue Hospital, New York City.

A storm of protests from friends broke upon us when the press announced that we were entering Bellevue. These were based mainly upon the report that we were going to eat our meat raw and the belief that we were using lean meat exclusively. The first was just a false rumor; the trouble under the second head was linguistic.

Eating meat raw, our friends chorused, would make us social outcasts. It is proper to serve oysters raw, and clams, in the United States; herring raw in Norway; several kinds of fish raw in Japan; and beef raw almost anywhere in the world if only you change the name and call it rare. The fashion of giving raw meat to infants was spreading, but we were babes neither in years nor stature and could not take advantage of that dispensation.

The answer to the raw meat scare was to explain a basic procedure of our experiments—Andersen and I were to select our food by palate (so long as it was meat). It proved that in most of our meals for a year he leaned to medium cooking and I to well done.

The linguistic trouble came from a recent change of American usage. In Elizabethan English meat was any kind of food, as in the expression "meat and drink." In modern England this has narrowed down to what is implied by the rhyme about Jack Sprat eating no fat and his wife no lean, although they both ate meat. In the United States *meat*, in the last few years, has become a synonym for *lean*. The meaning can become even narrower, as when somebody, usually a woman, tells you that she is strictly forbidden by her physician to touch meat, but that she is permitted all the chicken she wants, with an occasional lamb chop. To that woman *meat* signifies *lean beef*.

In the linguistic sense, then, we pacified our friends by references to Mr. and Mrs. Sprat. Our diet would be of meat in the English sense. We were just going to live under modern conditions on the food of our more or less remote ancestors; the food, too, of certain contemporary "primitive hunters."

II

During our first three weeks in Bellevue Hospital we were fed measured quantities of what might be called a standard mixed diet: fruits, cereals, bacon and eggs, that sort of thing for breakfast; meats, vegetables, including fruits, for lunch and dinner. During this time various specialists examined us from practically every angle that seemed pertinent.

Most tedious, and let us hope correspondingly valuable, were the calorimeter studies. With no food since the evening before, we would go in the late morning to the calorimeter room and sit quiet for an hour to get over the physiological effect of having perhaps walked up a single flight of stairs. Then, as effortlessly as we could, we slid into calorimeters which were like big coffins with glass sides, and every-

body waited about an hour or so until we had got over the disturbance of having slid in. The box was now closed up, and for three hours we lay there as nearly motionless as we could well be while a corps of scientists visible through the glass pattered about and studied our chemical and other physiological processes. We were not permitted to read and cautioned even against thinking about anything particularly pleasant or particularly disagreeable, for thoughts and feelings heat or cool you, speed things up or slow them down, play hob generally with "normal" processes.

(Dr. DuBois told of a calorimeter test ruined by mental disturbance. A nervous Roumanian had developed an intense dislike for a fellow-patient named Kelly. During the second hour of an experiment that had been going very well, Max caught a glimpse of the hated Kelly through the window. This raised his metabolism ten per cent during that whole hour.)

With the air we breathed and the rest of our intakes and excretions carefully analyzed, with our blood chemistry determined and a check on such things as the billions of living organisms which inhabit the human intestinal tract, we were ready for the meat.

During the three weeks of mixed diet and preliminary check-up, we had been free to come and go. Now we were placed under lock and key. Neither of us was permitted at any time, day or night, to be out of sight of a doctor or a nurse. This was in part the ordinary rigidity of a controlled scientific experiment, but it was in some part a bow to the skepticism of the mixed-diet advocates and to the emotional storms which were sweeping the vegetarian realms.

Nor was the skepticism and excitement all newspaper talk. One of the leading European authorities, most

orthodox and belonging to no particular school, was touring the United States. He called on us during the preliminary three weeks and assured the presiding physicians most solemnly that we should be unable to go more than four or five days on meat. He had tried it out himself on experimental human subjects who usually broke down in about three days. These breakdowns, I thought, were of psychological antecedents; but our European authority insisted they were strictly physiological—quite independent of the emotions.

The experiment started smoothly with Andersen, who was permitted to eat in such quantity as he liked such things as he liked, provided only that they came under our definition of meat—steaks, chops, brains fried in bacon fat, boiled short-ribs, chicken, fish, liver and bacon. In my case there was a hitch, in a way foreseen.

For I had published in 1913, on pages 140-142 of *My Life with the Eskimo*, an account of how some natives and I became ill when we had to go for two or three weeks on lean meat, caribou so skinny that there was no appreciable fat behind the eyes or in the marrow. So when Dr. DuBois suggested that I start the meat period by eating as large quantities as I possibly could of chopped fatless muscle, I predicted trouble. But he countered by citing my own experience where illness had not come until after two or three weeks, and he now proposed lean for only two or three days. So I gave in.

The chief purpose of placing me abruptly on exclusively lean was that there would be a sharp contrast with Andersen who was going to be on a normal meat diet, consisting of such proportions of lean and fat as his own taste determined.

As said, in the Arctic we had become ill during the second or third fatless week. I now became ill on the second

day. The time difference between Bellevue and the Arctic was due no doubt mainly to the existence of a little fat, here and there, in our northern caribou—we had eaten the tissue from behind the eyes, we had broken the bones for marrow, and in doing everything we could to get fat we had evidently secured more than we realized. At Bellevue the meat, carefully scrutinized, had been as lean as such muscle tissue well can be. Then, in the Arctic we had eaten tendons and other indigestible matter, we had chewed the soft ends of bones, getting a deal of bulk that way when we were trying to secure fat. What we ate at Bellevue contained no bulk material, so that my stomach could be compelled to hold a much larger amount of lean.

The symptoms brought on at Bellevue by an incomplete meat diet (lean without fat) were exactly the same as in the Arctic, except that they came on faster—diarrhœa and a feeling of general baffling discomfort.

Up north the Eskimos and I had been cured immediately when we got some fat. Dr. DuBois now cured me the same way, by giving me fat sirloin steaks, brains fried in bacon fat, and things of that sort. In two or three days I was all right, but I had lost considerable weight.

III

For the first three weeks I was watched day and night by the Institute staff. My exercise was supposed to be about that of an average business man. I went out for walks, but always under guard. If I telephoned, the attendant stood at the door of the booth; if I went into a shop, he was never more than a few feet away; and he was always vigilant. As Dr. DuBois explained, and as I well knew in advance, this was not because the supervising staff were suspicious of me but rather because they wanted to be able to say that

they knew of their own knowledge my complete abstinence from all solids and liquids, except those which I received in Bellevue and which I ate and drank under the watch of attendants.

But my affairs unfortunately demanded that I travel widely through the United States and Canada. This was an added reason why Andersen had been secured for the experiment. When, after three weeks, they had to put me on parole, so to speak, they retained him under lock and key, for a total of something over 90 days.

Those who had believed that a meat diet would lead to death had set at anything from four to fifteen days the point where Dr. Lieb, as clinical supervisor, would have to call a halt in view of danger to the subjects. Those who expected a slower breakdown had placed the appearance of the dread symptoms long before 90 days. In any case, Andersen reported back to the hospital constantly after he left it, and I whenever I was in town.

After my three weeks and Andersen's thirteen, and with the constant analyses of excretions and blood when we came back to the hospital for check-ups, the doctors felt certain they would catch us if we broke diet. Moreover, long before the thirteen weeks ended they had satisfied themselves that Andersen had no longing for fruits or other vegetable materials and, therefore, no motive for breach of contract.

Toward the end of the covenanted year Andersen and I returned to Bellevue for final intensive studies of some weeks on the meat diet, and then our first three weeks on a mixed diet. At this end of the experiment all went smoothly with me, but not so with Andersen.

My trouble, it will be remembered, had been that at the outset they stuffed me with lean, permitting no fat. His difficulty, or at least annoyance, began on the second day after he completed a

year on meat (January 25, 1929) when they asked him to eat all the fat he could, to the nausea limit, permitting along with it only a tiny bit of lean, about 45 grams per day. There they kept him, on the verge of nausea, for a week. The second week they added his first taste of vegetables in a year, thrice-cooked cabbage netting about 35 grams of carbohydrate per day. The third week they omitted the cabbage but retained the high proportion of fat to lean.

These three weeks, Andersen says, were the only difficult part of the experiment. Looking back at it now, he thinks if it were possible to separate the nausea from the other unpleasantness there would have been a good deal left over—that he wasn't, properly speaking, well at the end of the third week. However, that is speculation if not imagination.

Returning to facts, we have the ominous one that a pneumonia epidemic was sweeping New York. The hospital was crowded with patients; some of the staff got the disease, and with them Andersen. It was Type II pneumonia in his case, and the physicians were gravely worried, for this type was proving deadly in that epidemic, carrying off fifty per cent of its Bellevue victims. Andersen, however, reacted quickly to treatment, ran an unusually short course, and convalesced rapidly.

IV

The broad results of the experiment were, so far as Andersen and I could tell, and so far as the supervising physicians could tell, that we were in at least as good average health during the year as we had been during the three mixed-diet weeks at the start. We thought our health had been a little better than average. We enjoyed and prospered as well on the meat in midsummer as in midwinter, and felt no

more discomfort from the heat than our fellow New Yorkers did.

In view of beliefs that are strangely current, it is worth emphasizing that we liked our meat as fat in July as in January. This ought not to surprise Americans (though it usually does), for they know or have heard that fat pork is a staple and relished food of the Negro in Mississippi. Our Negro literature is rich with the praise of opossum fat, nor did Negroes develop the taste for fats in our Southern States; for Carl Akeley relates from tropical Africa such yarns of fat gorging as have not yet been surpassed from the Arctic. A frequent complaint of travelers in Spain is against foods that swim in oil, and there are similar complaints when we visit rural Latin America. We find, when we stop to think, that many if not most tropical people love greasy food.

Then there is the parallel belief that the largest meat consumption is in cold countries. True, the hundred-percenters are way up north, the Eskimos, Samoyeds, Chukchis. But the heaviest meat eaters who speak English are the Australians, tropical and sub-tropical, while the nearest you come to an exclusive meat diet among people of European stock is in tropical Argentina where the cowboys live on beef and maté. They like their meat fat and (so an Argentinian New Yorker tells me) will threaten to quit work, or at least did twenty years ago, if you attempt to feed them in any considerable part on cereals, greens, and fruits.

It appears that, excepting as tastes are controlled by propaganda and fashion, the longing for fat, summer or winter, depends on what else you eat. If yours is a meat diet, then you simply must have fat with your lean; otherwise you would sicken and die. But since fats, sugars, and starches are in most practical respects dietetically equivalent, you eat more of any one of

them on a mixed diet if you decrease the combined amount of the other two.

Sir Hubert Wilkins, when we were in the Arctic together, both living exclusively on meat, told me what remains my best single instance of how fats are crowded out by commerce, fashion, and expense. The expense is frequently not the least; fat, which is only about twice as nourishing as sugar, costs, as I write, at my neighborhood grocery 50¢ a pound (bacon) or 35¢ (butter) while sugar is only 5½¢.

Sir Hubert's father, the first white child born in South Australia, told that when he was young the herdsmen, who were the majority of the population, lived practically exclusively on mutton (sometimes on beef) and tea. At all times of year they killed the fattest sheep for their own use and when in the open, which was frequently, they roasted the fattest parts against a fire with a dripping pan underneath, later dipping the meat into the drippings as they ate. But then gradually commerce developed, breads and pastries began to be used, jams and jellies were imported or manufactured, and with the advance of starches and sugars, the use of fat decreased. Now, except that the Australians eat rather more meat per year than people do in the British Isles, the proportion of fat to the rest of the diet is probably about the same in Australia as elsewhere within the Empire.

A conclusion of our experiment which the medical profession seemingly find difficult to assimilate, but which at the same time is one of our clearest results, is that a normal meat diet is not a high protein diet. We averaged about a pound and a third of lean per day and a half a pound of fat (this is about like eating a two-pound broiled sirloin with all the fat such a steak usually has on it). That seems like eating mostly lean; but grow technical and you find, in energy units, that we

were really getting three-quarters of our calories from the fat. That is what the scientists meant when they said at the end of our experiment that our diet had proved to be not so very high in protein.

That meat, as some have contended, is a particularly stimulating food I verified during our New York experiment to the extent that it seems to me I was more optimistic and energetic than ordinarily. I looked forward with more anticipation to the next day or the next job and was more likely to expect pleasure or success. This may have a bearing on the common report that the uncivilized Eskimos are the happiest people in the world. There have been many explanations—that an Arctic climate is invigorating, that a hunter's life is pleasant, and that the poor wretches just don't know how badly off they are. We now add the suggestion that the optimism may be directly caused by what they eat.

Some additional fairly precise things can be said of how we fared during the year on meat. For instance, with Dr. DuBois as pacemaker, we used every few weeks to run around the reservoir in Central Park and thence to his house, going up the stairs two or three at a time, plumping down on cots and having scientific attendants register our breathing, pulse rate, and other crude reactions. These tests appear to show that our stamina increased with the lengthening of the meat period.

Andersen, who had had one head cold after another when working nearly stripped outdoors in his Florida orange grove, suffered only two or three attacks during the meat year in New York, and those light. He did not regain his lost hair; but he reported that there had been a marked decrease in the shedding. As said, according to the reports of the doctors, Andersen was troubled when he came from Florida with certain toxin-producing in-

testinal micro-organisms in relation to which physicians at that time ordinarily prescribed elimination of meat from the diet. This condition did not trouble him while on the meat.

A phase of our experiment has a relation to slimming, slenderizing, reducing, the treatment of obesity. I was "about ten pounds overweight" at the beginning of the meat diet and lost all of it. This reminds me to say that Eskimos, when still on their native meats, are never corpulent—at least I have seen none. They may be well fleshed. Some, especially women, are notably heavier in middle age than when young. But they are not corpulent in our sense.

When you see Eskimos in their native garments you do get the impression of fat round faces on fat round bodies, but the roundness of face is a racial peculiarity and the rest of the effect is produced by loose and puffy garments. See them stripped and you do not find the abdominal protuberances and folds which are so numerous at Coney Island beaches and so persuasive in arguments against nudism.

There is no racial immunity among Eskimos to corpulence. You prove that by how quickly they get fat and how fat they grow on European diets.

Only one serious fear of the experimenters was realized—our diet for the year turned out low in calcium. This was not demonstrated by any tests upon Andersen or me, and certainly you could not have proved it by asking us or looking at us, for we felt better and looked healthier than our average for the years immediately previous. The calcium deficiency appeared solely through the food analysis of the chemists.

Part of our routine was to give the chemists for analysis pieces of meat as nearly as possible identical with those

we ate. For instance, lamb would be split down through the middle of the spine and we had the chops from one side cooked for us while they got the chops from the other side to analyze. When the diet was sirloin steaks, they received ones matching ours. The only way in which the diet was not identical with the food analyzed was that Andersen and I followed the Eskimo custom of eating fish bones and chewing rib ends; from these sources we no doubt obtained a certain amount of calcium.

Toward the latter part of the test it became startlingly clear, on paper, that we were not getting enough calcium for health. But we were healthy. The escape from that dilemma was to assume that a calcium deficiency which did not hurt us in one year might destroy us in ten or twenty.

You study bones when you look for a calcium deficiency. The thing to do, then, was to examine the skeletons of people who had died at a reasonably high age after living from infancy upon an exclusive meat diet. Such skeletons are those of Eskimos who are known to have died before European influences came in. The Institute of American Meat Packers were induced to make a subsidiary appropriation to the Peabody Museum of Harvard University where Dr. Earnest A. Hooton, Professor of Physical Anthropology, undertook a thoroughgoing study with regard to the calcium problem in relation to the Museum's collection of the skeletons of meat eaters. Dr. Hooton reported no sign of calcium deficiency. On the contrary, there was every indication that the meat eaters had been liberally, or at least adequately, supplied. They had suffered no more in a lifetime from calcium deficiency than we had in our short year (really short, by the way, for we enjoyed it).

(To be continued)