

Interview with Kerr L. White

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Conducted by Edward Berkowitz

Berkowitz: I want to ask you a little bit about yourself and about health services research. Let's start by talking about yourself. It's interesting to me that you're a Canadian and yet your career has been here in America for the most part. Was that ever an issue for you or with you?

White: No. I visited the States frequently when growing up. My mother was English but had numerous American relatives. My father was a journalist for the *Times* of London and *The Economist*, as well as for several American newspapers; I grew up with an international perspective. I recall that in my undergraduate McGill University Yearbook I stated that: "I am a citizen of the world, an internationalist." Moving to the United States didn't really concern me; it bothered my wife a little more. When I graduated from McGill in medicine after my residency and a fellowship I could have stayed on as a faculty member. I had an excellent opportunity to do the mixture of research, patient care, and teaching that interested me at the University of North Carolina in Chapel Hill, and we moved there.

Berkowitz: McGill has always been a very English place. I guess the tension between the French and the English was not as pronounced in Canada then.

White: No. It's grown over the years. The French were downtrodden and strongly dominated by the Catholic Church. On balance in the thirties and forties the limited theologically-oriented classical education provided for most French Canadians didn't prepare them for the commercial, intellectual, and technologically-based world that would characterize the emerging global village. In recent decades French Canadians have come into their own and they have demonstrated tremendous ability and capacity to run their part of the world in first-rate fashion. I hope Quebec won't separate from the rest of Canada. Both parties would lose a great deal.

Berkowitz: Are you an American citizen now?

White: I'm a naturalized American and I am also a British subject. Neither government is too fond of the idea but legally you can have both passports. It's very useful if you want to go visiting, for example, Japan, Hong Kong, and Australia on one trip.

Berkowitz: One thing that's unusual about your background is that you received a graduate degree in economics at Yale in 1941. What did you intend at that point? Had you not decided to go into medicine?

White: I majored in economics and political science at McGill and was actually quite interested in transportation. I received fellowship to Yale's graduate department of economics and focused on transportation economics and management. One professor who had the greatest influence on me was Elliott Dunlop Smith. He had worked in the personnel department of the Avery Dennison Company where he observed the importance of the work place environment on workers' morale and productivity. This was back in 1940. I learned then about the Hawthorne Effect which later influenced me, and I also learned about the effect of occupation and the work place environment, including management's attitudes and behavior, on employees' health. That got me interested in medicine to which I then

gravitated gradually.

My first job was with the RCA Victor Company in Montreal as a junior personnel assistant. One of my tasks each Friday afternoon was to pay off the piece workers who assembled radios for the Army and the Defense Department. These people, largely young women, were paid by the hour. I would give them pay envelopes containing \$5.89, \$17.21, and so forth for a week's work. They would break down in tears because they had no steady income and their wages were miserable. Incensed by this apparent gross injustice I took the initiative to help the RCA workers successfully organize a union. We struck the plant, and I spent a lot of time with union organizers in poverty-stricken homes. We won the strike, negotiated a contract, increased their salaries, established a minimum wage and assorted other benefits. Of course I left RCA to do all this and for my troubles was investigated by the Royal Canadian Mounted Police (the RCMP is the Canadian equivalent of the CIA) but nothing ever came of it. In the personnel department I worked with the company's health department and saw how they excluded people with health disabilities, and if anyone showed up with back problems or any difficulties they were likely to get fired. This seemed to me unjust.

Berkowitz: Partly a workers' compensation problem, though, right?

White: Yes, but we didn't have much of that in those days. The experiences at Yale and RCA are what got me interested in medicine.

Berkowitz: In 1941 did you know you were going to have to go in the Army then?

White: No, but I suspected that I might have to at some time; Canada was in World War II from the beginning in 1939. I applied for medical school in 1942 and had I been drafted probably could have gotten a deferral, but I went ahead and enlisted voluntarily. I was in the Army for three years.

Berkowitz: Did you go abroad?

White: I was in Canada and then in England. I was there on D-Day and during all the rocket and "buzz" bombing. I went to medical school after I was discharged from the Army in 1944.

Berkowitz: You went to medical school at McGill?

White: That's right.

Berkowitz: In medical school, once you reached the stage where you have to decide what your field is going to be, what did you think it was going to be?

White: I had eclectic interests in many aspects of medicine and wrote a number of articles at the time, including one on comprehensive medical education and health care in which I looked at a variety of contemporary experiments in the provision of care and in medical education. These were published in the McGill students' medical journal. Another article dealt with iatrogenic illness; I wasn't sure that doctors always did more good than harm. I concluded at the time that the power of medicine was shifting from surgery to internal medicine and that if you wanted to have any influence, you'd better get qualified in internal medicine. So I did decide on that, although I also was attracted to general practice. Our daughters were delivered by the last general practitioner who had obstetrical privileges at the Royal Victoria Hospital, one of two principal teaching hospitals of McGill. But I was already interested in broader aspects of health.

My father knew Jimmy McIntosh, a fellow Scot, who was then the Dean of the *London School of Hygiene and Tropical Medicine*. McIntosh told my father about John Ryle at Oxford, my father's university. Ryle had been Regis Professor of Medicine at Cambridge, which as you know is one of the top posts in academic medicine in Britain. He became persuaded that medical schools should adopt a broader population-based perspective in addition to the purely clinical emphasis that prevailed. The Nuffield trust established a chair and an *Institute of Social Medicine*, the first of its kind anywhere, at Oxford University, and Ryle switched universities and disciplines to become the first Professor of Social Medicine at Oxford. He initiated studies of the distribution of health services and of disease in the population. I was intrigued by his work and was in the process of negotiating to spend a fellowship year or so with him, but he died, unfortunately, and that never came to pass.

Ryle wrote a remarkable little volume called *Changing Disciplines*, based on a series of lectures he gave on a tour sponsored in the United States and Canada by the Rockefeller Foundation, for which, by coincidence, I recently worked. He also wrote several volumes on clinical medicine that I thought were remarkably insightful. He discussed the natural history of common symptoms, concepts such as the basis for determining what is "normal" and a variety of other topics that questioned the received clinical wisdom and invited further exploration. For the first time I had encountered a distinguished academic clinician who was not just behaving as an authoritarian professor but as one of a new breed emphasizing the importance of seeking a scientific basis for clinical interventions.

When it came time for internship, I could have stayed at McGill, and was told that later I should go to Boston or Baltimore for additional training. I was pleased by the faculty's interest in me, but I said that I didn't want to spend a year running around with a syringe drawing blood for others to do studies in the laboratory. I wanted to learn first-rate clinical medicine. In retrospect the clinical care at McGill was far superior to anything I have encountered in any American teaching hospital, including Hopkins. I wanted to go to a place where they really do clinical medicine full-time. The three sisters, as they were known, had that reputation. They are the Mary Imogene Bassett Hospital in Cooperstown, New York, the Mary Fletcher Hospital in Burlington, Vermont, and the Mary Hitchcock Hospital in Hanover. A friend of mine, who has since died, and I decided together that we would go for the Mary Hitchcock Hospital. We were both accepted there and we really did get superb clinical training. I've never seen clinical care of such high quality anywhere else. Things may have changed, for this was a long time ago, but we got a lot of one-on-one mentoring.

Berkowitz: Isn't it an awfully rural area, Hanover?

White: It is.

Berkowitz: It has Dartmouth affiliation, the hospital?

White: Yes. Now Dartmouth-Hitchcock is quite a substantial medical center, although it was more modest in those days. It has quite a large catchment area in New Hampshire and Vermont and the adjacent states. But you're right, it's basically rural compared to New York or Boston urban centers. My former student, Jack Wennberg, has a large research center at Dartmouth. He does all of his world-renowned population-based studies of the huge geographic variations in diagnoses and clinical interventions from that vantage point. Location in a semi-rural environment doesn't have anything to do with first-rate care, education, or research. After my residency in internal medicine at Dartmouth, I went back to McGill with a fellowship there in medicine. I've also had some training in psychiatry. It wasn't full training in psychiatry, but I sat in on seminars and participated. Professor J.S.L. Browne, the Chair of Medicine at McGill, invited me to stay on, but I had to support my wife and two children, and I'd have had to spend most of my time running around to different hospitals doing private practice, which was all right, but seemed to me to involve an excessive expenditure of energy for a pretty modest

income. JSL (as Browne was known) was enormous help by introducing me to Lester Evans of the Commonwealth Fund in New York. He generously took an interest in me. In fact, my visit to the Commonwealth Fund in 1949 started a relationship that has extended for almost fifty years. Evans discussed two or three possible openings that he thought might provide opportunities. One was the University of North Carolina at Chapel Hill where I was appointed an assistant professor of internal medicine and started my formal academic career.

Berkowitz: By this time you're on the academic track. Was that something that you had decided at some point along the line? On the one hand you're doing this clinical work at Dartmouth, but you've sort of decided that you're going to be an academic.

White: Well, yes. Actually I did some research while at Hitchcock and published two or three papers from there. I'm not sure I chose the academic path until JSL encouraged me. I hadn't consciously thought about it prior to that, although indirectly, of course, they'd encouraged me to do that at McGill when I graduated. At Dartmouth they asked me to stay on in the Hitchcock Clinic and faculty, but it would have been primarily a clinical job in medicine, although there was teaching involved and possibly some research. At any rate I thought I should get some further training and returned to McGill for the fellowship year. I can't say that I made a conscious decision in medical school or even before that to embark on an academic career.

Berkowitz: When you went to North Carolina you didn't want to be in public health? Did that cross your mind?

White: No. At McGill we had a former health officer from Ontario who was the professor of preventive medicine and public health. He gave a series of dismal lectures on how to dig a pit privy and how to can tomatoes so you wouldn't get botulism and other dreary subjects. I thought there must be more to this aspect of medicine than this guy was able to set forth. In fact I went to see him a couple of times and said, "What about this public health business?" and he said, "Well, it's so-and-so." He was a most uninspiring character. I took a pledge at that time that I wouldn't have much to do with public health as practiced by these kinds of guys. So the short answer is no, I didn't entertain the idea of an MPH. I did entertain the idea of spending the year with Ryle in his new *Institute of Social Medicine* at Oxford had it been feasible, but as I mentioned earlier, he died unexpectedly.

Berkowitz: Hadn't North Carolina just become a four-year medical school?

White: Yes, it had been a two-year medical school.

Berkowitz: It's interesting that that could be the case, a major academic center as late as the 1940s was just a two-year school.

White: It was a two-year school, and it became a four-year school in 1952-53; in fact, it was the first new four-year school after World War II. There had been no clinical faculty before when it was a two-year school, so we were among the first clinical faculty there, and it blossomed forth. There was no School of Public Health there, either. The latter was started about 1954 by Ed McGavran in the basement of the School of Medicine's building. I became friends with many of its new faculty, particularly John Cassel, a social epidemiologist who was a long way ahead of his time; a remarkable fellow. He was one of a group of refugees from apartheid in South Africa. They were disciples of Sidney Kark, a pioneer in developing the concept of community-oriented medical care. He wrote several books on the subject. He's in Israel now. I think he went back to South Africa for a couple of years and has been a visiting professor in various universities. I last saw him ten or a dozen years ago. In fact, he and

his wife were my patients when we were all at Chapel Hill. He had come there as the first head of its department of epidemiology.

Kark's group, all of whom immigrated from South Africa, included a number of remarkable people. Abe Adelstein was head of health statistics in the United Kingdom; Harry Phillips became a leading authority on rehabilitation in Boston; Mervin Susser of Columbia is a noted epidemiologist, and, of course, John Cassel. John and I became great friends; he died prematurely, it was very sad. Bernie Greenberg, another friend and colleague, was head of biostatistics in the School of Public Health. Bernie later became Dean of the School of Public Health. I learned a great deal from him. We were all good friends, and I arranged for John and Bernie to teach in the School of Medicine, and I used to participate in their seminars in the School of Public Health, and we all collaborated on research. But I had pledged to myself that I would not join the faculty of a School of Public Health. I felt that the real future was getting physicians to think more broadly about disease and medical care.

The greatest influence on me at that time was that the newly enlarged school, when it opened, had sponsored a conference in which Jerry Morris from London was the star attraction. He was head of the U.K. *Medical Research Council's Social Medicine Research Unit* at the London Hospital. Unfortunately the seminar took place prior to my arrival at Chapel Hill, but I heard all about it. Cecil Sheps was there. Although he later moved to Boston and then to Pittsburgh, he returned to UNC and eventually became Vice-Chancellor. An article by Morris published in the *British Medical Journal*, titled *The Uses of Epidemiology*, impressed me greatly. Morris demonstrated that epidemiological concepts and methods could be applied to better understand and evaluate the provision of health services. Among other things, he emphasized the importance of determining whether medical interventions of all kinds did more good than harm. He examined the impact on health and disease of a host of nutritional, environmental, occupational, and social factors. While all this was going on, the Rockefeller Foundation, represented by John Grant, one of its most creative officers, had offered to put up a building for us and to finance a prepaid faculty group practice to demonstrate its feasibility for providing medical care, conducting research, and educating medical students. All of this was back in 1953-54. I have a copy of the original proposal set forth by John Grant to the University of North Carolina School of Medicine. But this subversive plan was turned down by the medical faculty. It was really off-the-wall in those days.

Berkowitz: There were only a few of them, like Group Health in Washington, Kaiser in California.

White: Right. But Grant thought that group practice and prepayment would become the preferred mode of practice in the future and that an academic center should take this on and develop it, but the local and state medical societies were up in arms and threatened that terrible things would happen if these ideas were embraced by the new medical school. They lobbied the state legislature to oppose the plan, with the result that the medical school backed off; they feared that their state funds would be cut. That exercise had a powerful influence on me also.

Berkowitz: I'm curious about this. Before there was this four-year school at Chapel Hill, were there any four-year schools of medicine in North Carolina?

White: Duke probably had a four-year medical school, and probably Bowman-Gray, but I'm not certain about that. Duke almost certainly was going then, yes.

Berkowitz: So the idea was that medical education was considered to be a private school kind of thing. At the University of Missouri, I know, they had a two-year school for a long time. Maybe that was the idea, that the state school was not expected to have a medical school.

White: I'm not sure of that history. I don't know how many state universities had medical schools. California must have had some, but I just don't know.

Berkowitz: That's interesting. When I think of North Carolina, not being in medicine, one of their big things was rural sociology. Was the medicine sort of rural-oriented, too?

White: Well yes, to some extent. We did a lot of research with Reuben Hill, a member of the UNC Department of Sociology, and with Harvey Smith, a sociologist on the faculty of the School of Medicine. And there was Harry Martin, also a sociologist, whom I used to take on clinical rounds with me. He fainted once when he saw a patient with a little blood on him. We worked very closely with all of them. I used to attend the weekly meetings of the UNC *Institute of Sociology*; I think that was the entity's name.

Our group coalesced around Bernie Greenberg and me, primarily, and then there was Dan Martin, an internist, who worked with me as a Research Fellow; he later became a leading figure in the Trover Clinic in Kentucky. We also had Frank Williams, another member of the UNC Department of Medicine, who later became Director of the *National Institute on Aging*. He was a little junior to me. There was also Bob Huntley, who became Chair and Professor of Community Medicine at Georgetown. We had a nucleus of curious faculty colleagues from diverse disciplines and started to do what we called Medical Care Research. About 1955 the Hill-Burton Act included a million dollars for research on what it called "hospital and medical facilities". I saw a notice of this in the *Journal of the American Medical Association (JAMA)* and thought it was the kind of money we needed to get hold of, so we put in a grant application--W74 was the number--I remember it well, and we were awarded a fair package of money. That got us started on a study of patient referrals from general practitioners (family physicians as they are called now), to consultants or specialists at university medical centers in North Carolina.

There were two questions that intrigued me: Are we doing more good than harm in what we do for patients? And second: Are we responding to each patient's real needs, expectations, and what they want from the clinical encounter? These seemed to me not unreasonable questions, but apparently they hadn't occurred to many of the medical academicians of that era. So our research group embarked on this referral study. Bernie Greenberg, the statistician, helped us immeasurably. We drew a probability sample of all the General Practitioners in the state and interviewed nearly all of them. Then we had index patients who were referred to our General Clinic at UNC by each of the General Practitioners in our study. The physicians were interviewed by us, and the patients were interviewed both at the UNC General Clinic and at home by a social worker who was part of our team. We followed each one and kept track, among other factors, of the different referral patterns to Duke, UNC, and other medical centers. We had a very high response rate of about 96%, as I recall it. This was the first real referral pattern study that had been done. We published four or five substantial papers of one kind or another over a period of several years. The best known of these is *The Ecology of Medical Care* published in the *New England Journal of Medicine* in 1961. In that paper, among other matters, we introduced the term "primary medical care". Our referral study has been replicated numerous times with basically the same results. The most extensive study was completed a few years ago in Canada. They found essentially the same things we did. We documented massive miscommunication among the referring physician, the patient, the medical student at our Clinic, the attending physician, and sometimes other participants in the referral process. The General Practitioner referred the patient for one reason, the patient thought he or she was being referred for some other reason, the medical student determined the problem to be an entirely different condition, and then the resident and the attending physician considered something else to be the most important issue. The latter might be interested in thyroid disease and succeeded in finding a thyroid nodule, while the patient was really depressed but had been referred by the General Practitioner because of "back pain". Just massive miscommunication. This seemed to make news on various fronts.

Berkowitz: Is that really lack of communication? That sounds more like propaganda for rehabilitation medicine. They always talk about coordination of care and that sort of thing.

White: No, no. That was just one example, but there were many other kinds of poor communication.

Berkowitz: But the point is that the patient had all those things wrong, correct? It just depended on which doctor was looking at the patient.

White: Yes, which doctor would be looking at him. Yes, I guess all of those things could be wrong in one way or another but responding to the patient's expectations should be at the top of the list.

Berkowitz: The rehab doctor would say what you need is a coordinator of the team. I don't know how much in vogue that is now, but it was a big idea in the late 1940s and early 1950s.

White: In our articles we emphasized the need for everyone to have a general physician to coordinate his or her care. That's when we introduced the term "primary medical care" that I mentioned earlier. I have made a career out of expounding the essential role of "primary care" in any well-balanced health care system. Primary care is of central importance when considering the needs of the entire population. It should have a high priority when designing a medical curriculum, selecting research projects, and organizing health care services. I don't know whether you've run across our article *The Ecology of Medical Care* in which these ideas were set forth.

Berkowitz: Yes. That was published in 1961 in the *New England Journal of Medicine*. Is that right?

White: We illustrated our findings with a series of five nested boxes. We give credit for the nested boxes idea to an article by John and Elizabeth Horder that I came across when I was in England. They had looked at their own practice and described the distribution of patients who went to the hospital, were referred to consultants, and so forth. I thought we could do the same kind of analysis for whole populations, and we did.

Berkowitz: This grant that you got from Hill-Burton, you said was W74. Was that the name of your proposal or the number of the grant?

White: It was the number of the grant. I think the title was something like "A Study of Patient Referral Patterns in North Carolina".

Berkowitz: Now that you know a little bit more about the political process, how come there was that money? Was that Fogarty's idea? Was that Hill's idea? Fogarty was big on earmarking stuff in Hill-Burton.

White: It was Louis Block and his boss, Jack Haldeman, of the U.S. Public Health Service who initiated the idea. Block, I think, had been a hospital consultant, and he spoke to Fogarty and/or Hill and got them to put the funds into the Hill-Burton appropriation. To the best of my knowledge, that was the initial money for the field that eventually became known as Health Services Research.

I had been doing cardiovascular research and was interested in the effect of emotions on cardiovascular disease, particularly on cardiac failure. We had done a number of laboratory studies, extensive interviews with patients, and population-based surveys. In fact, our first grant submission got turned down by the NIH Cardiovascular Study Section. The National Heart Council under Tinsley Harrison, the author of one of the major text books on medicine, decided that my ideas were not all that crazy. He

came down to Chapel Hill leading a site visit group himself. From his clinical experience, he thought I was on to something. Our findings that emotional distress can play a major role in precipitating cardiac failure has later been confirmed by other studies. We got the grant to do laboratory work on the effects of emotions on venous and heart function. We gave a paper at an annual meeting of the *American Heart Association* that attracted considerable attention. The story was published in numerous newspapers across the country and in *Time*. A TV story received widespread airing, but I never saw it.

My interest in cardiac failure was not restricted to the laboratory or the bedside; it included the question: "Is this a big problem or a little problem in the population generally?" There was very little data available. I couldn't find any statistics on it because at that time it was not readily classified in the *International Classification of Diseases (ICD)*. One of the students from the School of Public Health, Michel Ibrahim, who is now the Dean of the School of Public Health at Chapel Hill, did his doctoral dissertation with me. We did an epidemiological population-based study that was published in the *Annals of Internal Medicine* in which we showed the distribution of cardiac failure in the entire population for the first time. We found that one percent of the population suffers from it at any given time, and that ten percent of those over 65 years of age are experiencing it. When I went to Vermont, we repeated the study and found the same kinds of distributions. Those studies, the first of their kind, were incorporated in Michael DeBakey's landmark report on *Heart Disease, Stroke, and Cancer* back in the 1960s. By studying this problem in the laboratory, the clinic or bedside, and the population, I think we showed that all three approaches can be useful in understanding the genesis, natural history, and distribution of health problems.

Berkowitz: By population-based you mean you look at data from a large number of people?

White: It is not just a matter of large numbers. Population-based studies encompass a base population that is generally the entire population within a defined geo-political jurisdiction but may be a sub-set of that population. Studies may be made from randomly drawn samples of the index population so that the bias of selection is minimized.

I learned from Jerry Morris, whom I mentioned earlier, when I spent a year with him in 1959-60, about the three ways you can examine health problems. You can work in the laboratory, which is fine for certain problems and questions; you can study patients in the clinic or hospital in the one-to-one encounter between the physician and the patient; and then you can do epidemiological studies of the population. One isn't good or bad, right or wrong, soft or hard. It depends on the question and the problem that you're tackling. Some things can best be observed in a population. You can see what the distributions are, because it often takes large numbers to detect the factors of interest, and you also want to include not only those who seek medical care and come to either a physician or a hospital, but also those who don't seek care, who may or may not need care or have some disability.

Berkowitz: So if you want to do an epidemiological study, you need to do a population- based study.

White: Right. There are exceptions, however.

Berkowitz: A disease study? Maybe you do a clinical study?

White: The notion that you should use only one approach to understanding diseases is one of the problems that has resulted from the separation of schools of public health from medical schools. The split was the doing of the Rockefeller Foundation in 1918 when the well-meaning officers of that day took away the population perspective from the clinical and the laboratory or biomedical (as the latter is called today) perspectives.

A volume I wrote several years ago, *Healing the Schism: Epidemiology, Medicine, and the Public's Health*, deals with the history of this unfortunate separation. Clinicians employed all three approaches previously, but then suddenly the population perspective was hived off into separate bailiwicks--schools of public health--starting with Johns Hopkins. I think this well-intentioned initiative did more harm than good. It resulted in those investigators and teachers familiar with the population perspective being separated intellectually and culturally from the biomedical investigators and teachers who were making the extraordinary advances in our understanding of disease processes. The latter, however, were contributing much less to our understanding of the multiple social, behavioral, psychological, environmental, occupational, and nutritional factors that influence the maintenance of health and the genesis of ill health.

I was going to tell you about the ecology paper. We had grants from both the Rockefeller Foundation and the Commonwealth Fund to support our General Clinic. We were providing medical care for the general population in an effort to re-establish the role of the General Physician. Chuck Burnett, Chair of our Department of Internal Medicine and a strong supporter of our work, was off sick, and his number two, Lou Welt, was in charge. Lou was a renowned nephrologist and wrote one of the classic texts on the subject. He was very much of a reductionist and seemed to some of us to be becoming increasingly narrow in his thinking about medicine and medical education. I remember going to see him and saying: "Officials from the Commonwealth Fund are coming from New York to do a review of our teaching and research to determine whether they should re-fund us. We need to write a report." "Oh", Lou said, "just tell them we spent the money!" I was mad as hopping hell, and I remember going back to my office and saying, "These highly specialized guys just don't understand what the problem is. It's not only the tertiary care that needs to be focused on in the medical school, but there are all those people out there with other kinds of disabilities and problems who don't necessarily come to university medical centers but require understanding and care. Somehow we have got to prepare physicians to care for their needs also." So I took American and British population-based data and our data from our own referral studies and created the nesting squares diagram and wrote *The Ecology of Medical Care*. Bernie Greenberg, my friend the biostatistician, helped to get the sizing of the squares mathematically correct. That's the article in which we introduced the term "primary medical care."

Berkowitz: To mean what?

White: To mean the fundamental, basic, initial, and continuing medical care that provides the underpinning of any balanced, effective, and efficient health care system organized to meet the needs of all the people. You need primary care and secondary care, and you also need tertiary care. In late 1960 or early 1961, Richard Scott, a General Practitioner whom I first met in Scotland in 1959, had established at the University of Edinburgh the first General Practice Teaching Unit in the world. It was supported by the Rockefeller Foundation. The Foundation established a similar Unit at the University of Manchester under Robert F.L Logan, whom I also knew very well. John Brotherston, who was then Dean of the Faculty of Medicine at Edinburgh, had established Scott's Unit, created an endowed Chair, and made Scott the first Sir James Mackenzie Professor of General Practice. His was the first Chair of General Practice in the world. James Mackenzie is the guru and hero of family physicians and general practitioners the world over. His story is an important one; every medical student should read his two biographies: *Beloved Physician* by McNair Wilson and *Sir James Mackenzie MD: General Practitioner 1883-1925* by Alex Mair.

At any rate, Scott and I were both preparing working papers for a *World Health Organization Expert Committee on the Future of the Family Doctor*; in other words, the future of general practice. He came to Chapel Hill, and we were in the bar of the Carolina Inn having a drink before dinner. Scott asked: "Why is general practice declining so much in the United States?" I replied: "General and Practice are now two dirty words in academic circles. The medical school faculties don't want anything to do with

preparing generalists. Specialization is the cry of the day. Most contemporary medical faculty members don't seem to understand the need for generalists. We need some other term for this essential component of medical care." Scott and I discussed terms such as "basic care", "general care", and "fundamental care", and I came up with the term "primary medical care" during that conversation. Then Bernie Greenberg, Frank Williams, and I put the term into the "ecology" article later on. Unbeknownst to me at the time was the famous Dawson report; I only learned about it in the middle-1970s. Lord Dawson, the Chief of the British Army's Medical Service during World War I, had been commissioned by his Government to prepare a report on the future structure of that country's medical services in preparation for a possible *National Health Service*. In his landmark Report, published in 1920, Dawson talked about "primary health centers" and presented diagrams that showed how these centers radiated into larger secondary medical care centers; there were also to be central teaching hospitals at the hub of each complex or region. So the notion of primary health care had been introduced earlier, although I didn't know it at the time that we introduced a similar term and concept.

Berkowitz: I have another candidate for you, too. Are you familiar with Oscar Ewing, the Social Security Administrator and health insurance advocate. He had a similar chart, maybe in the Magnuson Report, or one of these reports, that was exactly the same. The university hospital in the center and the other hospitals around it in concentric circles, so that idea was around. Howard Rusk and similar types of people were very aware of this kind of idea.

White: Another was John Grant at the Rockefeller Foundation. He was born in China; I think his parents were missionaries. (His son, the late Jim Grant, was the head of UNICEF.) John Grant had seen something of the distribution of health care in China. One of his close friends and also one of my long-time friends was Professor "CC" Chen of Chengdu. I still correspond with him two or three times a year; he's now in his 90s and was widely regarded at one time as Mr. Medical Care of China. In his younger days he established China's first systematically organized rural health system in Dingxian. I think John Grant may have got the idea of regionalization from his knowledge of Chen's pioneering work. From China, John Grant went to Saskatchewan, where he helped the government introduce a form of regionalization. He then went to Puerto Rico, where the emerging University of Puerto Rico with its innovative medical school was being developed. He helped design the regionalization patterns on that island. There was the medical school at the center with more peripheral referral centers, and then primary care health centers. Puerto Rico was the first place in North America, apart from Saskatchewan, that really implemented the concept of regionalization. So these ideas were floating around with origins in Britain, China, Saskatchewan, and Puerto Rico. I don't know the exact origin of them. Remember, as I mentioned earlier, it was John Grant of the Rockefeller Foundation who offered to fund the establishment of prepaid university medical school faculty group practice for us in North Carolina.

Berkowitz: But the term "primary care" is your term? Other people had these ideas, but they didn't use that word.

White: As far as I know we originated it - certainly in the United States. Dawson had used the term "primary health center," although unbeknownst to me. But we introduced the term as a substitute for general practice. In retrospect I think we should have employed some other term. I now prefer the concept of "general medicine" and the term "generalist physician". The contemporary American arrangement where we have internists, pediatricians, and family physicians all doing more or less the same thing seems pretty bizarre from the viewpoint of other countries.

Berkowitz: Tell me about the "ecology" article now.

White: Well, we completed the paper and sent it to the *New England Journal of Medicine*, and I remember the editor, Joe Garland, calling me. I was at a meeting in Atlantic City and had to reply

through a pay phone. The only thing he objected to in the article was the title, *The Ecology of Medical Care*. I said something to the effect "but that is what it's all about, the word 'ecology' in the title has to stay. Accept that title or we'll submit the article elsewhere." So Garland graciously accepted our title and published the article in 1961. Over the decades our analyses have been replicated in diverse settings, and our original findings have been supported.

Berkowitz: The idea that I took away from looking at that graph is that you start with a funnel. There's a lot of people in the general population, some of them get sick, and very few of them end up at Johns Hopkins. That's the point, right? Yet Johns Hopkins is training people just to see people who are at Johns Hopkins. Do I have the right idea?

White: Yes, absolutely. I recall a conversation back in 1975 or 1976 with Victor McKusick, a distinguished Professor of Medicine at Hopkins and at that time Chairman of the Department of Medicine. I said, "We have a terrible problem with the distribution of specialists in this country and also with the mix of generalists and specialists." "Yes," he replied, "it's just terrible and getting worse." "What is Hopkins going to do about it?" I inquired. "Nothing", he replied. "Why not?" was my response. "Because all of our specialists get good jobs," he said. That's the Hopkins faculty's view of the medical world.

Berkowitz: That article was published in 1961, and that really became one of your signature pieces.

White: Apparently, yes. I didn't realize it at the time, but it caught on. Of course, we got some pretty angry mail initially from various people who wanted to know: "What is this 'stuff' all about? Teaching centers are where the 'real' medical action takes place and we should focus on the needs of the seriously ill patients". But, as I said previously, our studies have been replicated in many situations and the basic relationships seem to hold. In 1973, I wrote the lead article for an issue of *Scientific American* devoted to health care. My contribution titled *Life and Death and Medicine* used annual, rather than monthly, figures from the *U.S. National Center for Health Statistics*. They continued to support the relationships in the original "ecology" paper. The *Scientific American* artists drew the boxes in three dimensions, and they have been copied frequently.

Berkowitz: Is that changing now? For example, I had a thyroidectomy a couple of years ago that was done at Hopkins. It wasn't a particularly fancy thing, but it was done at Hopkins, and they seemed really eager to have me come there. They're hungry for people to come for these routine operations.

White: In the early 1960s, a friend of mine, Don Anderson, formerly at the University of British Columbia as Professor of Epidemiology, did a study of referral patterns in British Columbia to the major Vancouver teaching hospital. He had all the specialists in the hospital, the principal tertiary care center for that province, state which patients absolutely needed to see a hospital-based specialist. When the smoke cleared, it looked as if 100, and certainly no more than 200 beds, per million population could take care of all that province's tertiary care. So the short answer is that these large over-bedded teaching hospitals are indeed hungry for patients.

Berkowitz: Mine certainly wasn't a tertiary care situation. It was just something that could have been done by almost any surgeon.

White: This is where the primary care and the so-called gate-keepers come in.

Berkowitz: The argument was made to me that if it had been malignant, wouldn't I like to go to the tumor clinic. He was hoping I would get into the next stage, in which case I would be tertiary care. But,

like most of these things, it wasn't; it was just perfectly normal.

White: They say, "You'd be in superb hands here; terrible things might happen out there if you let somebody else do it."

Berkowitz: That's interesting. Let me ask you another particular question. You said that when you were talking to the editor about the "ecology" article you were in Atlantic City. What meeting was this?

White: They used to be known as the "Atlantic City meetings" and included the *American Society for Clinical Investigation*, the *American Federation for Clinical Research*, the *Association of American Physicians*, and perhaps a couple of other lesser medical research organizations. The collective meetings went on for four or five days.

Berkowitz: And this was where academic doctors went?

White: Yes. This was the research academicians' intellectual highlight for the year.

Berkowitz: I had an interview on another project with Robert Glaser.

White: Oh, yes, I know Bob Glaser.

Berkowitz: He's a real political type. He talked about how good those meetings were in Atlantic City. He said your meetings were these meetings. You didn't go to the *American Medical Association* or the *American Public Health Association*.

White: Yes, well, I did go to those later on, some of them, and I gave talks at meetings of both the *American Medical Association* and the *American Public Health Association*. I was on the latter's Governing Council for several years and involved in its *Medical Care Section* for a number of years. I became disenchanted with their lack of influence on public policy and disinterest in research into the adequacy of health services. I promulgated what I have called "**White's Rule**", namely "that the influence of any organization is inverse relationship to the number of resolutions it passes". The *American Public Health Association* used to pass resolutions such as: "Thou shalt stop smoking," "Thou shalt do this and not do that," but nothing changes. It seemed to me that clinicians were where the action and power were, and that you needed to get them on board to encourage significant behavioral and lifestyle changes; all these declarations and resolutions were achieving nothing. I was also on several committees of the *American Medical Association* off and on. They were none too effective either.

I should emphasize that the research and thinking of Jerry Morris whom I mentioned earlier attracted me greatly. In 1959, I applied successfully for a Commonwealth Fund fellowship to England where I spent a highly profitable year, primarily with Morris, but also attending the *London School of Hygiene and Tropical Medicine* taking courses in epidemiology and statistics from Bradford Hill, Donald Reid and others. Bradford Hill was the doyen of health statisticians and developed the initial design for the Randomized Clinical Trial, which, as you probably know, is the gold standard for the current emphasis on Evidence-Based Medicine. Richard Doll was also on the staff at that time as an instructor; he is now Sir Richard Doll, an eminent epidemiologist with a global reputation.

I also visited a number of other leaders in British epidemiology and related activities, including Archie Cochrane of Cardiff, Roy Acheson and Walter Holland of London, Tom McKeown of Birmingham, Bob Logan, Douglas Black, and Robert Platt of Manchester, John Pemberton of Belfast, Donald Acheson of Oxford, Douglas Baird and his distinguished wife of Aberdeen, and John Brotherston of Edinburgh. All

except Acheson, Holland, Logan, and Black have since died. I also visited a number of prominent General Practitioners or "primary care" doctors, including John and Elizabeth Horder, John Fry, Stuart Carne, Phillip Hopkins, and John Hunt (later Lord Hunt, founder of the Royal College of General Practitioners) all of London, Tev Eimerl of Manchester, and Richard Scott of Edinburgh. And many others. I became friends with Michael and Enid Balint and attended a number of the latter's renowned clinical sessions with General Practitioners. There were many other visits, all recounted in my rather full report to the Commonwealth Fund on completion of my sabbatical. I learned a great deal from all of them. Have you heard of the Cochrane Collaboration?

Berkowitz: No.

White: I visited Archie Cochrane. He was a wry Scot with a glorious sense of humor. I remember visiting him in Cardiff. He asked me about my activities, and I replied that I was interested in applying epidemiological methods to the study of what we called medical care in those days but now refer to as "health services". He thought that was a bad idea, and that epidemiology should have a rather narrow focus on infectious diseases but could be applied to the study of chronic diseases. The application of epidemiological concepts and methods to the study of health services was, in Cochrane's view at that time, inappropriate. The British epidemiologists emphasized the investigation of chronic diseases, in spite of the fact that the infectious disease epidemiologists, largely American and eastern European, weren't about to accept such distortions of the "discipline" initially. I learned from Archie Cochrane the value of studying relatively large populations by means of carefully drawn samples and the importance of maintaining contact with them for repeated surveys over the years. He also stressed the importance of double-blind randomized clinical trials and of studying the distribution of so-called "normal" phenomena. For example, the textbook standards used in the laboratories of the day provided "normal" values for various blood elements to establish the presence of clinical anemias. Unfortunately these "normal standards" were based on samples drawn from volunteers found in medical centers, often sick patients. Sometimes the volunteers were from the staff or students or from otherwise "healthy" patients. The standards were not derived from a large general population base. Archie and one of his colleagues did population-based studies of the distribution of hemoglobin levels and of the different kinds of anemia, so the medical profession had more accurate and realistic knowledge of what to call "normal". This is the kind of thing that John Ryle of Oxford addressed in his essay on the question: "What is normal?" that I mentioned earlier. I learned a great deal from Archie Cochrane.

I should continue with this story because later Cochrane started looking at health services and wrote a classic little volume, *Effectiveness and Efficiency: Random Reflections on Health Services*, published in 1971 by the Nuffield Provincial Hospitals Trust. It attracted a lot of attention at the time, but the fundamental message was not readily accepted by the medical profession. Cochrane set forth the urgent case for examining carefully the evidence supporting the decision that each medical intervention did more good than harm; he cited numerous examples to the contrary. For example, he showed that the consumption of Vitamin B-12 in Britain was something like 8 or 9 times the amount required to treat all the cases in that country of pernicious anemia, for which it is specific. The rest of its use was just acting as an expensive placebo. Cochrane quoted one observer of the passing scene who commented that the health services reminded him of a crematorium: so much went in and so little came out!

I was most impressed with Cochrane's little volume; it was a pithy presentation of some extremely important concepts bearing on medical practice. In 1959 I had met Gordon McLachlan, the Secretary, as he was called, although he was really the president of the Nuffield Provincial Hospitals Trust. McLachlan had commissioned and published Cochrane's little treatise. I arranged with Gordon to get about 200 (it may have been considerably more) copies of *Effectiveness and Efficiency* for free distribution at the 1974 meeting of the *Institute of Medicine*. And that's how we launched Archie Cochrane in this country, I think, and many aspects of the now burgeoning concern for the quality of

medical care.

In the early 1990s Iain Chalmers and his colleagues in Oxford established what is known as the Cochrane Collaboration. There are now units around the world that search the medical literature for articles describing clinical trials to see what forms of intervention are supported by evidence, that in fact they are more efficacious than others. They are building up enormous data bases and starting to have a huge impact on the practice of medicine. I had suggested to Iain Chalmers initially that they should do like McDonald's and franchise the Cochrane Collaborating Centers all over the world, and that's essentially what they've done. So that's the global Cochrane Collaboration. Professor Kay Dickerson at the University of Maryland is the key anchor point in this country.

Berkowitz: It's interesting that you don't talk about economics much. You have this training in economics, and what you're talking about is like a cost-benefit study in economics.

White: I encountered that in Britain too. The first cost-benefit study that I came across, one of the first that was ever done, was for the Victoria Line of the London Underground. That's where a lot of the concepts and methods surrounding this topic were introduced. Yes, the economics of medicine and health services have always interested me. Of course, the British *National Health Service* fascinated me from its earliest days. I have copies of the original Beveridge Report and the initial British Government's **White** Paper setting forth the goals of the new *National Health Service*--both published in the early 1940s before World War II ended. In the 1960s the *NHS* was already 12 years old, and we were still hassling around in this country about what should be done to increase the availability of health care.

Berkowitz: They're still hassling around in this country about what could be done.

White: Oh, yes. In fact I was just reading *The Economist*--my favorite news magazine for almost seventy years. They recently had an extensive article on health care. There's just mayhem in this country at present .

Another contribution of the British to the evolving field of health services research was the establishment of its first scientific journal. Abraham Marcus, also a friend of mine whom I met first in 1959, was the medical reporter for the London *Observer*. He started the journal *Medical Care* in 1962. Pitman was the publisher, but about 1965 they decided that the market was not large enough to support such a journal. I was on its original Editorial Board and negotiated a transfer of the publication rights from Pitman, the British publisher, to Lippincott in this country. The *Medical Care* section of the *American Public Health Association* assumed responsibility for the journal. I was asked to take over as editor but had my hands full with other responsibilities at Johns Hopkins.

Berkowitz: So what gets published in *Medical Care*? Who publishes in that, academic doctors or epidemiologists?

White: Epidemiologists may or may not also be academic doctors. So-called health service researchers, some of whom are epidemiologists, are the main contributors. It's a research journal primarily; not so much descriptive. Most of the articles are peer-reviewed research. *Medical Care* just came out with a special supplement on the down-sizing of hospital staffs, especially by reducing the ratio of registered nurses to patients and the substitution of minimally-trained and often unlicensed "aides". I wrote the introduction; Linda Aitken is directing this large international study from her *Research Center* at the University of Pennsylvania.

One of the first articles published in *Medical Care* was on prescribing habits and patterns of all the doctors serving an entire northeast English town. To make a long story short, they found that only about 10% of the prescriptions were definitely specific for the purposes for which they were prescribed. Another maybe 15 or 20% were probably useful, and about half of them were of doubtful value completely. It seemed to me that these observations cried out for further investigation. I took that 10% figure and bandied it around a good deal in various articles and talks. It was immortalized when I was in the Advisory Committee of the Congressional *Office of Technology Assessment* in a lot of their literature and quoted in many places.

In 1976 Archie Cochrane and I were doing a kind of dog and pony show in New Zealand. At a joint affair at the Wellington Hospital the audience consisted largely of **White**-coated, British-trained *pukka* clinicians, and I didn't want to startle them too much, so I said, "Only something on the order of 15% or possibly 20% of all interventions by physicians have any objective evidence that they do more good than harm." Cochrane, in the middle of my sentence, interrupted me saying, "Kerr, you're a damned liar. You know perfectly well it's not more than 10%."

Berkowitz: What do you do with information like that? So you can prove that the interventions are not always effective, or on the average they're not effective. So what? What does that get you?

White: Well, let's jump ahead to students of ours at Johns Hopkins like Jack Wennberg and Bob Brook. Let's take Wennberg first. Jerry Morris introduced me to the work of an English physician named Glover. In the 1930s Glover published a series of articles reporting vast differences in tonsillectomy rates for children in comparable British provincial cathedral towns. I thought these findings demanded closer scrutiny. How come all these differences? Either the kids are sicker, or the doctors are different in the various towns studied. There was something strange going on. This was before there was a *National Health Service* in the U.K., so perhaps it could be explained by differences in the access to care. These findings became known as the *Glover Phenomenon*.

When I returned to the University of North Carolina, I suggested to the medical school faculty that we look at our hospital's discharge abstract data using a system devised by Virgil Slee in the *Professional Activities System (PAS)* organization in Michigan; it had been supported by the Kellogg Foundation. Unfortunately his abstracts had far too much data and not enough information, but it was really the only system around at that time. I suggested that we try to install this at Chapel Hill. I was laughed at. My colleagues said, "You obviously don't know where you are. This is a university teaching academic center. We are the gold standard for medical care; when we say it's so, why it's so. We don't need all this kind of stuff." I replied, "Well, if it's so, then I'm like Alice in Wonderland; I would just as soon see it done on paper." But they wouldn't let me. So we embarked on an analogous, although simpler, abstract study, in our hospital's outpatient General Medical Clinic. We thought we ran a pretty tight ship down there and were giving what we thought was exemplary care, and that we were keeping careful records. When we examined our results, we found all kinds of room for improvement: laboratory results that were overlooked, errors that were made, things that weren't done, patients who didn't keep appointments with their referring physicians, or didn't return to our Clinic as scheduled. We published our findings in what was then the *Journal of Chronic Disease* but is now the *Journal of Clinical Epidemiology*. Bob Huntley was the lead author. I think ours was the first article on the quality of care in an ambulatory setting.

I became increasingly interested in this problem and wondered what the findings would be in a broad-based study in a general population. When I was offered a job at the University of Vermont in 1962 as head of what I think was the first medical school department that used the term "epidemiology", I saw it as an opportunity to do population-based research in a relatively small state. We called it the *Department of Epidemiology and Community Medicine*. Yale in the early 1960s had referred to its

School of Epidemiology and Public Health which, in turn, was administratively part of the School of Medicine, but it was essentially a school of public health.

Berkowitz: That department at Yale was quite distinguished. A lot of people had been there, for example those people on the Committee on Costs in Medical Care. Falk had been there.

White: Yes, absolutely. He was a good friend of mine. I'm not sure he always got the credit he deserved.

Berkowitz: He was probably a very abrasive guy. I've never met him.

White: Yes, he was somewhat. I remember I was involved in an NIH grant site-visit with him once, and he didn't handle the visit very well with the Study Section's representatives. He rubbed some of the people the wrong way and got kind of a rough deal, I thought.

Berkowitz: Yours was the first Department of Epidemiology and Community Medicine?

White: As far as I know. At any rate, I was attracted by a small state with a relatively stable population and the prospect of installing a state-wide population-based hospital discharge data system. So when I got there, we proceeded to do just that. I was given a huge budget by the university and also received a large Federal NIH grant to accomplish my objective. It turned out to be a much more difficult task than I had envisaged. We had quite a time. I was called a communist by the medical society, and I was hauled up before the trustees of the medical society. I talked to rooms full of lawyers, hospital boards, and administrators. But eventually we did install Virgil Slee's *Professional Activities System (PAS)* in all of Vermont hospitals. I had several long discussions with Virgil Slee, the founder of *PAS*, in motels and assorted other venues.

When I went to Hopkins, Jack Wennberg was one of our first students. On graduation, he was looking for a job. I said, "Why don't we get you up to Vermont, and you can look at the hospital discharge abstract data for the entire state". Wennberg took the job in Vermont and started to mine the data. He examined the distributions and variations in the rates for interventions and procedures, particularly in Medicare patients, in different geopolitical jurisdictions. He and Alan Gittelsohn, a statistician at Hopkins, were friends and worked together on the initial studies. Then Wennberg examined data from Maine and New Hampshire. He is now an international authority on these matters and heads an innovative *Center for Evaluative Clinical Sciences* at Dartmouth College. Unfortunately, he never seems to refer to Glover as the originator of the basic concepts he has so elegantly applied. To hear Jack tell it, the whole idea emerged with him. I think it's important to give credit to the person and place where it arose. The initial observation that there are large geographic variations in a wide variety of clinical maneuvers came from Glover, and I'm pretty sure I learned of the concept from Jerry Morris in 1959 and brought it to this continent. Nobody else that I know of was discussing it.

During my sabbatical in England, I also learned about their *Hospital Activities Analysis* system that George Godber, then Britain's Chief Medical Officer, introduced. I thought it was most interesting. They collected a minimum amount of data on all hospital discharges in Britain, but unfortunately they didn't analyze it effectively at the time. In fact, the idea of collecting data on all hospital discharges came from Florence Nightingale, the nurse. Observing, caring, and counting were her hallmarks. She introduced the idea of a model hospital discharge abstract in the 1860s. It takes a long time for a new idea to be accepted by the medical profession!

Berkowitz: Did you ever worry, you do all this research, and you have this control over what the

hospitals and doctors do, and you can judge, in some sense, the qualities of care. That leads directly to what we have today, which is managed care. Which you probably don't like?

White: Well, I like it and I don't like it. I was on the board of the first for-profit HMO, *HMO International* in Los Angeles in about 1970, and I was upbraided by my academic colleagues who thought that this was a terribly bad idea. I said, "I'll know more about it after I've been on the board and seen what goes on and learn about it." I had it vetted by Bob Huntley and also Peter Lee, Phil Lee's brother. They checked it out for me before I joined the Board.

Berkowitz: I never heard of him before, Phil Lee's brother. Was he a doctor? That's quite a family.

White: Yes. Oh, his father was a great person, too. He founded the Palo Alto Clinic, and, among other important contributions, he urged this country to adopt national health insurance. Peter did a study of the Commonwealth Fund's grant initiatives to implement what its staff called "comprehensive care". The fund was the principle source of support for our educational and research programs in the UNC General Clinic I mentioned earlier. Our activities were discussed at some length in Peter's volume for the Commonwealth Fund.

Returning to my joining the Board of *HMO International*, I also asked a professor of medicine at Stanford to vet the quality of care given by *HMO International* before I agreed to join its board so I could get a pretty good idea of the whole enterprise. I also asked Nathan Stark to join me on the Board as one of the two outside directors, and he agreed. Nathan had been a Vice-president of Hallmark cards and was largely responsible for developing the Truman Medical Center in Kansas City. He became Vice-Chancellor for Health Affairs at the University of Pittsburgh and later Under-Secretary of Health, Education, and Welfare in the Federal Government. I must say we both learned an enormous amount from being on that Board. We encountered some pretty greedy SOBs who were running the enterprise, but most of the clinical care was fairly good. Among other measures to improve the quality of services, they were introducing sensible and timely information systems for monitoring care. Eventually the company was bought out by CIGNA and became the nucleus of the *CIGNA Health Care System*. On the whole I learned a great deal about many aspects of HMOs and so-called "managed care" in its earliest stages. That was more than 25 years ago.

In 1967 I gave a talk at the annual meeting of the *American Association for the Advancement of Science* in New York. Gerry Piel, the editor of *Scientific American*, asked me to do it. Among other things, I suggested that the United States would end up with a "fourth party" involved in the organization of health services. There would be the "purchaser", either an individual (as a patient or potential patient) or an organization on behalf of its employees or members, as the first party; the so-called "provider" of care as the second party; the "insurer" as the third party; and the "manager" as the fourth party. I said also that this country would eventually experience vertical consolidation and integration of care and services. These emerging arrangements would not include a national health service as in Britain but would probably end up with something on the order of six or eight national health organizations or systems.

Berkowitz: That's like Clinton proposed.

White: Something like that, but the entities I envisaged would be huge. We'd follow the model of the airlines in their gradual consolidation from bush airlines and "barnstormers" to regional airlines to larger and larger national systems. Eventually you might have choices, for example, of American, United, or Continental Health Care Systems, and within that you would have a choice of clinics, and within them a choice of doctors. You wouldn't have infinite choices, probably. Above all, the systems would be

efficiently and effectively managed so that there was seamless service from home care through primary care to secondary care in community hospitals, and tertiary care in the largest medical centers. This is where my background and interest in health economics comes in. From what I have learned about hospitals over the decades, many are abominably managed. I still think they are, even our local university hospital. Their management practices are atrocious. There is enormous waste, inept communication and organization, and poor staff morale. Industry has known about these elementary managerial principles for sixty years or more.

The Economist says that there are now about 1,000 HMOs, and they see them shrinking in a decade or two to about 30. My figure was about 6 or 8. I don't know whether it's 6 or 8 or 30, but my guess is that's the direction consolidation will go. In my AAAS talk, I suggested that some of these mega-systems would be owned by insurance companies, some would be owned by labor unions, some would be owned by government, some by some non-profit entities, and others possibly by for-profit corporations. I also recommended that there be two organizations, one the equivalent of the old CAB [*Civilian Aviation Board*] to see that there was adequate coverage. If you're going to receive a franchise in Baltimore, you're going to have provide services in the rural areas of Maryland as well. Secondly, I recommended that an entity equivalent of the FAA (*Federal Aviation Administration*) be established to monitor the quality of care. So it wasn't too far removed from Clinton's initiative. Their effort was thoroughly misguided, however. Clinton should have given the Congress four or five major principles bearing on the provision of health services and then let them craft the ways, means, and laws. The principles might include: universal coverage, portability across the country, comprehensive coverage, free at the point of service, and paid for through a mix of individual premiums, employer contributions, and state and Federal general taxes. That is what happened with the Hall Commission in Canada. Several of my former students were involved in the Clinton effort, so I had some insight into what they were doing.

But about the HMO idea. Paul Elwood and I were at one of the *Sun Valley Forums* sponsored in part by the Commonwealth Fund in the 1970s. The general theme as I recall it was the optimal organization of medical care. I put forward a paper, which he liked. They were apparently new ideas to him. I set forth many of the concepts expressed in the AAAS paper that I described earlier. I outlined a hierarchical arrangement and the urgent need to manage services more effectively and efficiently and to provide seamless services with respect to records, communications, and the transfer of the patient between different levels of care and facilities. He liked the ideas. A few weeks later we had a two- or three-hour telephone conversation one Saturday afternoon. He was discussing the idea of getting the HMO or Health Maintenance Organization notion going. I didn't generate the term; he generated the term, but the concept, I believe, came from this *Sun Valley Forum*. To give credit where it is due, even my ideas were built in turn on the *Kaiser Permanente* experience. It was the first large-scale HMO.

Berkowitz: Group Health in Washington was older. You talked about the Placebo Effect in regard to your work. Do you have a judgment about that? Do you know the book *Persuasion and Healing* by Jerome Frank, who was your colleague at Hopkins? He talks about the Placebo Effect. His idea is that it's not so bad. If it works, that's fine. If people take a sugar pill and it makes them feel better, we shouldn't discount that. You don't, do you?

White: I think it's the most undervalued ubiquitous therapeutic intervention that we have. It has an all-purpose, extremely powerful effect, and it's "caring". It's a manifestation of love, if you like.

Berkowitz: So you're not averse to that?

White: Not averse to it? I'm very positive about it. In fact, I wrote John Eisenberg, who is an old friend of mine, when he became Administrator of the *Agency for Health Care Policy and Research*. Cliff Gaus, a former student in my department at Hopkins, had been the previous head. When Cliff went back to

civilian life, John Eisenberg took over. I wrote him and said, "There are two things I think you need to look at. One is the Placebo Effect, and the other is the current downsizing of registered nurses and their replacement with minimally-trained, inexperienced, poorly-educated, and unlicensed "aides". It's an outrageous way to "save money".

The Placebo Effect needs to be looked at much more. There is more and more literature coming out. A Professor of Gastroenterology at Yale, Howard Spiro, wrote an interesting book on the Placebo Effect. He showed that there were large differences between London and Dundee in the healing rates and the Placebo Effects for identical treatment regimens for peptic ulcer disease, although the same protocol was used in both places. Big differences also were found between England and the United States in the healing and Placebo rates for peptic ulcer disease, although again they were using the same pills and the same protocols. Clearly there was something else in addition to therapeutic regimen and the Placebo Effect. It was probably the Hawthorne Effect, which, in turn, is related to an organization's overall culture, the managerial climate in which the hospital operates, including administrative attitudes towards the employees and the nurses, and the general reputation of the hospital and its doctors. This is transferred somehow so that it influences the entire institution's overall healing rates. Over thirty years ago Professor Reginald Revans of the University of Manchester, supported and published by the *Nuffield Provincial Hospitals Trust*, showed this in a classical set of studies in different hospitals and for different conditions. The more authoritarian the administration of similar-sized hospitals, the greater the labor turnover of nurses and other employees, and the longer the patient's hospital stay for six common medical conditions and six common surgical conditions. The converse was of course true.

Berkowitz: Although that's pretty hard to replicate, isn't it? The object of all this is to replicate, right? The only reason to do any of this research is that you're trying to generalize and then replicate elsewhere, correct? Maybe it's just that one guy is better, that one physician has a better touch than the other, which may not be replicable any more than a model school is replicable elsewhere. It might just depend on one teacher. We talk about the Hawthorne Effect in a generalized way, but it might have been particular to that branch of Western Electric and those experimenters from the Harvard Business School or wherever they were from, that they were warm toward them, that they felt wanted.

White: I think this should be examined; the phenomenon should be studied much more in health care settings. It is, of course, widely accepted in industry, commerce, and other organizational settings. Hospitals seem ignorant of it. We should look at the various components of the Placebo and Hawthorne Effects to determine how they influence immune and other humoral and endocrine responses, and neurotransmitters, in both the doctor and the patient. Some of this has been done in various places. I don't know whether you're familiar with quarterly journal *Advances* published by the *Fetzer Institute* in Michigan. Many of these issues are discussed at some length. There needs to be much, much more research done on them.

Berkowitz: I hear a very optimistic view that science can win out here, that we can actually understand this. Some people would say we really don't understand this. One guy is a good country doctor, we don't know why he's good, and we're never going to know why. It's utopian to think that we're going to be able to understand the brain chemistry that explains why people are receptive to someone. You're pretty upbeat about this.

White: Oh, yes. It needs a lot of work, but it's a legitimate area for inquiry. In fact Larry Green, a protege of mine who's head of the Department of Family Medicine at the University of Colorado, was awarded the Curtis Hames Prize this year. He sent a questionnaire around asking, "What do you think about research in family medicine?" I said, "I don't think much of it. I think you're really missing the boat." Instead of talking about appointment breaking, patient loads, residency programs and a lot of ephemeral epiphenomena, they should really be looking at the natural history of disease and what it is

about certain doctors that results in cures. They should be studying the origins and the natural history of diseases like cardiac failure or peptic ulcer. They're on this big kick now of antibiotics for peptic ulcer because they found a bug that's there. Well, there are bugs floating around all over the place, but anyone who's talked to a patient with peptic ulcer knows they're also under great stress and strain when the ulcer becomes symptomatic. So it's not one thing or the other. It's a combination of factors. There is a web of causality. This needs to be teased out. There's a whole area of research here that can best be done in primary care settings, but that is, for the most part, completely neglected.

Berkowitz: I see.

White: We were back in Vermont before these digressions. Let me return to Bob Brook. In 1959 in England, Jerry Morris had done studies comparing teaching and non-teaching hospitals and found that the case-fatality rates for different conditions such as peptic ulcer, myocardial infarction, and enlargement of the prostate gland varied greatly between the two types of hospitals. So what accounted for the variations? Were there differences in referral patterns, differences in the severity or complexity of the patients' problems, or differences in the quality of the care that the patients were receiving? What one thing or, more likely, combination of factors, produced the differences? These findings raised questions about the quality of care and the need to look at this in greater detail. So I internalized this idea too. When I got back to Chapel Hill, I mentioned earlier that I wanted to put in PAS in the hospital, but the idea was rejected. We did modified quality studies in the outpatient department. When I went to Hopkins, I thought it would be nice to replicate some of Morris' ideas. Bob Brook was a student, so I approached Hopkins, and again we were rejected with the ploy: "You don't know where you are. What we do sets the gold standard for care everywhere!"

Berkowitz: This really is the gold standard.

White: So we went to Julie Krevans, with whom I worked quite closely. He was the head of medicine at Baltimore City Hospital, a Hopkins affiliate. He let us do the study there, and, sure enough, we found all kinds of problems: x-rays not looked at, appointments broken, lab results buried away. Bob Brook published the findings in the *New England Journal of Medicine*. He got his start investigating all this material; now "quality evaluation and assurance" are growth industries. But these ideas came from Jerry Morris and the English epidemiologists. I brought them over in 1960; they had stimulated my interest in getting these matters investigated. Eventually we got into the Hopkins hospital proper and found the same problems there that we had found at the Baltimore City Hospital. There were all kinds of misadventures uncovered there.

Berkowitz: Getting back to your going to Hopkins and leaving Vermont, was that just simply an offer you couldn't refuse from Johns Hopkins to start this new department there?

White: Yes. Apparently they'd been on the verge of approaching me before I went to Vermont, and I'd only been there, I guess, less than a year before they invited me down. I said I couldn't leave the University of Vermont right away. Yes, they offered me a substantial budget and prime real estate, originally in the hospital itself. I thought, "Well, you can probably say things from Hopkins that you can't say from Vermont, or you can say the same sort of things but your colleagues pay more attention when you come from Hopkins." It never had occurred to me before that I would ever end up at Hopkins.

Berkowitz: This was the School of Hygiene, of course. Were you worried about that?

White: Yes, it was the School of Hygiene, and, yes, I was worried about it. But the real estate was going to be in the medical school, and we were going to be able to work with medical students, so I thought

this was pretty close to what I would have preferred, and you can't have perfection. But, yes, I was worried. In fact I had brought John Last from Australia to join my department at Vermont, and I invited him to come down to Hopkins with me, but he wanted to stay in a medical school. Of course, he's now the editor of the widely acclaimed *Dictionary of Epidemiology* and of the best selling book on public health; it is the former Maxie Rosenau. He is now at the University of Ottawa, still in a medical school. Yes, it did concern me, and I have to tell you that, with notable exceptions, of which I would list people like Abe Lillienfeld, Alan Ross, and Russ Morgan at Hopkins, and in the University of North Carolina's School of Public Health people like John Cassel, Al Tyroler, and Bernie Greenberg, the general discourse and level of intellectual activity is considerably lower in schools of public health than medical schools, on average. There are, of course, some real jerks in medical schools, and there is much useless activity, obviously, but I think that, on balance, the medical schools are really a cut above schools of public health intellectually. But there are some notable exceptions, and I don't want to suggest otherwise. I have spent about half my career in each of the two cultures.

Berkowitz: Yes. It must have caused a lot of tension at Hopkins, though, because I'm sure the public health people felt a sense of separation and probably thought that was good, and you were a person coming in saying, "What really matters is medical practice, and we have to get in there."

White: You know Medicare and Medicaid were just coming in, and I think they hired me to give a course on how to fill out Medicare forms. Ernest Stebbins, the Hygiene dean, suggested to me that I should give some such offering, and I said, "That's not what I came here to do." (I think poor Stebbins is dead now.) I started writing articles and saying things that they thought were subversive to the interests of the public health fraternity. You see, there was the field of so-called Public Health Administration, and I was attached to that department initially; John Hume was the head. The public health administrators had fought the idea of getting into medical care at all; they were very concerned that the *AMA* would do terrible things to them if they started messing around with medical care. In fact, many of the state acts establishing health departments within state governments specifically enjoined them from getting mixed up in medical care. They also required the state's Commissioner of Health to have an MPH from a school of public health.

I mentioned John and Elizabeth Horder and their nest of boxes depicting the distribution of patients in their own practice, and that it had been the model for our own *Ecology of Medical Care* paper, but there was another physician, John Fry, a general practitioner in Beckingham, (near London) England (he died a few years ago), who was one of the originators of research in general practice. He published numerous papers based on research in his practice; he also published a number of books, including several that compared health care systems in different countries with special emphasis on primary care. He and a colleague at the University of Manchester, Tev Eimerl, had introduced a recording system for use in general practices. The simple record forms collected basic data about the patient and the reason for the contact or encounter with the general practitioner. Eimerl's system became famous as the E-Book, named after its inventor. Based on the use of this recording method in numerous practices, the United Kingdom's Registrar General of medical matters, W.P.D. Logan, had published a number of annual volumes giving the distributions of medical problems in general practice. These were the first accounts of what went on in the largest component of the health care enterprise--primary care. Before that it was a "black box". There were one or two studies of single practices that had been done in this country in the early 1950s, including one in North Carolina in 1953 by Dr. Leon Taubenhaus. Here we were, running medical schools, without any useful information about the distribution of health care problems in the populations served. When I got to Vermont, this was another idea that I gleaned while in England. It was feasible to study general practices and the content and distribution of medical care. We therefore introduced the E-Book in Vermont. John Last worked with me on this and we published an article in *Medical Care* on the content of general practice. The study was not completely population-based, but it was a start. I pursued the idea when I was involved with the *National Center for Health Statistics*. About

1970 I had a discussion, actually in the men's room of the Department of Health, Education and Welfare, as it was called then, with Ted Woolsey, who was the Deputy Director and later became Director of the *National Center*. I said, "You know you guys are publishing statistics about people who are dead, and you're doing a little bit on people who are hospitalized, but you're not doing anything on the initial problems that are brought to the sources of health care. We should be studying the problems of the living as well as collecting data on the dead. We might be able to help the living, but we can't do much about the others." Woolsey acknowledged that it was a good idea and promised to do something about it. To make a long story short, when he became Director of the *Center*, he commissioned us, about 1972, at Hopkins to develop the survey design for the *National Ambulatory Medical Care Survey* (NAMCS); our team designed that survey. It was the first time that the country's medical schools had ever had any information about the distribution of health problems in primary care. The *National Center for Health Statistics* had more requests for the NAMCS reports than for any of their other publications up to that time.

Berkowitz: Of course, but there is a tradition there, too: Edgar Sydenstricker's studies of morbidity in the 1930s, National Health Survey. They were public health types, those guys.

White: Yes. Sydenstricker's operation was the predecessor of the *National Center for Health Statistics*. They built on his work.

Berkowitz: Hagerstown, Maryland, was one place.

White: But the *National Ambulatory Medical Care Survey*, like the other national surveys conducted by the *National Center for Health Statistics*, was population-based, rather than studies-based on selected or limited populations, or specially-selected populations. We had quite a time getting everybody on board for NAMCS survey, especially the AMA and all the specialty societies. There were people who thought it was a communist plot and terrible things were going to happen. But eventually the NAMCS reports became best sellers and were in great demand by faculty members of the country's medical schools, and, for the first time, they learned about the content of care in both general and specialty practice. The medical school establishments that control the curriculum, however, haven't paid too much attention to NAMCS data until fairly recently.

Berkowitz: There had been surveys about morbidity, there were surveys about disability, but your survey was about what? What kind of questions did you ask?

White: First of all, we asked about the patient's initial complaint, condition, problem, or symptom.

Berkowitz: These were people who were already sick and who were going to the doctor?

White: Yes, in general, but some were "healthy" people going for a "check-up" or for information. The *National Center* already conducted the *National Health Interview Survey* that provided information on what lay people said about their own health. We wanted to find out what went on in doctors' offices, what kind of problems did their patients bring to them, what did they call them, how urgent were they, how serious were they, what diagnostic tests were undertaken, what treatments or prescriptions were ordered, what referrals were made, and how long did the encounter last.

Berkowitz: That's interesting. And you followed them all the way through to see what happened to them? Did you have other doctors look at cases to see if they were correctly diagnosed? Was that part of it?

White: No. But another thing I found out in England from the general practice studies conducted there was that 60% of the problems brought to the initial sources of medical care cannot be given *International Classification of Disease (ICD)* labels. They're simply problems, conditions, or complaints. Maybe the patient isn't sleeping well at night or "My boss is harassing me." There's no place for them in the ICD. More recently, I helped organize the *International Classification of Primary Care*, published by Oxford University Press. This classification is now being used widely in Europe and elsewhere but rather slowly in this country. There's a whole array of other health statistics with which I was involved as chair of the *National Committee on Vital and Health Statistics (NCVHS)*.

Berkowitz: Whose committee was this?

White: The *NCVHCS* was advisory to the Secretary of Health, Education, and Welfare and the *National Center for Health Statistics* on matters statistical. These national committees were originally set up by the *World Health Organization (WHO)*. *WHO* invited all of its participating countries to establish a national committee on health statistics. Initially they were to be concerned largely with classifying and recording causes of death and developing the *International Classification of Diseases*. At first it was a voluntary committee until I got Ted Kennedy to make it a statutory committee for the first time.

About the same period, it dawned on me that the *National Center for Health Statistics* had a whole series of unrelated national surveys and data collection systems such as the Health Interview Survey, the Health and Nutrition Survey, the Ambulatory Care Survey, the Hospital Discharge Survey, the Disability Survey and, of course, the Vital Statistics reports. Somehow they should all be integrated so that the public and the politicians could better understand the state of the nation's health and the patterns of disease. I was able to persuade Kennedy to put into the Public Health Authorization Bill a phrase something like: "There shall be an annual report to Congress on the health of the United States people." When the first issue of *Health: United States* was published, it hit the front pages of the *New York Times*, the *Washington Post* and the *Wall Street Journal*. I was in Dorothy Rice's office that morning; she had just taken over as Director of the *NCHS*. She received a call from the **White** House asking, "What the hell is this?" David Matthews was the Secretary of HEW, it was during Ford's presidency, 1974-'75. Of course Matthew's minions had signed off on our report, but they hadn't paid any attention to what they were signing, so Dorothy had to explain what it was all about. But it's been a highly popular publication ever since and usually gets front page coverage in most newspapers each year. We used to do a chart book that the politicians used. They could tell whether the slope of the curve was going up or going down or the bars were getting bigger or smaller. They couldn't read a statistical table, but they could look at a picture and get some meaning out of it. That too was a bestseller.

Berkowitz: That was your baby, yes?

White: Yes. I helped to put it together. Perhaps I should mention that I had a number of encounters with Kennedy over the years. As a member of the Advisory Committee for the Health Program of the *Office of Technology Assessment (OTA)*, I often had opportunities to talk to him; he attended our meetings regularly. On one occasion I mentioned to him that the budget for the *National Center for Health Statistics* was inadequate for the tasks required of it. He made a note on the back of a match-book cover. I thought that would be the end of the matter. The next day, however, I was at a meeting in New York and was called out for a phone call from one of Kennedy's aides. He wanted to know more precise details about the budget problem. I gave him these, and, to make a long story short, Kennedy succeeded in getting the *NCHS* budget increased adequately. His follow-through was quite remarkable. While I am on the subject of Kennedy, I recall that I was offered the job of Associate Director of the *Office of Technology Assessment* by the then Director, I forget his name. I had to decide between the job at the Rockefeller Foundation and the *OTA* position. I had declined the latter, but Kennedy phoned me personally and tried to persuade me to change my mind. I was sorely tempted but didn't think I would

be happy as a bureaucrat. I had had a couple of overtures about joining that fraternity in earlier years, but they never interested me. Unbeknownst to me, I was apparently runner-up as Surgeon-General in the Johnson era but was black-balled by the public health establishment for obvious reasons. They knew my views on public health training.

Berkowitz: I wanted to talk with you, since we're on this subject, about the government now. This *National Center for Health Services Research* was apparently your baby, too. Tell me about that.

White: When I took over as chair of the *Health Services Research Study Section*, it was partly a creature of NIH and partly of the so-called "down town" public health fraternity. It was really run along NIH lines. I'd been a member of the Study Section for several years before they made me chairman. Previously that entity had been called the *Hospital and Medical facilities Study Section*. It was the entity that distributed the Hill-Burton Act money for research--the first million dollars for health services research back about 1955. Most of the research being funded was quite limited and largely concerned with improving hospitals. Examples of the research they supported included proposals to cut down the noise from the garbage cans, how to beef up the central supply department, or how to improve the food. It was nuts and bolts stuff. The research had nothing to do with why you use a hospital or whether it does any good or not. Our W74 grant was a notable exception and suggested a new direction of investigation.

About 1959 there was a landmark joint meeting between another NIH entity the *Nursing Study Section* and the *Hospital and Medical Facilities Study Section*. We met in the basement of the Westwood Building, in Bethesda, on the NIH campus; it was one of the ancillary buildings they were using at the time, maybe they are still using it. Out of that meeting emerged the term *Health Services Research*. I don't recall doing it, but someone introduced the term Health Services Research. Previously we'd called this emerging field "Medical Care Research". That was when we'd been involved in Chapel Hill. Paul Densen, Cecil Sheps, George Reader and a few others - we were a small group who met periodically to discuss our research.

When I became chair of the study section in 1962, I decided we should try to put our new field on the academic map. We adopted the models that had been used to develop public and political interest in heretofore neglected areas such as environmental sciences and genetics. This meant building up an understanding of the field and making it visible to a wide variety of potential stakeholders. So we introduced programs such as Young Investigator Awards and funding for university-based Health Services Research Centers. We established a number of these around the country, and many still flourish. And we commissioned a set of scholarly papers by credible academics who reviewed the numerous disciplines that could contribute to the broad field of Health Services Research. Their papers were critiqued at two major conferences, and all were published by the Milbank Memorial Fund under the editorship of Donald Mainland, a distinguished medical statistician and a close friend of mine. The entire series set out for the first time the potential scope, for example, of economics, sociology, epidemiology, and management theory for investigating a wide variety of components of health services systems. The methods and concepts that were used in the disparate disciplines were discussed.

Berkowitz: This would have been when, in the early 1960s?

White: Yes. Probably 1963-64, somewhere in there. That was one approach to establishing the field. We did a number of other things also. I wanted to send a team to Europe to have a look at health services there and the potential for comparative health services studies. I remember talking to the people in the Public Health Service who said: "Oh, you can't do that. They've just got a lot of communist and socialist ideas there. It would just pollute the whole environment here. You can't send anybody over." I replied: "Why don't we go over and find out what they're doing. If they have made a lot of mistakes, we

might as well know about them so that we can avoid repeating them. If there are some good things they're doing, maybe some of them can be adapted for use in our country." Eventually I got permission. I assured them: "I'm not trying to get myself on a junket. I'm not going on this trip myself." We persuaded Bob Haggerty to head it up; he later succeeded me as chair of the Health Services Study Section.

Next I advanced the idea that the Study Section should visit a number of places that were influential in the provision of health services or that might be suitable venues for the conduct of research. Again, I was told initially that this was a bad idea; eventually the Public Health officials agreed. With the help of that bureaucratic wizard and consummate negotiator, Tom McCarthy, the Study Section's Executive Secretary, the entire Study Section visited and held extended seminars and conferences with, for example, the American Hospital Association, the California State Health Department, the Centers for Disease Control, and Puerto Rico's Health Department and the University of Puerto Rico where regionalization of health services was becoming a reality. Then I said, "We want to visit the AMA." Once more the public health types told us: "You can't talk to the AMA. They are the enemy!" This is truly unbelievable stuff now.

Berkowitz: Especially at that point, right?

White: Especially at that point. I said: "There are all these doctors, and they're doing all these things to the people out there; we need to know more about their activities." And they said, "You can't do it!" Tom McCarthy, whom I mentioned a moment ago--and, incidentally, you should talk to him since he knows where all the bodies are buried--said, "We'll just go and send the bill to the Public Health Service. What are they going to do, put us in jail?" So we went to Chicago and had an excellent meeting at AMA headquarters, and we sent them the bill to the Public Health Service, and nothing happened. We had very constructive talks with them. Charlie Edwards was the host; he later became Assistant Secretary for Health at HEW.

I forget whether it was that meeting or a subsequent one, in the bar of the Palmer House Hotel, when I said, "What we need, now that we've got all these things going on, is to create a *National Center for Health Services Research*." I sketched this out on a paper napkin on a table covered with a red and **White** checkerboard cloth; I remember it well. Tom McCarthy was there and Evelyn Flook, one of the bureaucrats dispensing the money after we'd made the decisions, and Gil Barnhart who had a similar role. We knew that the annual meeting of the *Association of American Medical Colleges (AAMC)* was in progress in Washington, D.C., so we decided we'd try to get an announcement circulated there. Gil Barnhart and Evelyn Flook rushed back to Washington and produced a brochure in about 36 hours that described what the proposed entity would do and justified the need for it. We distributed hundreds of copies at the meeting. I made a few comments, but the general reaction seemed to be that I was "just a trouble maker". Health Services Research was not regarded as of interest to the medical schools of the country. Their emphasis was on biomedical research.

We then lobbied Phil Lee who was Assistant Secretary for Health at HEW and George Silver his Deputy, and various other people. They appointed Paul Elwood to do a feasibility study; there were various iterations of this, and eventually authorization of the *National Center for Health Services Research* was enacted, and it was created.

Berkowitz: What year was that?

White: I think it was 1968 that it was created.

Berkowitz: What was your model? Was it inspired by the NIH model?

White: I think the NIH model, yes. We thought that there should be a visible and substantial entity charged with examining as many aspects as it could of the health services enterprise. I'm not sure that we spelled out all of them, but certainly quality, access, costs, efficacy, and efficiency were prominent. We believed that it should have a substantial in-house staff, an adequate budget, should award grants to individuals and institutions, stimulate and promote the field, and that it should disseminate the results. The new *Center* might start modestly, but we saw it expanding and playing an essential role in improving the nation's health care arrangements.

One thing that probably emerged at some point is that if we're going to have competition and variation in the styles, organizational formats, ownership, and staffing patterns of health care facilities and services, we need to be able to compare them. We need to know the results or outcomes of their care, what the costs are, what the levels of patient satisfaction are, and so forth. The only way to do this is with appropriate information, its wide dissemination, and public discussion.

You have to start somewhere. I remember saying that I thought, perhaps not at the outset, but somewhere along the line, that it's really going to startle the horses when the newspapers start publishing comparisons of the local hospitals and their mortality rates, morbidity rates, costs, and outcomes. Sure enough, that's what did happen eventually. So that's how it started.

Berkowitz: Do you remember who it was on Kennedy's staff?

White: No, I really don't at this time. I testified before his Congressional Committee, and I probably spoke to one of his staff afterwards. There were hearings held about it, and we got the legislation passed, as I mentioned earlier. There were later hearings also that Senator Bell of Maryland held. One of the issues was where the *National Center for Health Services Research* would be located administratively. Would it be part of NIH? Or should it be part of the so-called "downtown" part of HEW? Should it be attached to the Secretary's office?

Berkowitz: So, should it report to the Secretary or the Assistant Secretary? Would it be part of the Public Health Service? Was that the idea?

White: We thought the higher up in the bureaucracy it was, the better its chances of success. That gets us into a later report. The *President's Science Advisory Committee* appointed a sub-committee on Health Services Research and Development which I chaired. After a year's work, we issued our report in 1971. It didn't get very far because, towards the end of our assignment, we were summarily ushered out of the Old Executive Office Building in a great hurry. That was when Mr. Nixon was getting into trouble, and we were shuffled off into another side building, and the report, although it was printed and distributed modestly, never got the kind of discussion we had hoped for. But in that report we argued that the *National Center* should be centrally located and should report to the Secretary or at least the Assistant Secretary. I remember later on holding discussions with Julie Richmond when he was Assistant Secretary about its location and functions.

Berkowitz: That would be during the Carter administration? He was Assistant Secretary of HEW and Surgeon General. Where was it in the bureaucracy when Carter was president? Was it in the Public Health Service somewhere?

White: No, I don't think it was ever in the Public Health Service.

Berkowitz: It was just free-floating in the Department?

White: Yes. Of course, it fell on unhappy times because of what some regarded as Paul Sanazaro's misguided policies; he was the first Director of the *Center*. He had asked me to chair his advisory committee. It was called the *Scientific and Professional Advisory Board* for the *National Center*. We had some pretty fancy people, such as Rosemary Stevens, David Mechanic, and Bob Haggerty; Martin Feldstein was a member also. Earlier I tried to recruit him for my department at Hopkins. He did a number of excellent studies on health care when he was at Oxford. He published in the journal *Medical Care* that my friend Bram Marcus had started. That is where I first learned of Feldstein's work. Finally John Dunlop hired him at Harvard. Herb Klarman was with me when we interviewed him at Hopkins. We had a pretty good committee, but our advice wasn't taken. I recall Paul returning from a visit with Wilbur Cohen, who was Secretary of Health, Education, and Welfare. He reported that Cohen had told him that there were three urgent priorities for the *Center*: reducing costs, improving quality, and increasing access to medical care. I asked how long the *Center* had to accomplish all this, and Sanazaro said "six months". Our committee said this was impossible, and Cohen should have been disabused of the notion that these were feasible goals in the short term.

Unfortunately Paul wanted to expand the *Center* rapidly. One of his first moves was to have the Bureau of Chronic Disease transferred to the *Center*; it had a lot of second-rate grants that had been turned down by NIH from the categorical Institutes. They dealt with the management of chronic diseases and their services. The studies were not well designed and there was a raft of bureaucrats who weren't all that sharp, but the Bureau had a lot of money compared to the initial budget allocated for the *National Center for Health Services Research and Development*, as Sanazaro renamed it. He took all these people on board, and they brought with them a lot more trouble with the mediocre grants and even more with the unhappy bureaucrats, although he did get more money. He lost the focus of the whole *Center* and its mission, I think, as he expanded much too quickly and in the wrong way. Bob Brook could really tell you a whole lot about what went on because he was a Fellow at the *Center* during this period. He is now at RAND and UCLA. The other highly knowledgeable person would be Tom McCarthy; he was Deputy Director of the Center for awhile, and he has total recall of almost everything. He knows names, dates, people, and players, etc.. He is semi-retired now and lives in Potomac, Maryland. He'd be well worth talking to.

So the *National Center* fell on hard times during the early 1970s. It promised too much and wasn't able to deliver on it. Congress became increasingly impatient. My principle has always been to promise very little and deliver as much as you can sooner than your sponsors expect; you surprise people with something that's useful and helpful fairly early. So the *Center* got into conceptual problems and indulged in "scatteration": that is, it took on too many different projects with little focus and few priorities. All this led to credibility problems, and the budget was gradually decreased starting about the middle 1970s. It had pretty well evaporated by the early 1980s. Also, another problem was that there was unhealthy and unnecessary rivalry between the *National Center for Health Services Research* and the *National Center for Health Statistics*. You have to have data in order to do the analyses, and the question is who controls the data? Then there was a three-way split with HCFA.

Berkowitz: And before that SSA.

White: Ida Merriam had appointed Dorothy Rice to her staff and Dorothy had hired Cliff Gaus. I well remember Cliff Gaus at Hopkins in his carrel working on his thesis one evening about 6:00 PM. He was doing a study of hospital care in Allegheny County and Pittsburgh. For this he needed population-based hospital discharge data for the entire county from Blue Cross and Blue Shield, Medicare, and other insurance carriers. He was having only moderate success and great difficulty. I recall him saying something to the effect: "My gosh, if I ever get control of this, we're going to have a system for

documenting hospital care, and I'm going to see that it takes place." Well, I knew this was his goal, and when Dorothy took over the *National Center for Health Statistics*, I said, "You'd better get Cliff Gaus over here on your team, or he's going to have your lunch and get all the hospital discharge and perhaps other health care data over in HCFA, and they're going to be controlling it." She said, "Oh, no, Cliff's a nice guy, and I hired him." And I said, "Sure he's a nice guy, but this is what he's doing." So there was an on-going fight between the *National Center for Health Statistics* and their approach to vital statistics, that is, counting the dead and so on, which I said is fine, but let's do something about the living, and HCFA controlling more and more of the hospital costs and the hospital discharge data, although the *National Center for Health Statistics* introduced the Hospital Discharge Abstract Survey and the Uniform Minimum Data Set that was used to collect the data.

That is another interesting story. I organized an international meeting in 1967 at Airlie House to discuss a variety of approaches to collecting information on hospital discharges in a uniform format. This was not my idea: Florence Nightingale advanced it in the 1860s. We published the Conference's report in the journal *Medical Care* and in a separate volume. Included were specific recommendations for a Uniform Minimum Data Set for hospital discharge information. There were about 14 elements that we thought were important. As I mentioned earlier, Virgil Slee's Professional Activities System and one or two other systems that were much smaller had collected, as the *New Yorker* used to put it, "roomfuls of data untouched by human thought". They collected plenty of data but did not produce much usable information. So we independently promulgated the results of our deliberations, and I subsequently persuaded the officials (Ted Woolsey, I think) at the *National Center for Health Statistics* to set up an official committee to consider our initiatives.

It took 17 years for the Federal Government finally to adopt this simple list of 12 or 14 items on what happens to people when they're in the hospital, who they are, what their trouble was, what was done for them, what the costs were, and so on. The authority on this bizarre bureaucratic exercise is Jim Cooley at Georgia State University; he has followed all its history of twists and turns and jurisdictional infighting and has written various pieces about it. If you don't think information is power, you need to find out why the AMA and the American Hospital Association, assorted hospitals and their doctors, the health insurance companies, and several large Federal and state bureaucracies were all battling to gain control of the data. And it still goes on. They still haven't got the details ironed out twenty years after our Airlie House Conference and 130 years after Florence Nightingale proposed the idea. Following the Hospital Discharge Abstract Conference, we held an analogous one for Ambulatory Care Data and then one for Long-term Care Data; both these reports were published in *Medical Care* also, and in separate volumes.

Returning to the theme of the three-way split, there is little doubt that the constant bickering among HCFA, NCHS, and NCHSRD was of absolutely no help to the latter. It didn't have any control over the vast sums of money disbursed on Medicare and Medicaid and their accompanying data and didn't have control over the other statistical apparatus and the underlying national surveys all controlled by NCHS. All the *National Center for Health Services Research and Development* could do was to conduct or commission *ad hoc* studies and special surveys, and fund investigator initiated projects. We suggested in our President's Advisory Committee Report, *Improving Health Care through Research and Development*, that the statistical activities of these three bureaucracies be coordinated, if not consolidated, so that at least their individual missions meshed and weren't entirely discrete unrelated operations. Their data and information should be compatible and admit comparisons and analyses. To achieve that we needed identical terms, definitions, classifications, and standards for the recording and collection of data. The NCHSRD, without control of any data, went into a decline and literally dropped off the bureaucratic and academic map for several years until it was resurrected as the *Agency for Health Care Policy and Research*.

Berkowitz: When was that? Who was president then?

White: I can't be absolutely certain; I think Reagan. Cliff Gaus can tell you.

Berkowitz: He was the first head?

White: No, there was another fellow, Clifton Jarrett, who was the first head. He seemed largely ineffectual.

Berkowitz: So Clifton Jarrett, Cliff Gaus, and then John Eisenberg.

White: That's my recollection of it, yes. Jarrett was the first head.

Berkowitz: The *Agency for Health Care Policy and Research* is the successor to the *National Center*. And it's at the agency level like HCFA?

White: In the bureaucracy, I think so, yes. Incidentally, Cliff also started *the Association for Health Services Research* back in about 1981 or 1982.

Berkowitz: Really? Were you involved in that as well?

White: No, I wasn't. They've made me an Honorary Fellow, but, no.

Berkowitz: Also that journal started around then, too?

White: I did help to get it started, but that was some years before the *AHSR* was organized. The American Hospital Association wanted to start a journal to be called *Hospital Research*, and I was on the *Health Services Study Section*. The AHA sought a grant to finance the start-up of their new journal. I was also on the site-visit team to negotiate the grant with the AHA. I said, "That's a great idea to start this journal, but it's got to cover more than hospital research. It needs to be called something like *Health Services Research*." The AHA officials argued back and forth, and I patiently explained, "If you want the money, you'll need to change the focus." So they huddled in the back room and came back and said they thought they'd change the focus. So we gave them the grant and helped to get the new journal launched.

Berkowitz: When was that?

White: It was over 32 years ago, so that would make it 1964 or 1965.

Berkowitz: What's the constituency for all this different stuff? This *Center* and this *Agency*? Your department would have been one constituency, Department of Health Care Organization or Health Policy and Management? Were there other places that were doing this? Would you say this is your field, health services research?

White: I guess so. I don't like being categorized. I'm in the primary care/family medicine/general medicine camp also. I'm in the internal medicine camp. I'm in the health statistics camp. I'm in the epidemiology camp, especially clinical epidemiology camp. I'm interested in examining problems that bear on all aspects of patient care and using the best methods available with which to study them. Formally, I guess I'm an internist and epidemiologist involved in health services research.

Berkowitz: Where else beside Hopkins do they do this health services research?

White: Most medical schools and schools of public health now have units that do this work, although the titles vary. There is a large and growing membership in the *Association for Health Services Research*; it is now a couple of thousand individuals, I think. Its remarkable first Executive Director of it, Alice Hersh, died very suddenly last year. She was a most talented leader. Cliff Gaus got it started, but she really put it on the map. Are you familiar with their publications?

There is another global journal that I helped to get started, the *International Journal of Epidemiology*. I was on the Council of the *International Epidemiological Association* for about 15 years--longer than anyone else I believe--as its treasurer and later president. The *IEA* was started about 1954 with a small meeting in Nordwyck, the Netherlands; the Rockefeller Foundation funded the initial few meetings and also many later ones. John Pemberton, Professor of Social Medicine at Queens University, Belfast, and Mickey Willard, a most creative physician working at a rural hospital in Maine, founded what they initially called the International Corresponding Club. Its purpose was to promote exchange of ideas by young academics interested in broadening medicine's horizons; most of them had traveling fellowships from the Commonwealth Fund or the Rockefeller Foundation to visit the U.K. and Europe or North America. Pemberton and Willard initially published a mimeographed bulletin for several years. In 1971, I negotiated a contract with Oxford University Press to start formal publication of the *International Journal of Epidemiology*, and it has expanded and flourished ever since.

Berkowitz: So this is somewhat inchoate but still a big field. It's got economists, it's got epidemiologists...

White: ...it's got sociologists, statisticians, and anyone who is interested. The INCLEN network that I started when I was with the Rockefeller Foundation now have 55 Clinical Epidemiology Units in 25 countries.

Berkowitz: And that stands for what?

White: *International Clinical Epidemiology Network*. It's really a health services research enterprise. Each of the component Units from a medical school in the developing world consists of about four or five clinicians trained in epidemiology, about two health statisticians, a health economist, and a sociologist--all trained in epidemiological concepts and methods. They all earn Master of Science (M.Sc.) degrees at six universities in the developed world: McMaster University and the University of Toronto in Canada; the University of Pennsylvania and the University of North Carolina at Chapel Hill in this country; and the University of Newcastle in Australia. John Eisenberg was very influential in getting health economists added to each Unit; some were trained at the Wharton School at Penn initially. So we have this cadre of about ten young medical school faculty in each of these Units, usually based in a clinical department and sponsored by the school's dean or a clinical department chair. They are weaving together concerns and problems of equity, efficacy, effectiveness, and efficiency in health care around the world.

Their work, therefore, is really another form of health services research. It's the same basic undertaking designed to change the thinking about health policies and practices by medical educators, politicians, and the public. These Units provide the climate for asking such impertinent but important questions as: Does what we are doing for individuals or the community do more good than harm? Are we responding appropriately not only to individual needs but also to the population's needs? What do individuals and the public collectively expect and require? After all, it is the people who do the suffering and pay the bills. They're entitled to a pretty big say-so about what kind of services are going to be provided.

It's all part of a theme, not so much trying to establish a discipline, although some people are doing that. They want to set up certifying boards and rules for getting in and rules for keeping others out. We had big fights in the *International Epidemiological Association* over who's an epidemiologist. There were those who wanted everyone to go through an elaborate selection process in order to maintain an elite organization. Les Breslow and I thought that if the *IEA* could accommodate infectious disease types and the chronic disease types, it should be able to accommodate the health services research types. We went further and argued that anybody who was interested enough to pay the annual membership fee of \$10.00 and could write his or her name should be eligible to join. What's the point of having an elite organization? We don't need that kind of thing. Our views prevailed eventually. So now we have two or three thousand members from all over the globe.

Berkowitz: I see. Let's talk just for a minute about your career. In 1976 you left Hopkins. What was the reason for that?

White: To work on the *President's Science Advisory Committee* report, I had taken a year off from Hopkins's. I had been in the job chairing this new department for about seven years, so I decided to step down and return to civilian life.

Berkowitz: Who replaced you?

White: Phil Bonnett. He'd been a full professor in the department. Then I was getting a number of offers of deanships, but I never aspired to being a dean, especially of a school of public health. I had seven or so, one from a medical school and all the others from schools of public health. The University of California at Berkeley and the University of Pittsburgh were the two that interested me most. Also the *RAND* Corporation offered me the job as head of their health program. At any rate, I thought maybe I should be doing something else. The *United Hospital Fund* in New York approached me and offered me a job as Director of their new *Institute for Health Care Policy*. A fellow by the name of Joe Terenzio was running the United Hospital Fund. Paul Densen had recommended me for the job. Densen was a most talented health statistician and one of the pioneers in health services research. He worked for a long time with Sam Shapiro at the *Health Insurance Plan of New York (HIP)* and then went to Harvard to establish its *Health Services Research Center*.

Berkowitz: Another venue for pioneering health services research was HIP.

White: Oh, yes, and a very good one. Sam is an excellent example of how you become an expert. As far as I can tell there are three ways: Long Course, Short Course, and Self Proclamation. Most of the experts I know are now working in a field for which they were not trained! They are experts by the third route, self proclamation, although I am sure Sam never said that! Sam only had a bachelor's degree, but he is highly and widely regarded as a most innovative and skilled investigator. I recruited him to Hopkins as the director of our *Health Services Research Center* there. Anyway, Terenzio and his Board offered me this job as Director of their new *Institute for Health Care Policy*. I took it because it offered a measure of independence, a chance to start something new, a substantial budget and the capacity to make small grants for worthwhile projects. My wife and I liked the idea of living in New York because we had a daughter there. However, it wasn't long before I became disillusioned with the whole set-up.

Berkowitz: Did you have to live in New York?

White: Yes. We moved to Manhattan. I was a friend of Bob Ebert's, and he recommended me for a job at the Rockefeller Foundation, where he was a member of its Board.

Berkowitz: That's Bob Ebert from the Milbank Fund and formerly Dean of the Harvard Medical School?

White: Yes. As for Terenzio, I guess the best sort of phrase to describe his behavior was to call it what Lady Astor used to describe as "terminological inexactitude." We parted company. I didn't like his style. For example, I wanted to appoint a couple of people, and he didn't like the idea. His Board seemed only interested in protecting the reputations of the New York hospitals. They were opposed to asking questions about how they functioned. One of the studies that we were planning but never implemented was designed to determine how many lower gastrointestinal x-rays were "positive" and detected disease compared to the number that were "negative" or normal. I think I'd read somewhere that there was a question of whether they were a.) useful, and b.) how many of them were negative. So we were going to look at that question with several of the New York hospital's x-ray departments. I think some of the radiologists became frightened and got to certain United Fund Trustees, and they in turn seemed to generate a great deal of political interference. They wanted me to do things that would boost the local hospitals and support them. I was more inclined to roll over rocks and look at things. This wasn't the sort of climate in which I could flourish, and they, I'm sure, didn't want me doing that kind of research. The budget also waxed and waned until I figured it was time to go.

John Knowles, President of the Rockefeller Foundation, and I had had numerous discussions over the years about the rift between medicine and public health and what might be done to improve understanding and relationships. The two cultures were drifting farther and farther apart. In fact I had been interviewed by Knowles as a possible Director of Health Sciences, but I withdrew my name because it seemed to me they wanted someone with more tropical disease experience than I had. Ken Warren was appointed Director eventually. Then John Knowles appointed me as Deputy Director, and I had about half the budget of the Division of Health Sciences to get the *International Clinic Epidemiology Network* started. It was much more in keeping with what I wanted to do.

Berkowitz: And you did this project that you are describing. You stayed there until 1984 and then just decided to retire essentially?

White: Yes. Actually Rockefeller had a mandatory retirement of 65 in those days but I was kept on until 67.

Berkowitz: That's a considerable amount of time. That's 14 years. Have you been doing consulting?

White: Yes, I've done quite a few things. Most of the consultations involved weeks, months, or a year or two in one instance, and included multiple visits and much travel. I did a review for the Division of General Medicine for the University of Iowa, another review for the American Academy of Pediatrics of their research program, and a large study of the University of Saskatchewan's College of Medicine and its relationship to the province's population and to the health services there. I also did some statistical and health information consulting for UNICEF, UNDP, and the Pan-American Health Organization.

I've done two substantial stints in Australia. The first was for South Australia's Health Department. It involved an evaluation of its health promotion program--a pretty sorry affair. A second, more extensive review, was done for the Federal Minister of Health, Neal Blewett. I visited all the country's medical schools, several research institutes, and its one school of public health examining the teaching of public health. The net effect was my recommendation that the government close the School of Public Health, increase the budget for public health research, and redistribute the money to the nation's medical schools to enhance the teaching of public health. My suggestions were all accepted. There was a great outcry and much unhappiness on the part of the public health fraternity, but subsequent independent reviews of

my report and the political decisions made have shown that the new arrangements have increased the interest in public health and the numbers of people involved in it, and have improved training in public health. I also suggested establishing a *National Center for Epidemiology and Population Health* at the Australian National University in Canberra. It, too, has done remarkably well. In fact, I just received a notice that they're having their tenth anniversary celebration this fall. Bob Douglas, the Director, has done a fine job.

I've been to China as a consultant with a World Bank team reviewing their request for funds to develop aspects of their health services. Later I returned at the request of the Chinese to consult with the Ministry of Health about establishing something akin to the US *National Center for Health Statistics*. Both visits involved a considerable amount of travel in China. Two other visits there also enabled me to see a good deal of that immense country and to observe the extraordinary changes that are taking place.

I've written *Healing the Schism: Epidemiology, Medicine, and Public's Health*; that was done at the request of the Rockefeller Foundation. I edited another volume: *The Medical School's Mission and the Population's Health* and wrote a couple of chapters for it. That book was based on an international conference I organized for the *Royal Society of Medicine* and the *Josiah Macy Jr. Foundation*.

I've also written about 50 articles since I retired, and I'm still on three or four editorial boards. At Emory University, there is an *Institute for Health Services Research* named after me, and I'm a Director Emeritus. David Ballard is its President. His training in economics, internal medicine, and epidemiology is similar to mine, so we have much in common. The Institute is doing remarkably well.

Berkowitz: Why did you come to Charlottesville?

White: We had a place down here out in the country that we started building in 1978 . We used it for vacations, or, if I was away on a long trip, Isabel would come down here. Then we moved here full-time in 1984.

Berkowitz: Were you just attracted to this area?

White: I've got a brother who's on the faculty here, so that was one of the major attractions.

Berkowitz: What's his field?

White: He was for many years chairman of the University of Virginia's Department of Psychiatry, but his main preoccupation is the study of reincarnation. In fact, I think he just returned from a field-trip to India last night. He recently published a two-volume monograph in which he documents hundreds of cases of children who remember past lives. He has case reports in which there are photographs and medical reports of birthmarks and of the corresponding wounds that the children claim to have suffered in a previous life. He's been able to document independently medical records unbeknownst to the individuals of what happened to these individuals in their supposedly previous lives. There is a shorter version of his work, really a synopsis of all his findings, that was also published recently. He just found another case in Chicago a month or two ago; it is quite remarkable. I think he hopes to try publishing it in the *JAMA*, but the establishment journals are none too keen on his line of inquiry. It doesn't fit the prevailing paradigm. Most of his cases are in India, Burma, Sri Lanka, Turkey, and among Indians of the Northwest, British Columbia, Washington, and Alaska. That's his kind of work.

Berkowitz: That's interesting. Let me ask you one last question. You've done all this heterogeneous stuff, and you don't like to be pigeon-holed and don't really believe in specialization. What would you

say is your signature work? Would it be that ecology paper? What would be the answer if someone asked, "What is the legacy of Kerr **White** to the field of health services research?" What would be the answer?

White: I think helping clinicians and doctors generally to think more creatively about their mission and their activities and to adopt a broader paradigm that includes the needs of all the people and encompasses not only the biomedical, but also the clinical and the population perspectives. That paradigm would recognize that at least half of the health care enterprise is concerned with the caring as well as with the curing component. Such thinking becomes complicated, and it requires a change of attitude from the reductionist, molecular, single cause notions of disease which seem to dominate the prevailing paradigm, to use an overworked word, to a rather broader view of the whole enterprise. All right, you can do your reductionist research, and you can do your corneal and liver transplants, but at least understand that there's a lot more going on here, and there are a lot of other perspectives and many other needs. What we urgently require are balanced health care systems and balanced portfolios of research to really move ahead with all of the people's problems. I prefer these aspirations to the notions that mapping the genome and giving everyone a smart card that has their genetic copyright on it, or cleaning up the environment are enough to bring good health and improved life to everybody. That's complicated; I can't really sum it up in one word. People wonder how can you be engaged in this, or how can you be in that, but I see them all as parts of a whole. I see my small efforts as encouraging one facet of the whole and then encouraging another facet.

I think we need improved information about health, disease, and health services. And that includes information about the ubiquitous Placebo Effect--the most powerful therapeutic intervention doctors' possess--as well as information about pills, potions and procedures. We've heard of the wisdom we have lost in knowledge, and the knowledge we have lost in information, but worst of all is what has been lost in data. Bureaucrats and others keep talking about collecting more data. What we need is better information garnered from the data, and then we need to transform the information into intelligence and, above all, into wisdom to guide us. That seems in short supply in the medical enterprise. I realize that's all pretty complicated, but I'm not sure these things can be boiled down to a sound bite.

Berkowitz: It seems to me that in your case you have an intellectual legacy which is involved in changing the gestalt in some ways of the practice of medicine and of the clinical encounter. You also have your pedagogical legacy, these people that have been seeded by you into the field. And there's the organizational legacy too. The fact that the Federal bureaucracy now comprehends, in some vague way, this concept. That's a pretty major role.

White: I tried to pull them together. Most of these ideas are not that new. In *Healing the Schism* I tried to cover some of the historical background. We grew up politically informed. My father was a journalist; the Canadian correspondent for the *Times* of London, the *Economist*, and various American papers. In fact, we used to get the Baltimore *Sun* every day.

Berkowitz: Back when it was a really great paper.

White: Yes, it was. It was a terrific paper. And the Boston *Transcript*. So we grew up with many visitors coming through in a highly critical political environment. Half of them were British and half were American, so one got accustomed to translating social, political, and economic ideas back and forth. My father's best friend had established what was then the *Dominion Bureau of Statistics* and is now called *Statistics Canada*, so at home we used to get a flood of domestic and international statistical reports. I didn't pretend I could understand them as a youngster, but statistics were surrounding us all the time, so my interest in them was stimulated. Incidentally, I recently worked with Martin Wilk, a former Director of *Statistics Canada*. We prepared a fairly extensive piece on health information

systems describing what a truly comprehensive health information system should produce. It was never published. We developed it for the *Canadian Institute for Advanced Research* in Toronto when we were both members of its Advisory Committee for its Population Health Program.

Berkowitz: Is there anything else you'd like to mention, any topics we haven't covered? This was very good. It helps to give your essence.

White: Another interesting landmark was the 1970 meeting of the *WHO Expert Committee on Health Statistics*. John Brotherston, whom I mentioned earlier, was the Dean of the University of Edinburgh's Medical Faculty and later Scotland's Chief Medical Officer and Professor of Social Medicine. I think he was probably the best health officer that I ever encountered anywhere from any country. He chaired the WHO Expert Committee, and I was its rapporteur. In that meeting, we defined, as far as I know for the first time, the terms "efficacy", "effectiveness", and "efficiency" for evaluating health programs. The important distinctions among three terms came largely from two people. One was Sakari Haro of Finland, who probably knows more about health information than anybody I've ever encountered. The other one was a French health economist, George Roches, who unfortunately died some years ago; he wrote an excellent book in French on health economics. His intellectual descendant, Simone Sandier, runs CREDOC, which is a major international health statistics operation in Paris. She's a great friend of Uwe Reinhardt and is a member of the Institute of Medicine. Haro and Roches together came up with these distinctions, particularly between efficacy and effectiveness, which too often are confused. *Efficacy* refers to the evaluation of a medical intervention based on a tightly controlled, randomized, double-blind study. *Effectiveness* refers to the evaluation of the impact of a reputedly efficacious intervention in the real world with real doctors who may or may not prescribe the intervention properly for real patients with various manifestations of their disease, and who may or may not take the prescribed intervention properly. *Efficiency* refers to the prudent use of resources to optimally obtain desired outcomes. As far as I know, that was the first time that these important terms were defined. I think now we're getting the literature straightened out between *efficacy* and *effectiveness*, and I see them used much more accurately, especially by the Cochrane Collaboration. Again, these ideas emerged when a Frenchman and a Finn debated them, and the rest of us internalized them, and I wrote them up in the report, but I didn't generate the ideas. Ideas come from many different sources, and they're not the province of any one discipline or any one individual. One was a physician turned health statistician, and the other was a health economist.

From 1964 to 1976 our Hopkins department took the lead in a large WHO sponsored international study of health care use. It was called *the World Health Organization/ International Collaborative Study of Medical Care Utilization (WHO/ISMUCU)*. What we set out to do was to look at the distribution of the use of health services in defined populations. We studied seven areas in twelve countries: Argentina, Britain, Canada, Finland, Poland, Yugoslavia, and the United States. Our teams, all trained to the same standards, conducted household interviews with probability samples of about 1,000 households in each study area and almost 48,000 persons altogether; the overall response rate was 96%--quite unusual for such research, but we planned it very carefully. We found the patterns of use to be quite similar in the different study areas and countries. Barbara Starfield helped to write one of the more important papers documenting clearly that where there was adequate primary care--that is, where there are readily-available general physicians--hospital utilization was much less than when the converse was true, no matter what the country. Roemer's law states that "a made bed is a bed filled." We showed, I think conclusively, that no matter how many beds per thousand population, if there was adequate primary care, and people had access to a general physician, the use of hospital beds was much lower. All these were adjusted standardized population rates. This huge project was a great learning experience for all the participants; it went on for twelve years.

Berkowitz: Was that a Hopkins thing?

White: It started in Vermont. George Silver got me involved in it, but I was the chairman of the enterprise. In 1976 Oxford University Press published a huge volume of our results; it was titled *Health Care: An International Study*. We also put out about a hundred papers. A number of these were based on our findings, and analyses of the original data were conducted not only at Hopkins, but at other universities here and abroad. We made tapes of the records available to about a dozen universities. We had wonderful consultants, Ozzie Sagen, the brilliant Associate Director of the *National Center for Health Statistics*, and my best friend, and Charlie Cannel from the University of Michigan's *Social Survey Center*. We taught each other. I learned a great deal from the Finns, Poles, and Brits especially.

Berkowitz: That's another side of you, isn't it?

White: Yes, I've always been an internationalist. Another enterprise was a volume I did: *The Task of Medicine* published in 1988. Charles Odegaard, former President of the University of Washington, wrote a seminal book titled *Dear Doctor* in which he basically upbraided the medical profession for their failure to listen to their patients and to truly "care" for them. It was sponsored by the Kaiser Family Foundation and was widely distributed. He wrote about the need for better communication, better understanding between patients and doctors; some of the things we've been talking about. When Al Tarlov, President of the Kaiser Family Foundation, held a meeting at Wickenburg, Arizona, to discuss Odegaard's book, he asked me to be the rapporteur. I wrote a long piece reviewing and recounting the issues discussed, and, together with several appendices of the more interesting papers presented at the meeting, it was published as *The Task of Medicine: Dialogue at Wickenburg*. The volume was well reviewed, and there were thousands of requests for it. The head of the New York Health Department who died recently, David Axelrod, ordered copies for all the first year medical students in New York State. Kaiser distributed at least ten thousand copies. That's not a best seller, but for academic medical books it is more than satisfactory. Of course, they were distributed free, so that makes a substantial difference. According to its producer, with whom I met several times, *The Task of Medicine* was the inspiration for an eight-part documentary filmed by an international consortium of national TV networks including the BBC, PBS, Australian and Spanish TV and others. The series was called *Medicine at the Crossroads*, but it had a different title in Britain. I had a bit part in the sections dealing with Hopkins.

I was also Editor-in-Chief of a volume for the *Pan-American Health Organization (World Health Organization): Health Services Research: An Anthology*, for which we selected a hundred articles. We had collected nominations from a whole host of people around the world. Barbara Starfield was on the editorial committee that I chaired, and we whittled the original candidates down to a hundred papers after several iterations of the list among a group of experts. The volume seems to have been well received.

Another enterprise that I was involved in was *the Ambulatory Sentinel Practice Network (ASPN)*. I helped sponsor it while at Rockefeller, and it's now going great guns. The network consists of over 100 practitioners, primarily family practitioners, but some internists and some pediatricians, mostly in the United States but also some in Canada. They select a discrete medical problem such as headache, earache in children, spontaneous abortion, carpal tunnel syndrome or depression, for example, and each participating clinician records on a simple form essential data about the encounter. Collectively the network can examine the distribution of the problem and the diagnostic and treatment interventions undertaken. Again, this is another way of finding out what goes on in doctors' offices. To date, this method has been quite successful. It was modeled after similar networks in Britain and Holland. Many would think that these were American developments, but I thought these were good ideas originated abroad and we sponsored a couple of the initial ASPN meetings when I was at Rockefeller. There are many different approaches to developing a field.

Another volume we did when I was president of the *International Epidemiological Association* was

Epidemiology as a Fundamental Science: Uses in Planning, Administration, and Evaluation. Oxford University Press published that also. It must have come out about 1976. It sold very well and only went out of print about a year ago. In one chapter, I did some calculations estimating how many epidemiologists would be needed in the United States. A couple of years ago I saw a reference to my estimate, and it was seen as a substantial underestimate as of now, although some regarded my numbers as a gross overestimate at the time. Apparently our volume was used as a textbook in a number of medical schools. We didn't take any royalties because without royalties the book would sell for less. There are many different ways you can approach the dissemination of ideas. Most of them seem to work most of the time.

One of the things that concerns me greatly in this country, and this is true not only of the fields that interest me, is the lack of institutional memory. It's especially pervasive in the bureaucracy. People arrive and they think history began when they took office. There seems to be little awareness of what other people have contributed and what has come before. As a friend of mine says, "if you see a tortoise on a fence post, you know it didn't get there by itself!" There are very few good new ideas around--especially in health care. Most of the new ideas are not that good, and most of the good ideas are not that new! At McGill's medical school, we had a wonderful Professor of the History of Medicine, Andrew McPhail, and we also had a very good Professor of Physiology, Hebel Hoff, who went on to Baylor University in Texas. He used to dramatize many of the essential principles of physiology with demonstrations of the original experiments illustrating the fundamental discovery. Both these teachers helped a lot to encourage my respect for medical history and the past accomplishments of the pioneers. I guess growing up in a home that had enormous regard for history and for the breadth and range of human experiences made a difference also. All these exposures made lasting impressions on me. For them I am eternally grateful. We'll have to see what comes in the future.

Berkowitz: Yes. Very good. That's a good note on which to end.

White: Thank you.