

Historic correspondence between James Watt, former director of the National Heart Institute and Zdenek Fejjar, former head of the WHO Cardiovascular Diseases Unit.

1992

Dear Zdenek,

In my old age I have decided to see if I can acquire a new skill and this the result. As you can see I still have not gotten to the error free stage but with practice I may get there. In any event I plan to use you for practice. My excuse for not sending you some of my recollections about the early days of the Heart Institute has been that in retirement I had no secretary and I never did like to use a recording machine. So here goes.

I will start with my first memory of Zdenek. It was at grande rounds in Prague given by Paul White. As was his custom he put in a very controversial comment about the case at hand and see who would be brave enough to take [on] the master. We were both impressed by the young man who did. Not only did you take him on but did it with a well reasoned explanation of why you chose to argue with him. Paul was pleased and impressed. Later when Candau asked me about some one for the job at WHO we both thought of you. As I remember it you also get a boost from one of your friends in Sweden.

As to the Heart Institute (NHI) the early days were a bit of a problem for me. I was a regular corps officer in the USPHS and had spent 16 yrs doing research in infectious diseases, mostly with diarrheal diseases. I really had given no thought to the cardiovascular system since I got out the hospital training. One of my friends said it would be real easy for me. All I had to do was shift from an open to a closed circuit!

It was not that easy for me nor for the cardiovascular establishment. At that time the planning for the 2nd World Congress of Cardiology was just getting underway and I was co-chairman of the planning committee. When I went to the first of many meetings the only person in the room I had ever met was Paul White and I had only met him once. Needless to say I did a lot of listening and very little talking. On balance though I think it was a stroke of luck to have that come as the first order of business with so many leaders of the professions. I got to work with them on a common goal with no real problems of competing interests, a matter that was inevitable when one got into priorities on how to spend a limited supply of money.

The congress was fun and one side effect was that this was the first time in many years that any one from USSR other than people in their embassy had applied for a visa to come to USA. Our State Department was really not sure about how to handle the situation since the Russians had waited until the last minute to send in their registration. I never was sure why it caused such a commotion. Suffice to say that I and the Secretary General of the Congress were called to the office of the Secretary of State, Mr. Dulles and given a talk on how to greet them. In a word we were told to be polite but cool. One of the people who came was a man you got to know later. He was a surgeon who became minister of health, Prof. Petrovsky. It was at the congress that Paul White made the contacts that led to his being invited to the Soviet Union and the first of a series of exchange visits of people in the cardiovascular field.

One of the unusual things about the law setting the NHI was the fact that no money could be spent on grants unless it had

been approved by the Heart Council. The Council was made up of 15 people. 6 of them were experts in the field, 6 were layman, business people lawyers and people active in the health field such as Mary Lasker. In addition the medical corps of the army, navy, and veterans adm. had ex officio members. The real work was done by the 12 appointed members. Working with that group was the most rewarding part of my job as I look back on it.

Council members were appointed by the secretary of HEW on the advice of the surgeon general for a four year term. This fact led to an interesting point in a couple of years. In the beginning all of the members knew as much or more about their job as I did and did not hesitate to tell me if they had any divergence of view. In a couple of years tho I realized that we were not having as lively discussions as had been the case in the 1st 2 years. It took a while for me to realize that these people all experts on their own home turf were a bit intimidated by the new environment. I learned that it was up to me to get them to feel at home as quickly as I could if I really wanted their candid opinion. I suspect that you saw this same phenomenon with the turnover of the executive board of WHO.

I think the most traumatic experience came in my 2nd month on the job. Eisenhower became President in Jan. Pres. Truman had presented a budget to the congress but the new administration wanted one of their own. Truman had submitted a budget for the heart institute of 12 million dollars. We were told to cut it by 15% and another of 10% reduction. We were given a week end to do it. I had no time to consult with the council and very little to talk to my lab chiefs. It was obvious that a simple 10% or 15% reduction applied across the board

would neither be fair or for that matter defensible. I don't think that anybody was happy with the result. Fortunately the congress did not agree with the president and we wound that year with an appropriation of 16 million. Under the rules laid down by the office of the president I was supposed to defend the budget against any cut and any increase. As you can see I was not very successful. That budget pattern held throughout my eight years as director much to the pleasure for the research people and to the displeasure of the people in the presidents budget bureau.

I determined never to get caught again as unprepared for a quick order to cut the budget. Beginning that spring I made each lab chief defend his budget request and when it was accepted he was then told to show how he reduce it by 10 and 20%. At first they risted with great fuss. In a while though it was clear to them that doing so was their best protection not only against arbitrary cuts but in showing me how and where a cut hurt they gave me the information needed to defend them and their program. You may have noticed that I frequently asked people in WHO how to cut their requests and very few of them liked the idea. Some of them saw it for what it was, giving me the information I needed to defend the WHO budget before our state dept and the congress.

While on the subget of the budget I might as well give you a brief Explanation of why I was so unsuccessful in defending the Heart budget. Mary Lasker and some of her friends organized a group of people for each institute to present to the congress a budget that considered appropriate. This of course was free of the constraints imposed by the administration. These people went before the appropriation committees and presented a budget that they felt would meet the needs of the

country. The people who testified were folks like Paul White, Cowles Andrus and Mike DeBakey. Most of the time the amounts they requested were about the same as the amounts prepared by the Various institute directors before it had been trimmed by the various levels of the administration. From that point off view at least one could say that I was reasonably successful in the defense of a budget. The lobbying effort led by Mrs. Lasker was one of the real success stories of medical reseach.

A year or two after I became director an unanticipated problem came to our attention. The very real increase in the level of support of research led to an almost exponential increase in the number of publications. Some of the people doing research complained that they had no time left for lab work if they really tried to keep up with the literature. A bit of an exaggeration but not too far from the truth. The heart council had many a discussion about the problem and what they could do about it. One of their first attempts was while it did help the situation some thing more was needed. The National Library of Medicine had been created by placing the library of the defense dept and other small medical collections into the Public Health Service. The library was still operating with the tools of the last century and had never had financial support it needed to go modern. The heart inst. staff did a study to estimate the cost of a system to put the cardiovascular literature on a computer. I don't remember the total estimate but the council thought it would be money well spent. We were directed to negotiate a deal with library. There were a number of bureaucratic hurdless to be gotten over including who would pay the on going costs after the original program was set up. Thus money from the Heart Institute

bought the first computerized system. Its acronym was MEDLARS. This came from the first letters of its title, Medical Literature Analysis And Retrieval System. The next year the full costs of the system were out in the budget of the library by the congress. The pay-off for the heart institute was that first literature analysis done by the new system was in the cardiovascular field.

When the Heart Institute was created in 1948 Dr.C.J. Vanslycke was director and James A. Shannon the scientific director. Shannon planned the intramural research program which ranged all the way biochem and pharmacology to medicine and surgery and an instrument development laboratory. The leaders of these labs were recruited and put to work before there was any place for them on the campus at NIH. They were all brought together at the clinical center in 1952, the year I became Director. The only one of the lab chiefs that I picked was the chief of surgery. That had been left vacant until we had some operating rooms for the surgeons to use. I also had to get a replacement for Shannon. The lab chiefs that had been recruited were a very fine group and they wanted no part of an new man they didn't know. After several unsuccessful attempts to find a man they all liked we finally decided to have a try any running the show by committee. It worked well for a few months but when the realities of budget preparation were brought home to them one by one they began to get serious about having a boss. They then elected one of their own members as scientific director. Robert Berliner was the choice. He later became scientific director for all the institutes. He had resisted the idea of becoming an administrator for a long time but became quite [good] at it. He left the service to become Dean of the Yale Univ. Med. School.

All of the last paragraph is sort of a preamble to telling you about the two parts of the intramural research program that I took hand in. The Framingham study had its roots in a community study of tuberculosis. There had also been quite a bit of talk about the need for an epidemiological study of chronic diseases. The surgeon general had told the institute to get on with the job. There was almost no interest in the matter on the part of the basic science people. In fact there was really opposition to having any of the not unlimited funds go for such things. With my background in epidemiology I had no such prejudices, in fact I think I thought it was as good if not better than the stuff the science boys thought so much of.

In addition to the problem of belief in the value of epidemiological studies there was the problem of secure financing of a project planned for at least 20 years by an institute that depended on annual appropriations of the congress. As director I was able to assure the program of its fair share of the budget. The problem of getting good people to take such a long term commitment was made simpler by the fact that the members of the commissioned corps of the Public Health Service could be assigned to such duty by the surgeon general and the personnel did not have to worry about the possible change in the political climate. The way it turned out the most skeptical of the people in the institute came around to the view that the Framingham study was worth all of the funds spent on it. It was the first and in some cases, the only study to have any data on the risk factors in cardiovascular disease.

There is one little anecdote you may find amusing. For some time I tried to get Dr. T. Francis the Prof. of Epidemiology to get involved in a long term study of atherosclerosis. He

demurred saying that he really was not qualified since all his life he had worked on infectious diseases. One day I got a call from a very excited Dr. Francis. He said, "While was shaving this morning I had an idea. I will simply call atherosclerosis "arterial pox" and I will have no problem in knowing how to go about organizing a study". A short time later he applied for a grant to set a community study in the town of Tecumseh and it was approved by the council.

The other work that I took a personal interest in was the study of the ageing process. When the heart institute was founded in 1948 the Surgeon General gave it the responsibility of supporting and carrying out research on the aging process. A laboratory was already in being at the Baltimore City Hospital and it was transferred to the heart institute. Like Framingham it was an anomaly in the well-ordered plan of research set up by Jim Shannon. Also like Framingham it was not located on the NIH campus. I was very much aware of the fact that people who worked yway from headquarters needed a powerful friend at hdq. Unless there was some one there to speak for you at the time the budgets were marked up the folks away from home. I decided to be that friend. I am pleased at the results of the care given in those early days. One of the last things I did before going on to international health was get an appropriation for a research building dedicated to research on ageing at the Baltimore site. It has now evolved into the Nat.Inst.on Ageing.

On my first trip to the Soviet Union I asked to go to the primate colony in Sukhumi where they were reporting some studies on hypertension. I was impressed by the scope of the colony and that they had managed to keep it going the war when things were tough all over. In the U.S. the few places

doing research on non human primates had almost come to a halt. I had been convinced of the importance of such colonies while working with a colony in Puerto Rico in 1941.

I was puzzled over why nothing really substantive had been done. My conclusion was that no one had been willing to commit enough resources to bring such a colony to a critical mass. After discussion with the folks on the trip and later with the council we decided to go for a national primate center with a research base as broad as the NIH. In working for the funds for such a center we had to show that it was essential to the mission of the heart inst. We made the case but political reality made us change the focus from a national center to a series of regional centers. These regional centers, now 7 in number are one of the things I feel really paternalistic about. While the need for such centers made inevitable that some day they would have been funded I think I gave them a head start of many years.

After reading the previous pages I think should add a few more about the study of the ageing process I wrote about a couple of paragraphs back. Today virtually everyone thinks of the NIH as a source of money for research but before 1946 NIH had little or no money for grants. In fact the first study on the ageing process carried out by the Public Health Service was begun with a grant to NIH to initiate such a study. It was the laboratory headed by Dr. Nathan Shock that became a part of the Heart Institute when Congress passed the Heart Act in 1948 and it was decided administratively that the Heart Institute should be the focal point for conduct and support of research on ageing. At the time there was moderate amount of political support for a separate institute on the subject but

this did not happen until after we had gotten a research building at the Baltimore City Hospital.

While I like to take credit for pushing this through at the federal level, it would never have happened without the enlightened and dedicated support of two people in the Baltimore administration, Mrs. Lazarus was in charge of the city department that ran the health and welfare system. Mr. McMillan was superintendent of the hospital which was primarily for care of the indigent, I never did find out how these two people came to be such tenacious supporters of the program of research at a city hospital. I think though that it came about in no small part because they saw how the hospital was more and more having the beds filled with older people with chronic illness for which medicine had no answers except to warehouse them. I have a great deal of respect for their foresight and their willingness to push for something that was not a conventional part of a city health department.

Before closing this mess of words I think I will add a bit about my life before the heart institute. After getting my MD in 1935 my DrPH in 1936 from Johns Hopkins I took a couple of years on the house staff of the infectious disease hospital of the City of Detroit. I then took the exam for the regular corps of the USPHS. I was assigned to the laboratory of infectious diseases where I spent the next 15 years but never at the Headquarters in Washington and later in Bethesda. Most of the time looking into the causes, treatment and prevention of enteric diseases including poliomyelitis. In those years I got real interested in international affairs. I went to China on a cholera commission to assorted South American countries for PAHO And organized a commission on enteric diseases for the armed forces which took me to Korea when there

was a big outbreak of diarrhea in the POW camps. I think it was this diversity of experience that led to my selection as director of the heart institute. It was background and the exciting international involvement at NHI that made me jump at the more complete immersion in international affairs when the chance to do so came up. I still look on the WHO experience as the most fun of all. Without it I would not be pecking this out for you.

I think I will call it quits for now. I really am not sure what you want but am sure this will give you a sample of what is in my memory bank and also give you a way to ask specific questions.

All the best from
Jim Watt

Praha June 23, 1992

My dear Jim,

Very grateful for Your excellent account on the work in NHI and in it your hidden activity. I am ashamed not to reply earlier but an excuse is that I was away for several weeks attending one Congress in Nice and another in Geneva.

I wonder whether You could write a little more about Yourself - starting with birth et cet. and particularly about research in diarrhoe and your China experience. Then about personalities you dealt with at that time - Sid Petersen, Irwin Page and Dave Rutstein. Am I asking for too much? The reason is that you did so well at the start.

We are now living in a very exciting time and hope that the autumn will clarify the principle - to be or not - meaning one state or two. The second possibility seems to be more likely, but one cannot be sure with our younger brother!

As You can imagine I am still working in the Institute for the dismay of some and also chairing our oldest Society of Czech physicians which reached in these days 130 years of age. Altogether - more to do with less effectiveness - how long I don't know - Hanka is furious from time to time.

Thanks again and looking forward to hear from You again, with best personal regards to Peg and Yourself from both of us

As ever Yours

July 8 1992

Dear Zdenek,

That is a tall order you have set for me in your letter of 23 June but I will give it a try. Also will let you see if my typing has gotten any better since the stuff I sent you some time ago.

About me. I was born Apr. 28, 1911 in Thomasville Ga. Was valedictorian of my high school class. Went to Davidson College and then to medical school for two years at UNC and then graduated in 1935 from John Hopkins. While there my father's business failed and a friend of the family offered to pay my last years tuition if I would take a year at her expense in a school of public health. I accepted and by taking courses in the Hopkins School of Hygiene while in med school was able to get my DrPH degree in 1936.

I shared a room with Dr. A. V. Hardy while working on my thesis and he was planning a study of diarrheal disease in New Mexico. He offered me a job for the summer before I was to take hospital service in the fall. That was the beginning of an association that was to last with few interruption for almost 20 years. Over the years we published 20 or more papers on acute diarrhea. In 1938 I was commissioned in the Public Health Service and was assigned to the NIH. In carrying out these studies I worked 1st in MN, then Georgia. Where I was involved in studies of typhus and spotted fever (Hardy in 1938 gave a summer job to newly graduated MD from Columbia U. She was assigned to work with me and that is the way I met my wife to be). After GA we went to Puerto Rico, then to New Orleans where I was stationed during most of WW 2. Most of that time I was evaluating various Rx of diarrhea for the military. It was in NO that I ran into an epidemic of salmonel-

losis from hens eggs. As far as I know it was the first time that seemingly normal chicken eggs had been incriminated in spreading salmonellae. That along with some of the other studies got me the Bailey K. Ashford award of the Am. Soc. of Trop. Med. in 1945.

In '45 there was a major outbreak of cholera in China and a request for assistance thru our ambassador led to the formation of a small task force to go to Chungking. My boss and I thought that little could be done beyond the symbolism but we decided that maybe something could be learned if I tried out some of the newer culture media we had found successful. It took a bit of doing what with wartime shortages to get adequate supplies. Alas for all our efforts the plane carrying our supplies got in trouble over the hump and had to jettison cargo and by the time the backorder was filled the war was over and I was on the way home.

When I got back I found that DDT was available and it was thought that it might be a way to control flies. I was put in charge of such a study in Texas where we found that it was effective in controlling flies and Shigellosis. The fly control did not stop an epidemic of poliomyelitis nor the spread of salmonellae. It was also clear to me that basic sanitation and the provision of water was a damn sight better and more economical.

Back to N.O. in 48 where I continued to do drug evaluation and try to find out why there was more salmonella infection in kids there than in any other place we had worked. I also organized and directed a commission on enteric diseases for the armed forces. By this time I was also getting called on for consultant work not only in the states but also in South and Central America. A lot of observations in the military-

among POWs in Korea and in migrant worker camps had led to the inference that simply making water available in quantity would do a lot towards cutting down the spread of these infections. With the cooperation of the State of Calif. I was planning to move there and test this hypothesis in the migrant labor camps when I was tapped for the Heart Institute. I still think that cutting down on the fecal film on foodstuffs and our environment by having plenty of water around for home use and kids to play in is the biggest single factor in tipping the balance against the parasites that cause enteric infections. I am just sorry that it still has to be tested.

After looking over my comments on China I notice that I said little about what I did in the 6 months I was there. I was sent to a very rural town where cholera was reportedly raging. We flew in with water purification gear and found that the hospital set up to cope with the emergency had more staff than patients. It was while there that the bomb was dropped on Hiroshima. When I got back to Chungking I was sent by the army to Shanghai with an intelligence team to help the US POWs held there and find a suitable place for a field hospital for the troops to be sent in to take the Japan surrender. Was in the 1st US plane to land at the Lunhua airport and rode into town in the 1st jeep in the area. Later was in the 1st US plane to land at the airport in Amoy where I was sent to assist repatriation of Chinese coming home from the Philippines. The ship came to port flying the yellow quarantine flag. Since the usual port authorities did not exist I was appointed acting quarantine officer by the acting mayor of Amoy. From Amoy to Hong Kong by ship and back to Shanghai by plane. Came home via London just in time to have Xmas with the kids.

Sid Peterson. Met him and Vera in Puerto Rico where both of them worked for the health dept. At that time Sid was very jealous of Vera since she was getting ahead faster than he was. They almost split up and would have if Vera had not decided that the family was more important to her than the job. Peg became very close friends with Vera at that time and we still keep in touch. Despite the many positions he held Sid never achieved the success he craved.

Dave Rutstein. Got to know him 1st at meetings of the Am. Publ. Health Ass. while he was working for the N.Y. state H.D. A very bright guy who achieved his lifelong ambition when he was made a prof. at Harvard. Saw quite a lot of him while he was doing the studies on prevention of rheumatic fever with penicillin. Got into wine tasting while in Italy during WW2 and turned the hobby into profit by becoming the buyer for S S Pierce in Boston.

Irv Page. Another very smart man. met him at assorted meetings after I became Director of the Heart Institute. I think of him as a master of dry humor, a dedication to careful bench work in the lab and the first one to tell me that serotonin would be one of the important discoveries of our times. In a way serotonin is like aspirin. The longer it is studied the more things we find that it plays a part in. Irv was a very effective member of the heart council.

In closing the comments I have just recalled a piece of advice given me by the finance officer of the PHS. He said, "don't make the mistake of thinking that your testimony before an appropriation committee you are there to talk about money. Remember that only when you are talking about program will you be making money. Any time you get on the money angle you will be losing money".

Will add a few words about us. Norfolk has been fun and I am glad we moved when we did. Peg began to have equilibrium problems before we moved and got to the point that she was using a cane even around the house. Some months ago we started swimming on a regular basis at a year round pool a block from our house. It has been a big help and the cane sits in the corner unused. I keep up my garden and the both of us have a ball with the grandchildren. While we miss the Vt. country side there are compensations!

Hope this finds you well and not too busy tho I doubt it.

All the best

Jim

While I dont really like travel these days I think I could be persuaded to make a trip to Geneva for a reunion of staff + Bd members from my day. To hear how changes in this world has affected them would be worth the price of admission.

