HOSPITAL ADMINISTRATION ORAL HISTORY COLLECTION

Lewis E. Weeks Series

Kerr White

KERR L. WHITE

In First Person: An Oral History

Lewis E. Weeks
Editor

HOSPITAL ADMINISTRATION ORAL HISTORY COLLECTION
Lewis E. Weeks Series

Produced in cooperation with

Library of the American Hospital Association
Asa S. Bacon Memorial

Sponsored by
American Hospital Association
and
Hospital Research and Educational Trust
Chicago, Illinois

Lewis E. Weeks
2601 Hawthorn Road
Ann Arbor, Michigan 48104
(313) 662-4298

Kerr L. White

CHRONOLOGY

1917	Winnipeg, born there January 23
1940	McGill University, B.A. with honors
1940-1941	Yale University Graduate School, Strathcona Fellow
1941-1942	RCA Victor Company, Montreal, Personnel Assistant
1942-1945	Canadian Army
1949	McGill University, M.D., C.M.
1949-1950	Hitchcock Memorial Hospital, intern
1950-1952	Dartmouth Medical School, Hitchcock Memorial Hospital, VA Hospital, resident in internal medicine
1952-1953	Royal Victoria Hospital and McGill University, Montreal, Hosmer Fellow
1953-1957	University of North Carolina, School of Medicine, Assistant Professor of Medicine and Preventive Medicine
1957-1959	University of North Carolina, School of Medicine, Associate Professor of Medicine and Preventive Medicine
1957-1966	Health Services Research Study Section, National Institutes of Health, member and Chairman (1962-1966)
1959-1960	London School of Hygiene and Tropical Medicine and London Hospital, Commonwealth Advanced Fellow
1960-1062	University of North Carolina School of Medicine, Associate Professor of Preventive Medicine and Medicine
1962-1964	University of Vermont, Department of Epidemiology and Community Medicine, Chairman and Professor
1962-1970	American Public Health Association, Council of Medical Care Section, member; Program Committee, Chairman (1962-1964); Joint Commission on Medical Care Education, member (1967-1970)

1962-1967	National Conference on Public Health Training, Committee on Training Needs and Resources, Chairman
1963-1965	Association of American Medical Colleges, Planning Committee for Faculty Seminars and Research in Patient Care, Chairman
1963-1968	Association of Teachers of Preventive Medicine, Council member; Committee on Research Training, Chairman (1962-1965)
1964-1968	American Public Health Association, Governing Council, member
1964-1971	International Epidemiological Association, Treasurer Executive Committee member, Council member
1964-1976	World Health Organization/International Collaborative Study of Medical Care Utilization, Chairman
1964-1976	The Johns Hopkins University, Department of Health Care Organization, Professor, Chairman (1964-1972)
1965-1966	American Hospital Association, Hospital Research and Educational Trust, Advisory Council, member
1966-1980	National Center for Health Statistics, consultant, Standing Committee on Public Health Conference on Records and Statistics, member (1967-1970)
1966-1967	Regional Medical Programs Review Committee, National Institute of Health, member
1966-1970	American Sociological Association, Committee on International Research, member
1967-1969	New York Academy of Medicine, Committee on Social Policy for Health Care, member
1967-1971	National Advisory Health Services Council, member
1967-	World Health Organization, consultant, Expert Panel on Organization of Medical Care, member
1968-1973	National Center for Health Services Research and Development, Scientific and Professional Advisory Committee, Chairman
1968-1971	Maryland Hospital and Education Trust, Board of Directors, member

1969-1981	Foundation for Child Development, Board of Trustees member, Secretary (1979-1980)
1970-1972	American Medical Association, Advisory Committee on Undergraduate Medical Education, member
1970	World Health Organization, Fourteenth Expert Committee on Health Statistics, Rapporteur
1970-1972	American Public Health Association, Governing Council, member
1971-1981	International Epidemiological Association, Council member; Chairman (1974-1977); Past-President (1977-1981)
1971-1972	President's Science Advisory Committee, Panel on Health Services Research and Development, Chairman
1973-1976	National Academy of Sciences, Institute of Medicine, Council member; Membership Committee, Chairman (1974-1976)
1973-1977	University of Missouri-Kansas City, Chancellor's Board of Consultants, member
1973-1974	HMO International, Inc., Board of Directors, member
1973-1974	Maryland Health Data Committee, Inc., Board of Directors, member
1974-1979	Case Western Reserve University, Board of Trustees, member
1974-1978	Hospice, Inc., National Advisory Council, member
1974-1976	President's Biomedical Research Panel, Cluster on Epidemiology, Biostatistics and Bioengineering, member
1974-1976	National Research Council, Committee on the Study of National Needs for Biomedical and Behavioral Research Personnel, Panel of Health Services, member
1974-1978	Rhode Island Health Services Research, Inc., (SEARCH), Scientific and Professional Review Board, member
1974-1980	Harvard University, Overseers' Committee to visit the School of Public Health, member

1975	U.S. Congress, Office of Technology Assessment, Health Advisory Panel, member
1975-1980	National Committee on Vital and Health Statistics, Chairman
1977-1978	United Hospital Fund, Director, Institute of Health Care Studies
1977-1978	American Cancer Society, Blue Ribbon Commission on Smoking and Public Policy, member
1978-	The Rockefeller Foundation, Deputy Director, Health Sciences
1977-	Milbank Memorial Fund, Technical Board, member
1979-	Pan American Health Organization, Biblioteca Regional De Medicina, Sao Paulo, Scientific Advisory Committee, Chairman
1979-1981	Child Trends, Secretary-Treasury, Board of Directors, member

AFFILIATIONS AND MEMBERSHIPS

American Academy of Preventive Medicine, Fellow

American Arbitration Association

American Association for the Advancement of Science, Fellow

American Association of Health Data Systems

American College of Physicians, Fellow

American Foundation of Clinical Research

American Heart Association, Fellow

American Hospital Association

American Medical Association

American Public Health Association, Fellow

American Sociological Association

Association of American Medical Colleges

Association for Health Records

Association of Teachers of Preventive Medicine

Excerpta Medics, Editorial Board 1972-

Group Health Association of America, Inc.

Health Administration Press, Editorial Board, 1978-1979

Inquiry, Editorial Board, 1967-1979

Institute for European Health Services Research, Fellow

Institute of Medicine, National Academy of Sciences

Institute of Society, Ethics and the Life Sciences, Hasting Center

International Epidemiological Association

International Hospital Federation

International Journal of Epidemiology, Editorial Board, 1972-1981

International Journal of Health Services, Editorial Board, 1971-1979

Medical Care, Editorial Board, 1962-1973

New York Academy of Medicine, Fellow

North American Primary Care Research Group

Royal College of General Practitioners

Royal Society of Medicine, Fellow

Society for Epidemiological Research

Society for Health and Human Values

World Health Organization of National Academies, Academic Associations and Colleges of General Practice/Family Medicine

AWARDS AND HONORS

Alpha Omega Alpha

Argentine National Academy of Medicine, Honorary Foreign Member

University of Chicago
  Michael Davis Lecture, 1974

Harvard Medical School
  Eighth Annual Family Medicine Lecture, 1967

Ruhjsybuversuteut te Keudeb, Netherlands
  Elected to Boerhaave Chair in Social Medicine, 1967

University of Leuven, Belgium
  Doctor of Medicine (Honors Causa), 1978

Medical College of Wisconsin,
  Louis Quarles Commencement Address, 1976

McGill University
  Alan Oliver Gold Medal and Graduate Fellowship

University of North Carolina
  Distinguished Service Award, School of Medicine, 1976

Sigma Xi

BOOK PUBLICATIONS

Manual for Examination of Patients. Chicago: Year Book Publishers, Inc., 1960, Editorial Committee, Chairman

Medical Care Research. London and New York: Pergamon Press, 1965, Editor.

International Comparisons of Medical Care Utilization. Washington, D.C.: US Department of Health, Education, and Welfare, National Center for Health Statistics, Public Health Service Publications No. 1000, Series 2, No. 33, 1969, (with J. H. Murnaghan).

Hospital Discharge Data: Report of the Conference on Hospital Discharge Abstract Systems. Philadelphia: J. B. Lippincott Company, 1970 (with J. H. Murnaghan).

Statistical Indicators for the Planning and Evaluation of Public Health Programmes, Fourteenth Report of the WHO Expert Committee on Health Statistics, Technical Report Series No. 472. Geneva: World Health Organization, 1971, (Rapporteur).

Improving Health Care through Research and Development: Report of the Panel on Health Services Research and Development of the President's Science Advisory Committee. Washington, D.C.: US Government Printing Office, 1972, (Chairman).

National Ambulatory Medical Care Statistics: Background and Methodology, United States, 1967-72. Washington, D.C.: US Department of Health, Education and Welfare, National Center for Health Statistics, DHEW Publication No. (HRA) 74-1335, 1974, (with J. B. Tenney and J. W. Williamson).

Health Care: an International Study. London and New York: Oxford University Press, 1976, (with R. Kohn).

Epidemiology as a Fundamental Science: Uses in Planning, Administration and Evaluation. New York and London: Oxford University Press, 1976, (with M. Henderson).

Concepts and Information for National Planning and Management. Public Health Papers No. 67. Geneva: World Health Organization, 1977, (with D. O. Anderson, T. Purola, C. Vukmanovic, E. Kalimo, and B. M. Kleczkowski).

Epidemiology and Health Care Planning. London and New York: Oxford University Press, 1978, (with G. Knox, R. M. Acheson, D. O. Anderson and T. Bice).

The Health of Populations. New York: The Rockefeller Foundation, 1980, (with P. J. Bullock).

I suppose one should start with the early years. I was born in Canada. My father was a journalist for a number of British and American papers. I grew up in a family where there was a good deal of discussion about political, economic and international affairs. That determined my choice of undergraduate work at McGill where I majored in economics and political science. Through a friend of my father's who was the first head of Statistics in Canada, I was introduced to quantitative approaches in looking at public issues, in thinking about choices made by societies and at some of the policy issues of the day. I was strongly influenced by teachers at McGill, particularly by the head of the department of economics, Professor J. C. Hemmeon, who had a rather broad view of the role of economics and its potential influence on political choices. This was, of course, in the late 30s, before economics came to play a dominant role in areas such as international trade, commerce, banking, or even welfare problems and health care activities, as it does nowadays.

After majoring in economics I decided to undertake graduate work. There were various possibilities but I was intrigued with a course at Yale. I had some interest in transportation at the time and thought of going into that field. Yale had a master's degree in the subject; they changed the

requirements the year that I got there so they didn't give a degree and I just wrote a thesis, did the work for it and received a "Certificate."

I became greatly interested in a course given by a man, Elliott Dunlap Smith, who later became, I think, Provost of Carnegie-Mellon in Pittsburgh. He had worked with the Dennison Co., the people who used to make crepe paper and labels and now are a large stationery manufacturing company. Smith used to talk about industrial and labor relations and the influence of occupation and industrial conditions on health. I think that's where I developed some of my earliest interests in health and medicine. Smith introduced me to Chester Barnard's book The Functions of the Executive and to the managerial literature, to emerging work on personnel relations and the responsibilities of industry for the health and welfare of workers. He also introduced me to Roethlisberger and Dickson's book Management and the Worker, and to Elton Mayo's book The Human Problems of an Industrial Society, and his work on occupational fatigue at Harvard. A whole broad vista of new interests was open then. Reading Roethlisberger and Dickson I became aware of the experiments at the Hawthorne plant of the Western Electric Company and read these and related books by such earlier writers as Hugo Munsterberg and Vernon.

After finishing at Yale, instead of going to work in the transportation field, I got a job in the personnel department at the RCA Victor Company in Montreal. One of the first tasks they gave me was to pay off the piece workers who worked only part-time on Fridays; frequently this pattern occurred over the course of many weeks. They might work six hours; they might work thirty hours. They were on piece work. There were no unions and no guaranteed annual wage. I noticed that all other members of the personnel department would disappear on Friday afternoons and I would be the junior man

there and assigned this distressing job. These women, they were mostly women, occasionally men, were given perhaps $6.89 or $11.21 or something, and they'd simply break down in tears and cry in front of me because of the inadequate takehome pay they were getting. I also observed that it was the policy of the medical department to exclude people from jobs and employment who had any sort of disability which could possibly be a burden on the company. The net effect of these policies seemed to me, to say the least, to be socially unsatisfactory.

In my youth and exuberance, I regarded this as a poor way to run things. I became interested in a union the workers were forming and for a short time I helped to organize the union. I then decided to leave the company and for a few weeks spent much of my time with the union's affairs and was increasingly interested in their activities. During that period I visited the Montreal slums and saw how these people lived. It was a most moving experience.

I'd also worked in college at various summer jobs. One of them was an oiler on a freight ship going down to the Caribbean and Latin America. For the first time I really saw poverty in Puerto Rico's barrios and slums. I became quite concerned with interrelationships between poverty, employment and health.

Then the second world war was on and I decided to go into medicine largely because of those experiences. It seemed to me this profession, together with my interest in economics, provided an opportunity to combine public service and the two disciplines in an interesting career. I was accepted in medical school and realized I could get my education paid for if I served in the army so I postponed medicine and went into the army. I spent three years there, largely in personnel work; although I was not a physician, I worked in the

medical corps, part of the time in Canada but mostly in the United Kingdom.

I had to postpone medical education but became increasingly interested in the interactions between jobs and occupations, and health and disease. Finally in medical school at McGill I had developed a somewhat broader view than many classmates of the role of medicine in society and continued to read in related fields of economics, and industrial relations. I decided that I would complete postgraduate training in internal medicine because it seemed to me that the intellectual base for the entire medical enterprise rested in internal medicine; that was where the scientific strength and most of the political power was focused. It seemed to be the most stimulating specialty intellectually, at least to me, although it was somewhat narrow in its focus. There were some wonderful clinicians at McGill. They were very able teachers and extremely competent physicians in caring for patients. The head of the department of medicine with whom I became very friendly was J. S. L. Browne, a reknowed endocrinologist with a rather broad view of medicine and many interests outside of his profession. He befriended me and encouraged me in my efforts to extend my concerns beyond clinical medicine. I had two interests really. One was getting sound training in general clinical medicine. The other was in getting a broader view of the role of medicine in society; on that score I wasn't happy with what was being taught in McGill's public health department. It was headed by a former health officer from Ontario, and emphasized such matters as how to can tomatoes so that those who ate them wouldn't die of ptomaine poisoning, how to build an inexpensive privy, and a few elementary principles of immunization; but there was no historical or social scope to the teaching. Browne discouraged me from going into public health as a kind of intellectual wasteland, and I had to agree with him if

that was all those in the field could do.

At about that time I became interested in the writings of Professor John Ryle of Oxford. This was actually through a mutual friend of my father's who had learned of my interests. I had never heard of Ryle during my medical education. Ryle had been Regius Professor of Physic or Medicine at Cambridge and became interested in what was then called geographic pathology and in broader public policy issues of medicine. To the amazement and chagrin of his clinical colleagues, he then took on the task of starting the first Institute of Social Medicine, at Oxford. He was pretty well pilloried by his colleagues who took a dim view of the questions he was asking. He was not well accepted at Oxford and I guess one would have to say that he died of a broken heart. As a result I was unable to work with him. He was supported by The Rockefeller Foundation, my present employer, and came out to this country in the late 1950s. He gave a series of lectures, published in a little volume entitled Changing Disciplines that had quite an impact at the time. He spoke in numerous places around the country including the New York Academy of Medicine. He wrote several books that I read at the time. They were concerned with clinical medicine rather broadly in relationship to the populations served and the distribution of health services. So Ryle, with whom I corresponded but never met was another important influence on my thinking.

When I graduated from McGill I was advised to seek an internship either at the Royal Victoria Hospital or go to Boston or Hopkins and get the best possible training in internal medicine. I said I wanted thorough training but I wasn't sure I wanted to spend a year running around with a syringe drawing blood for the laboratory people to be working on. Moreover, I thought there

must be better ways of organizing health care than the way it was done around the Royal Victoria Hospital or the Montreal General Hospital in 1949. In our final year of medicine four of us got together and wrote a letter to the editor of McGill Medical Journal. One of the people who signed this letter was Doris Howell; she was formerly Professor of Pediatrics at Duke and is now Professor of Community Medicine at the University of California at San Diego. We were classmates. In this letter we said that clinical medicine was unduly fragmented; there were specialists for this and specialists for that but no generalists to act as a personal physician for the patient. I remember we used several graphic examples of patients who had been referred to multiple specialists in attempts to get their problems sorted out. Although generalists had not entirely disappeared in Montreal, they were waning rapidly and the only sure way to get health care was to see an array of these specialists. In our view we thought this was a poor way of trying to help patients.

At about the same time I wrote an article (this was while I was still in medical school) with the pretentious title of "The Science of Health." I re-read it several years ago when I was reflecting on a variety of developments in medicine that were going on currently. The article was written during the early days of the "comprehensive medicine" movement stimulated by the Commonwealth Fund, and the educational experiments at Case Western Reserve were just getting started. The Peckham experiments in London, which sponsored a health center that advocated health promotion and offered opportunities for investigators to study the interactions of families and neighbors on healthful practices and healthful ways of living, had attracted considerable attention. I wrote about these and related matters in this

article. I also summarized several long-term projects like the Grant Foundation's project on the influence of early life experiences of individuals on their later health and disease pattern.

By this time I had developed interests in the distribution of health problems in populations and in the role of medicine and the health services in coping with these problems. I had also become interested in the emotional aspects of health and disease because it seemed to me that doctors were not spending much time listening to their patients but rather were preoccupied with diagnosing, labeling, prescribing and doing assorted procedures. It wasn't clear that patients' problems were well understood. I did a fair bit of reading about the psychosocial aspects of medicine and in the field of psychosomatic medicine. I decided that there must be better ways of organizing health care than those practices in contemporary teaching hospitals. I looked around at various places and came across the "Three Sisters," as they were known then: the Mary Imogene Bassett Hospital in Cooperstown, New York, the Mary Fletcher Hospital in Burlington, Vermont and the Mary Hitchcock Hospital in Hanover, New Hampshire. I liked the idea of group practice but I didn't know much about these matters and there was nothing taught about them in medical school. I at least knew enough to think that the way medicine was headed in those days wasn't necessarily the way God meant it to run forever! I thought there might be some way of looking at the content of health services and their organization.

Well, I had opportunities for several "good" internships, having done well in my classes and so on. Against the advice of my mentors, I decided to go to the Hitchcock Memorial Hospital. I must say that the medical care provided at that institution and the clinical teaching were exemplary; I have never

encountered better medical care in any other setting. I was influenced there by Sven Gunderson, one of the distinguished Gunderson family that founded the Gunderson Clinic in Wisconsin. Another brother was professor of ophthalmology at Harvard, and a third was president of AMA. Sven was head of medicine at the Hitchcock Clinic and he took an interest in me. He was both a superb cardiologist and a first-rate general internist. I learned a great deal of clinical medicine from him. One of the things he used to do was to make us justify every test and x-ray we ordered and indicate how we thought the information would contribute to resolving the patient's problem. He was extremely cost conscious in those days. He emphasized that somebody had to pay for all the tests and procedures, and that moreover, many were painful to the patient, used up time, and kept them in the hospital, and away from their family, and that some of the tests are not without hazard. These were matters that were never discussed at McGill during my days there. I received, I think, some unusual clinical tutoring with continuity to it over three years of a type you don't always get in the midst of the more harried activities that dominate a university hospital.

Anyway, after three years at the Hitchcock Memorial Hospital in an internship and residency in internal medicine, I obtained a fellowship back at McGill. I spent a year partly in medicine and partly in psychiatry looking at some of the emotional aspects of health and disease, and broadening my understanding of these fields. Then John Browne had wanted me to stay on at McGill. He was going to put me up for a Markle Scholarship if I would stay but he was also a most generous and supportive person. He put me in touch with Lester Evans of the Commonwealth Fund whom I visited.

A new medical school was starting in Chapel Hill at that time under

Reese Berryhill; this was in 1952. I went down for a visit and found the new faculty full of high hopes and innovative plans for the future. They had had a symposium on the future of medical education and practice. One of the main speakers had been Professor J. N. Morris from London. I became interested in his epidemiological studies and his landmark book The Use of Epidemiology. I later spent a year with him in London. The aim of the school at Chapel Hill was, first of all, to meet the medical needs of the people in North Carolina and to train a combination of generalists and specialists who would try to embrace the emotional and social aspects of medical and health problems as well as the biological.

I was one of the new faculty gathered there from many other places. We had a remarkable leader as chairman of medicine, Chuck Burnett, who had come from the Massachusetts Memorial Hospital and Boston University. Skilled both as a clinician and teacher, he was also a very sensitive person. Unfortunately, Burnett died while relatively young. The second in charge of the department was Louis Welt who was at Chapel Hill many years prior to returning to Yale. I felt Lou Welt never saw things the same way that Chuck Burnett did but to his credit before he died several years ago Lou Welt went through quite a transformation. His interests broadened considerably. He grew and changed a lot over the years, but in the early days we had many struggles over values and priorities in medicine. Several of us were especially concerned with trying to get family medicine going and getting general internal medicine re-established. Although we were exercised about these problems the university itself had an explicit mandate to do something about them for the state legislature. We had a General Clinic in the outpatient department of the North Carolina Hospitals. I was in charge of the

teaching there for the fourth year students. We introduced a number of innovative teaching methods and emphasized continuity of patient care. I recall one arrangement in which we had the senior students act as the referring physician and ombudsman for the patients they cared for in the General Clinic. When patients had to be admitted to the inpatient wards of the hospital, the student from the General Clinic would accompany his or her patient to the ward and present the history to the attending physician the next day. The student would become an advocate for the patient, suggesting that tests not be repeated, x-rays not be presented, that the problem be accepted as it was presented initially by the patient and investigated further rather than starting from scratch. This was an intriguing sort of experience with many heated discussions between junior and senior students about the relative rights and needs of "their" patients.

Then I embarked on two types of research. There was great pressure, of course, to do laboratory research to which I had had some exposure at McGill. I was interested in cardiovascular disease. It seemed to me in talking to patients that if one just listened to them they would frequently tell you what their problems were. I was interested in cardiac failure, that is heart failure. We had a project that we ran for, I guess, five or six years. We were studying emotional factors in the precipitation of heart failure. We talked to a number of patients and then did studies of venous pressure and, while not dramatic or earth-shaking, at least we got them published in reputable journals and described in Time magazine and on T.V. The studies seemed to support the idea that the patient's loss of his physician, or a relative, a spouse or someone close to him or her was frequently associated with the onset of heart failure. This was not the only factor, obviously,

since the patients also had diseased hearts. But the presence of organic heart disease did not explain clinically why the patient went into heart failure on that particular day. Why not last week or next month? In fact, in clinical medicine there were several questions I always used to ask my patients: Why do you happen to come for care now? Why didn't you come some other time? What brought you here at this time? Then I would ask why they came to this particular place, why they came to see me particularly, why they didn't go to see somebody else? Then, thirdly, I would ask them to give me their own theory about the causes of their problem. For example, in the case of an accident, the patient might tell you they were preoccupied, or that they had been upset by a domestic quarrel, or they'd been drinking too much, or something of that kind. These kinds of questions, it seemed to me, could shed a good deal of light on many patient's problems, but they were not questions that were usually asked by the clinicians in those days.

At the time I was struck by the discrepancies between the levels of clinical practice at Chapel Hill and the patient care given at Hitchcock Clinic. As I mentioned earlier, I have not seen the equivalent of the care given at the Hitchcock Hospital in any university hospital. Now, there are obviously ups and downs and great variations, within and among institutions but, broadly speaking, the average patient would get extremely good care in a place like the Hitchcock Clinic. Whereas, down at the University of North Carolina I was amazed and appalled at the preoccupation with the laboratory tests and the x-rays and the kind of mechanistic view that dominated biomedical activities of the period.

At any rate, I had a unit in which several of us pursued studies of cardiac failure and the emotional factors impinging on this group of

patients. Increasingly, we began to ask, "What is the evidence that we are really doing much good in the outpatient department?" These patients were traveling long distances and we were supposed to be helping them. "Do we really provide much help, or don't we?" Even in the hospital I asked: "Do we have objective evidence that all this hospital work really makes a difference?"

About 1953, Vergil Slee was starting the Commission on Professional Hospital Activities (CPHA). I had read his original articles. So I suggested that we get Vergil down for a talk to the medical faculty and that we consider introducing his methods into the North Carolina Memorial Hospital. We might even get the Duke Endowment to support installation of the system throughout North Carolina, I thought. Well, he came down and gave a presentation. The whole idea was turned down completely by the medical staff. They said, "Look, don't you know where you are? This is a university hospital, and we know what we're doing. We're the last court of appeal. When we say it's so, why it is so!"

I replied, "Well, maybe it's so and maybe it isn't so, but it would be nicer to do it on paper and have the evidence. If it is true, it will be encouraging to know. If it is not true, we might like to take corrective measures of some kind."

These were the early days of quality assurance. Well, they voted us down saying they didn't want any of this kind of hocus pocus at the University of North Carolina. They were preoccupied increasingly with cellular and molecular events and with describing the processes of health and disease - extremely important topics, but very little concern was expressed for improving our understanding of how the patient came to be ill and how we could manage the problem better or how we could support the patient in coping with

his problem.

I was increasingly interested in these things and would teach them to medical students. I used to enjoy bedside clinical teaching on the wards and did quite a lot of it, as well as in the outpatient department. I said, "Why don't we have a look at what we are accomplishing in the outpatient department and see if we are doing what we think we are there?"

We set up a study that involved regular and systematic review of the outpatient charts. In spite of a rigorous teaching program, and a great deal of faculty involvement, we uncovered many deficiencies in our care which, in turn, we attempted to correct.

This work, incidently, was supported by the Commonwealth Fund; it was one of the innovations in medical education being funded at a variety of universities: Pennsylvania, Cornell, Case Western Reserve, and of course, North Carolina. These were all reviewed in a book by Peter Lee, Phil Lee's brother, some years ago; it was an account of these different experiments in medical education in the 1950s. The Commonwealth Fund was the link in the chain of events that took me from McGill to Chapel Hill and supported our early health services research.

In looking at medical care in our outpatient department more systematically, we found we were not doing nearly as well as we thought we were. We found many abnormalities that were overlooked, many laboratory findings that were never followed up, that were inaccurate or that needed repeating, and there were things not carried out that were supposed to be done. We published the results of this work; Bob Huntley was the senior author. He had been working with me; he is now at Georgetown where he heads the Department of Community Medicine there. He was also at the National Center

for Health Services Research, for a while. I guess that was really the first report on the quality of care in an outpatient department and one of the earliest that involved actual measurements of the quality of care anywhere. These studies were started in the late 1950s.

We also embarked at the same time on another study of the distribution and adequacy of medical care in North Carolina. It included enquiries about how patients first perceived their problems, and how the physicians who referred the patient perceived the problems. These general or family physicians were referred to disparagingly by faculty and students as the "local medical doctor," or LMD, and were seen as a somewhat inferior form of medical life by the people in the university. Then we examined how the patients' problem was perceived by the students, the house staff and the attending physician at the medical center's outpatient department. In completing this study we interviewed a sample of something over 100 physicians, which was virtually the total sample. There were a couple we didn't get for various reasons. But we saw virtually all the physicians and their referred patients; we also saw the patient's relatives. We published a series of papers on these studies. Among the interesting findings were the enormous discrepancies in the adequacy of communication between the patient and the referring physician, and between the referring physician and the University Hospital. In fact, the patient, referring physician, medical student, and attending physician, were frequently dealing with three or four different problems! The patient would think he or she was going for one problem, the local physician would be concerned with a second one, the student would get interested in a third problem, the attending would focus on a fourth problem, and none of them were communicating with each other at all. There were substantial discrepancies in what was said and what

was intended.

All of this supported my view that there was a lot more to be learned about the way the whole health services system was organized and operated. At the same time, we were having intense altercations between the hospital director and the medical school dean. I couldn't really find out what this fighting was all about, and who paid the interns and other costs. There seemed to be extensive misunderstandings about the responsibilities of the hospital administrator, the chairman of the different clinical departments, and the respective roles of the house staff, the students, and the patients. Then we had "private" clinics and what were known as "general" clinics. I was urging that they be merged; we had talks about group practice and about a variety of other forces and influences that were emerging in the medical care field. The dicussions about the problems and issues seemed to be pretty confused but above all they were uninformed by data, statistics or quantitative evidence. There were just opinions and most were strongly held and vigorously expressed.

About that time, John Grant, who had long been an officer with The Rockefeller Foundation, working at that time in Puerto Rico, came to Chapel Hill to visit Osler Peterson. Peterson had been doing his study of medical care in North Carolina which preceded our interests there. My understanding is that The Rockefeller Foundation was prepared to put up a building for a university-based group practice and that they would underwrite a prepaid medical practice which provided a general approach to the provision of health care in Chapel Hill. This would have been quite revolutionary from the organizational point of view and extremely interesting as a site for further study from an academic and research point of view, and certainly from a

teaching point of view. But the entire proposition was vetoed by the state medical profession, I think largely at the insistence of the physicians in the area. There weren't very many of them, but there was a great town and gown "shootout" and the thing never came to fruition. As you might imagine, both the initiative and its outcome interested me a great deal.

In getting our medical care studies started I needed help in their design and turned to the School of Public Health. I had become close friends with Bernie Greenberg who was then the head of biostatistics and now is the dean of the School of Public Health in Chapel Hill, and with the late John Cassel - quite a remarkable epidemiologist who had come to Chapel Hill about the same time I did. He came from South Africa, having been trained there by Dr. Sydney Kark who had developed an unusual group of physicians with a broad community approach. Numbered among them are: Mervin Susser, now at Columbia and the Abe Adlestein who is in the General Registrar's Office in London. Cassel was an epidemiologist with creative ideas about the possible uses of epidemiological concepts and methods; he was unfortunately never really accepted by the orthodox epidemiologists of the country. He used to teach in my courses and I taught in his and we conducted many seminars together. I could never quite understand the isolation of the School of Public Health from the Medical School and I thought it was most unfortunate that they were separate institutions. I made a pledge to myself at that time, and maybe to other people, that I never would join a school of public health because I thought the real business of bridging the gaps between individual medicine and population-based medicine had to be transacted within medical schools. If traditional attitudes and ideas were to be changed and responsibilities were to be broadened, that was probably the place to do it.

During the fifties, I became more widely acquainted with the literature of statistics, epidemiology and the organization of health services. There was very little research on the latter that had been done at the time. I recall that we started -- and I think Cecil Sheps probably took the initiative in this -- an Ambulatory Medical Care Club. At the time Cecil and George Reader, who is currently professor of public health at Cornell and was then in charge of the ambulatory clinic at Cornell-New York Hospital, and I guess Sydney Lee, Paul Densen and Len Rosenfeld were also in it. We met annually for several years. I remember presenting some of the data from our outpatient study on the quality of medical care. We showed we were not doing nearly as well as we thought we were supposed to be doing. We were assured by our colleagues in this "club" that such places as Cornell and the Beth Israel Hospital in Boston and other places had no such difficulties. Maybe we had troubles down in North Carolina, but they certainly did not have them in those places. I said, "Let's find out. If we can all do it on paper, it will be that much more reassuring to everybody." But they weren't too keen about this, so our study was, I guess, about the only one in existence for a while. Gradually the field developed further and interest in quality assurance grew.

At about the same time, George Reader had started a section at the annual Atlantic City meetings of the internists, which as you may know consist of the "Old Turks," the "Young Turks," "the baby Turks," and the "Foetal Turks!" These are the traditional hierarchical groups in the academic medical establishment. This new section focused on studies of medical education and patient care. It was an interesting effort at the time, but rather short-lived. The quality of the research was not high; it was mostly anecdotal and everybody was telling each other what went on in clinics and how

we were progressing in our educational reforms. The intellectual level was considerably below that of most other sections at the Atlantic City meetings.

I concluded at the time that it would be important to obtain further training in epidemiological methods and quantitative principles in order to conduct health services research in a more credible fashion. We had to get past the stage of anecdotal accounts. Although we had had some success in our early research, it seemed less than was possible and certainly less than was needed.

To get back a little bit, it may be worth recalling that our health services research in Chapel Hill was supported by one of the earliest grants from the first money that Jack Haldeman obtained under the Hill-Burton Act; Louis Block had worked with him on obtaining congressional appropriation of the funds. They got a million dollars in about 1954 or 1955. I remember reading about it in the Journal of the American Medical Association; immediately I said that was where we should get funds for our research. We got one of the early grants in 1955 I think it was. Tony Rourke was probably the chairman of the Study Section in those days, or perhaps it was Ned Rogers of Berkeley who had been professor of Public Health Administration.

But the first money attached to the Hill-Burton legislation was the beginning of what later became the National Center for Health Services Research and blossomed into the activities that for better or for worse, were subsumed under that general title. That's how the field got started, to the best of my knowledge.

The other grant we got, the one on cardiac failure, I remember, was turned down initially by the Heart Disease Study Section, but the National Heart Council became interested and particularly Tinsley Harrison, Professor of

Medicine at the University of Alabama and the author of one of the major textbooks in medicine, thought the idea had merit. He and a group of Council members visited us, and they approved the grant. It was a rather unusual idea for physicians that emotional factors should be tied in with heart failure, although the concept was familiar to the general public. One often heard people say, "Don't do that to me or I'll have heart failure!" The idea was not popular in orthodox medical circles but in recent years there has been a resurgence of investigative activity in this field.

These two interests of mine merged. I had read more of Jerry Morris's studies in Britian; John Ryle had died in the mid-fifties. I had thought of trying to spend the year with him but that was no longer possible. Morris was an established investigator who had written a stimulating book, The Uses of Epidemiology, and had done original studies on the role of exercise in coronary heart disease among London bus drivers and conductors. The studies showed that the drivers, who sat driving the bus all day in London traffic, had much higher rates of coronary heart disease (myocardial infarctions), than did the conductors who run up and down the stairs of the double-decker buses, involving much exercise and jostling with the passengers. Then Morris went on later to show that there was a good deal of self-selection in the physical builds of the individuals who took jobs as bus drivers; they were considerably larger around the waist at the time of initial employment than were the conductors, who tended to be slimmmer, and so perhaps this was the fact that determined the difference in heart disease. This whole sequence of epidemiological studies intrigued me greatly.

I applied to the Commonwealth Fund, who had always been very good to me over the years, and had taken an interest in my progress. They gave a

fellowship for a substantial year in 1959-60. I took my family to London and we had a wonderful year. I had been urged to take a Diploma in Public Health at the London School of Hygiene and Tropical Medicine. I replied: "I don't think the schools of public health have all that much to offer but I would like to learn much more about statistics and epidemiology."

The School ran a six month rather intensive course on statistics and epidemiology, which I think still goes on; it's probably the best of its kind available. I was fortunate to have Richard Doll, who was later knighted, and became the Regius Professor of Physic or Medicine at Oxford following in Ryle's footsteps but now twenty years later he was accorded considerably higher status. Also there was Bradford Hill (now Sir Austin Bradford Hill), who had written the first article on medical statistics in the Lancet; he really introduced the notion of randomized clinical trials into the medical arena. Doll and Hill were pioneers in the use of quantitative approaches in medicine; they discovered the link between cigarette smoking and cancer of the lung. For the first time, clinical trials enabled one to determine whether a particular form of intervention was useful or not useful, and to what extent the placebo effect and the Hawthorne effect influenced the outcome. For me, this provided new insights into the ways in which the efficacy of therapeutic endeavors and health services could be measured, and new insights into the power of the placebo and Hawthorne effects and their importance as healing agents in the physician's armamentarium.

I took their courses at the London School of Hygiene and Tropical Medicine and worked with Jerry Morris at the London Hospital; it was a most profitable year. I also had the opportunity during the preceding and the following summers to visit a number of departments of medicine, social medicine, and

epidemiology in Scandinavia, Western Europe and the United Kingdom, and met many fascinating people. It broadened my views considerably and supported my impressions that there were ways of going about improving our understanding of patients' problems on the one hand, and of evaluating the impact of our efforts in more objective fashion, on the other. I concluded that we really had not addressed these issues adequately in American medicine. Much of this thinking was especially stimulating to me in the sense that the possibilities for further research using epidemiological methods had not been applied in clinical settings.

I returned to Chapel Hill. Unfortunately, Chuck Burnett was seriously ill and never really functioned as a chairman from the time I got back in 1960. Lou Welt, the acting chairman was there, and I don't think he took kindly to many of my new approaches and attitudes.

I remember one day thinking to myself, "These fellows just don't understand the nature or distribution of the people's health problems! What they see here in this university hospital is just not the way it's seen outside." I was really quite angry. I determined to work the problem out with numbers, as Alice in Wonderland had urged!

I took the data from our study of North Carolina referral practices, and additional data from the American National Health Interview Survey, and the British Sickness Survey, as well as data from Morbidity Surveys conducted by British General Practitioners, and wrote a paper with Bernie Greenberg and Frank Williams, who is now at the University of Rochester and quite an authority on aging and the care of the aged. We called the paper, "The Ecology of Medical Care" and published it in the New England Journal of Medicine. It is illustrated with a series of squares which diminished in size

to represent the population at risk and the rates for those who had a complaint or a symptom which they could recall, the rates for those who had consulted a physician, those who had gone to a community hospital and those admitted to a university hospital. In one month only one out of a thousand people in the population went to a university hospital. Well, the response to that article was not entirely joyful in all quarters. The figures were disputed; it simply "wasn't so" was the usual response from the academic medical community. To this my reply was, "Well, if it's not so, somebody else should do similar studies and should at least attempt to replicate it; we should at least try to establish the state of affairs as accurately as possible." The message wasn't all that well received, but I think over the years it's been gradually accepted as being a fairly adequate depiction of the state of affairs. I've had occasion to re-examine the figures and publish them in an article in Scientific American in 1974. Others have replicated the same basic patterns and relationships in many other settings. I have also looked at the figures for other countries and they are essentially the same. Common problems are common and rare problems are rare, and the bulk of the early symptoms in the national history of most diseases are not really labelled carefully or understood by many physicians.

Another introduction I had in England was to Professor John Pemberton and others who were forming a new society composed of recipients of fellowships from the Commonwealth Fund and The Rockefeller Foundation, and who had moved back and forth between the two countries. It was called the International Corresponding Club initially, and later the International Epidemiological Association. Actually the Rockefeller Foundation's Jack Weir supported its development in the early days. I became interested in the organization since

it provided an international scientific network of individuals who were interested in stimulating the application of epidemiological concepts and methods. I was pleased to be invited to join. There was nothing particularly elaborate or fancy about the Association, it was just a group of individuals with mutual interests, so I joined in 1959, I guess it was.

Things weren't going too well with Lou Welt and the sort of interests that I had, and I didn't want to join a School of Public Health. I still thought that changes had to take place in medical education. The University of Vermont asked me to join their faculty. They wanted to revitalize a waning department of so-called preventive medicine. The Commonwealth Fund again had supported the department in its formative years. The Dean turned over a large part of the school budget to our department, and we got quite active in a number of different areas involving the epidemiology of common disorders, the recording of symptoms in general practice and in health services research. That was about 1962.

I think the first department of Community Medicine in the U.S. was Kurt Deuschle's at the University of Kentucky; and at about the same time Tony Payne had come to Yale from England to head up a new department created from a merger of Ira Hiscock's and C.-E.A. Winslow's department in the Yale School of Public Health with John Paul's Department of Preventive Medicine in the Medical School. Public Health and Preventive Medicine were merged and it was called the Department of Epidemiology and Public Health at Yale. We called our department at Vermont the Department of Epidemiology and Community Medicine, and I think it was the first one of its kind of that name; possibly the first department in a medical school anywhere to use "epidemiology" in its title.

Among the activities that we had in the department was a Family Practice Unit in which I was especially interested; one of the first to be established in this country. We had an interesting group of faculty members there. I was increasingly concerned with finding ways to get family medicine integrated into medical education. We had failed at Chapel Hill, although in recent years a Department of Family Medicine has been created. Although I had thought initially that internal medicine was the place to bring about change, it looked as if Vermont provided an opportunity to demonstrate the role of epidemiology more broadly in studying the natural history of disease by introducing it into family practice. The Family Practice Unit also provided a population for observing the interactions of emotional and social factors on health and disease.

At about that time I became increasingly interested in international health. We used to have students from other countries sent to us at Vermont from Harvard, and also a number of visitors from abroad. I had met George Silver and he had asked me casually one time whether I would be interested in undertaking a study of international comparisons of health care in two or three countries. He had just come back from Yugoslavia and England. I said I thought it would be interesting and worth doing but it would need to be done carefully and in a fairly systematic way, since the international health literature was full of anecdotal reports with little substance and few facts. The next thing I know I received a letter from George Silver saying he had spoken to two or three people in Britain and Yugoslavia, and it had been agreed that I would head an international study of health care; I wasn't aware I had agreed to this!

At any rate, I became more involved in this new enterprise, encouraged by

another new friend, Oswald Sagen. Ossie Sagen was one of the Associate Directors of the newly created National Center for Health Statistics. He was an excellent mathematician, had spent time at the Institute for Advanced Studies in Princeton and probably could have had a rather remarkable academic career, but he preferred to enter public service. He was a wonderful friend and I learned a great deal from him over the years. He became interested in the international study as did Cedo Vukmanovic, a wonderful Yugoslav, who had been at the University of Minnesota studying health statistics and health services. On his return to Yugoslavia, Vukmanovic, who has also remained a close friend, organized a meeting in Belgrade to launch the new comparative study of international health. This was done while I was still at Vermont. We started with a pilot study that included Vermont, the University of Manchester with Bob Logan (now Professor of Health Care Organization at the London School of Hygiene) and the town of Smederevo in Serbia with Cedo Vukmanovic. Many methodological problems were resolved in this feasibility study, although it was only completed after I went to Hopkins in 1964. I believe we showed that one could measure the use of health services in relation to measures of perceived needs and health care resources in comparable fashion internationally.

The other thread of my interests--cardiac failure--continued while I was at Vermont. We had done some epidemiological studies in North Carolina to see what the distribution of this common problem was. I had found that in the coding of death certificates, cardiac failure is not coded as a primary cause of death, as a rule, although as I suspected, and we established, it is a relatively common condition, especially in the elderly. It is the underlying anatomic disease process that is coded and counted and not the functional

breakdown of the circulatory system. From the viewpoint of treating patients, particularly when we have effective treatments, it seemed important that we know how many people suffered from this condition if services are to be organized sensibly. I said, "We don't really know how many of these potential patients there are around, whether it's a common problem or not a common problem. We don't know from the textbooks. You can't get any official statistics on the entity, since it is not coded; so we simply don't know."

It seemed to me that it was important to distinguish problems which affected large numbers of people, particularly problems that could be treated, from problems that affected just a few people, and problems for which there are no useful treatments yet available. While both types of problems concern the individual who suffers, if you wanted to have a larger impact, you'd probably try at least to deal with some of the commoner problems to see whether people were receiving adequate care.

For this study we coded the second causes of death on a large sample of death certificates in Vermont and found a great many more episodes of cardiac failure than anticipated. We compared these figures from surveys I had done in North Carolina and other studies. We prepared a report for the DeBakey National Commission on Heart Disease, Cancer and Stroke. These studies on the epidemiology of heart failure, later published in medical journals were the first, I think, that had been done on this relatively common problem.

My interests continued in the problems of family practice, the provision of personal health services and particularly in the adequacy of the help afforded for the emotional components of these problems. I had been asked to prepare a working paper for a WHO Expert Committee on General Practice -- this was in '62, I think. I had also been asked to go to a meeting in London on

the Future Role of Family Medicine. This was before the renaissance of general practice in Britain and Holland and later in the U.S., where at that time it was dying rapidly. I am not absolutely certain of the derivation of the term, but at any rate, I used the term "primary medical care" in my paper at that meeting. It was published in Britain and the term was adopted in the concluding statement of the meeting. Indeed the meeting argued for much greater emphasis on "primary medical care." I also used the term in my working paper for the WHO Expert Committee, and it was embodied in the WHO Report at that time. A slightly revised version of the WHO working paper was published in the Journal of Medical Education, about 1964.

I have been unable to trace any other source for the term "primary medical care." I have asked colleagues about the literature on it, but no one seems to know; it may well have been used by other people. I've had conversations with Richard Scott and also with John Fry about the term. In those days the term "general practice" was in great disfavor. Family medicine was just coming into vogue at the time, and I know, in conversations with Scott and Fry we talked of the need for some term implying basic or primary health services, in contrast to specialty or consultant services. At any rate, I think I was the first to popularize the term in a talk or in print in recent years. However, a similar term had been used in 1922 in the Dawson Report in Britain in which he referred to "Primary Health Centers"; I had not read the Dawson Report at that time but perhaps others had. Since the term "primary health centers" was used at that time, it may have been used in the intervening literature although I have been unable to find any references. Regardless of the origin, I used it in articles showing the decline of general practice in the United States. In addition to the article in the Journal of Medical

Education there were others in the Annals of Internal Medicine and one in the JAMA, based on a talk I gave at the annual February Conclave of the AMA on medical education in 1963.

In the early sixties, Walt Wiggins, who was then Executive Secretary of the AMA Council on Medical Education, had asked me to join the AMA and develop a commission on the future of family medicine. I discussed it with him at length and was intrigued at the idea but I wasn't keen on the idea of living in Chicago at the time. I wrote a three or four page letter in which I suggested that they set up an independent body analogous to a British Royal Commission. It seemed to me that the proposed format would be excessively dominated by the AMA and that other voices should be heard. If the Commission were set up independently, the report would be much more credible.

That commission eventually became the Millis Commission, and they looked at family and primary health care. I'm not sure where they got the term "primary health care" but the Commission report did a great deal to spark renewed interest in family medicine. The term was also used in a publication by the British Medical Association entitled "Primary Care." I asked both groups where they got the term from, and they couldn't provide any very clear account of its origins. I don't know whether all this is subliminal and everyone thought of the same term at the same time, or within a couple of years, but, at any rate, it seems to have arisen about the same time and probably was picked up by a variety of people.

That takes us up to about 1964. That was probably about the time that Health Services Research Study Section merged. I had joined the original Hospital Facilities Study Section about 1958, or '59, before I went to England. It had been set up by Louis Block and Jack Haldeman. In due course

we had a joint meeting -- about 1961 or 1962 -- of the Hospital and Medical Facilities Study Section, the Nursing Study Section and another one called The Human Ecology Study Section. The three study sections were reshuffled and the Health Services Research Study Section emerged from that meeting. I'm not sure who coined the term Health Services Research, but I think it was coined at that meeting held in the basement of an apartment building which NIH was using for meetings. The secretary of the study section was Glenn Lamson, who had worked with the Hospital and Medical Facilities Study Section when Cecil Sheps was chairman. The term Health Services Research caught on rapidly. It emerged in Britain later, and Gordon McLachlan used it in a number of reports from the Nuffield Provincial Hospitals Trust. I don't know who introduced the term, several of us were active in the discussion and debate. I said we needed a term that focused on research and looked at the services provided not just the buildings and equipment. The Hospital and Medical Facilities Study Section had been concerned largely with equipment, with diets and menus, and similar kinds of activities; it was regarded as too narrow in scope. We needed to know not just how to run a hospital, but why you run a hospital, and how it relates to other levels of service in the overall organization of health care.

About that time John Hopkins asked me if I would like to join their faculty. Apparently they had been contemplating looking into my qualifications before that, unbeknownst to me, when I decided to go to Vermont. I'd only been at Vermont about a year, when they offered me an appointment to start a new program concerned with the problems of medical care in the Hopkins School of Hygiene and Public Health. I waxed and waned over the decision for some time since I'd just gone to Vermont; in addition I had

some real reservations about joining a school of public health, particularly when I'd taken the "pledge" I mentioned earlier. On the other hand, I felt that one could do innovative things at Johns Hopkins because of its reputation, broad public and professional acceptance, and its position of alleged leadership, for better or for worse, that were not as readily accomplished at more modest universities. Hopkins would have the advantage of being a base from which one could work more readily. So I agreed to move there. They offered me attractive resources, but somewhat limited space initially, although the space was certainly adequate later on.

It was an interesting period and I enjoyed most of it. The school had a Department of Public Health Administration under John Hume, who later became the dean, and our new activity was a division of his department at first. The question of what we were to call this vague monster that was to be introduced into a traditional school of public health arose. I talked to Russell Nelson about it, too, and he expressed many reservations about the future of the School of Public Health and how it all related to the School of Medicine. I was familiar with the history of the place from the writings of Flexner, Welch and others but did'nt really know how the whole thing ran until I was there. I suggested calling our new entity a Department of Health Services Administration, or Health Care Organization or something of that kind, but Ernest Stebbins the dean then and Hume felt that was getting too close to Public Health Administration and might upset some people. I asked what Public Health Administration was all about but I could never get a clear definition. It seemed to consist of presiding over budgets of health departments and training health officers to deal with sanitation problems and the surveillance of communicable diseases. I asked, "What about hospitals? Are you training

hospital administrators?" Well, they once had a fellow, Ed Crosby there, and he had tried to do this but he moved on to become Executive Director of the American Hospital Association. They had Paul Lembcke at Hopkins for a time and he looked into the quality of medical care. For this effort, it seemed to me, he had been pretty well ousted from Hopkins. Looking under "medical mattresses" and generally enquiring about the kinds of medical care people are receiving and its adequacy was seen as threatening to the medical profession and hospitals. I said to myself, "Well, I fully intend to do these things. You'd better be braced for it, because it's what we're going to do." At any rate, the faculty seemed prepared to do something in the field of medical care, although they were not sure what it should be. In the mid-sixties we were beginning to see signs of the burgeoning impact of Medicare and Medicaid, and the need to understand the underlying structure and processes in the provision of health services.

So I went to Hopkins in 1964 and we opened up shop. We called it the Department of Medical Care and Hospitals, because these were sort of buzz words at the time. That seemed the least offensive to others and seemed to encompass most of the areas that we had in mind studying. The deans didn't like the other titles I proposed for the department. They thought such terms as Health Services Administration or Health Care Organization would upset too many people in too many quarters, that the alumni wouldn't understand, and other people would be misled in their understanding or what public health is all about.

I decided in initiating this new department that we would emphasize research and introduce quantitative methods insofar as possible. Research is not an end in itself, but it is a means of evaluating the whole machinery of

administration. If you want to organize health services sensibly and relate the resources to the "market," or the distribution of medical problems in the population, you need to use epidemiological and survey methods. Essentially, I saw an analogy with the commercial and industrial points of view which I had learned from Elliott Dunlop Smith at Yale many years ago, from reading Chester Barnard (later president of The Rockefeller Foundation), Elton Mayo, and the literature on industrial management, as well as the epidemiological perspectives learned from Jerry Morris.

It seemed to me that so-called public health administrators and health officers didn't know what their market was; they didn't know who they were giving services to, they didn't know whether the quality of the care was up to what they said it was, and there were precious few numbers to go with all this. It seemed to me, also, that if we were going to be credible in the field of health services organization we were going to have to follow the pattern of the biomedical sciences and would have to become more "scientific"--in quotation marks. In other words we would have to apply quantitative methods, the laws of logic and the rules of evidence to our work. We developed a research program as our first priority, and we focused on the postgraduate education of a mixture of physicians, graduates from disciplines such as economics and sociology, and others with a master's degree in hospital administration. We had quite an exciting time for a dozen years or so, and we trained a number of people of whom we are very proud. They include Tom Bice who is a Professor of Health Services at the University of Washington, Bob Blendon, Vice President of the Johnson Foundation, Cliff Gaus, who was head of the Health Care Financing Administration's Health Service Research Division, Peter Levin, Director of the Stanford University Hospital,

Al Mushlin of Rochester, Tom Ivan of Seattle, Bob Huntley, who had been with me at North Carolina and is now Professor of Community Medicine at Georgetown, and Chuck Buck, Secretary of Health for Maryland. Barbara Starfield is another of our faculty; she took my job after I left Hopkins. Abroad we have Aviva Ron in Israel, Felix Gutzwiller in Switzerland, Elizabeth Schach in West Germany, Esko Kalimo in Finland, Jose Paganini of Buenos Aires and now with WHO, and Oscar Echeverri of Cali, Columbia and now with the World Bank -- they all have jobs of considerable importance. These are just a few who come to mind; they are a wonderful group of friends and colleagues.

So we used health services research as our basic theme. One of our major projects was the large international study that we did in seven countries with twelve study areas. We completed the feasibility study in Vermont that I referred to earlier and then started the larger study when I got to Hopkins. That took a lot of time and effort but focused the interests of several members of the department. It was a remarkable experience for all of us who participated, and certainly not least for me, in that we had a rather eclectic group of investigators from different countries and also from different disciplines. We had epidemiologists, statisticians, social scientists, operations research people, systems analysts and of course physicians, and often several of each discipline from different countries. The contributions of the participants from other countries were extraordinary. I learned a great deal; it was really a unique traveling seminar on health services research in seven different countries. It was reputedly a worthwhile exercise for all of the people associated with the study. They included: Bob Logan who is now Professor of Health Care Organization at the London School of Hygiene; Esko Kalimo who is now head of the research group in the National

Pensions Institute in Finland; Cedo Vukmanovic who was head of the Federal Institute of Public Health in Belgrade and now head of a major unit of WHO in Geneva; Jose Paganini who is consultant to WHO; Abe Sonis who was Dean of the School of Public Health in Buenos Aires and is now Director of the WHO Regional Medical Library in Latin America in Sao Paulo, and Janus Indulski who became Vice-Rector and Head of the Institute of Social and Occupational Medicine in Lodz, Poland. Donald Anderson was very active in British Columbia in health care and epidemiology, but he became disillusioned, I guess, with the world at large and entered the Baptist ministry and is now pastor of a church. He has given up his work in epidemiology completely. Anne Crichton of British Columbia is still active and contributed greatly to the study. There were many others. They were an unusual group of people. We published, I guess, about 60 papers, one large book, nine manuals and several smaller volumes. I think it had some impact and perhaps encouraged the notion that generic principles could guide the organization of health services and that all countries have much to learn from each other and from objective comparisons.

It is interesting that using an epidemiological perspective in looking at health care problems requires one to focus on perceived health problems or morbidity of the people. You start by asking them what they are suffering from, how much they are worried or concerned about it, and how much it pains or hurts them. One then looks at the extent to which those with perceived health problems use services of different kinds, the costs of those services, and the balance among resources of different types, such as generalist and specialist physicians, nurses and hospital beds. Our whole approach seems to have had much more impact in Europe and to some extent in Latin America, and

even in Canada, than it did in the U.S. It hasn't been very helpful in this country, I think but the concepts and even the data have been used quite widely in public policy statements in other countries. I am afraid we still lack a sense of perspective about these matters in the U.S. and there is a naivete about the organization of social services and especially health services that seems to foreclose critical debate and impede innovation.

We also had a number of other people in the Department. They included Vicente Navarro, who was one of my first students in the department, he is now a full professor at Hopkins. He has a Marxist view of the scheme of things, but I helped him start the International Journal of Health Services. It is an enormously popular journal, widely read in many circles. I certainly defend his right to publish anything he wants in the journal, but I think it's unfortunate that it has become rather one-sided in its interpretation of history and its view of the problems of the world, because I think it had more influence when it was eclectic and had a broader range of points of view. But I defended his right to do this and he's carried on. Certainly he raises some extremely important questions about the factors and forces that impinge on health, disease and health services.

During this period of the sixties, a number of other things were going on. I was involved in the American Public Health Association on its Council, and in the Medical Care Section as head of the program committee for a number of years. We tried to jack up the standards for the acceptance of papers and for the running of the meetings. They used to let people talk about anything they wanted to for just about as long as they wanted. The intellectual standards were absurdly low. The programs ran over the alloted time, and they'd go on and on. We tried to introduce some rigor in it, and I think with

some success. You can get the details of the Medical Care Section's early history from people like Milton Roemer, Milton Terris and Rufus Rorem. People like Rufus, Nate Sinai and George Bugbee were on the Federal Hospital Council and on the Health Service Research Study Section. They taught me a great deal.

Increasingly, the American Public Health Association seemed to get activism and science mixed up; I became discouraged with this confusion of ends and means. I believe in activism; indeed, as I mentioned earlier, I helped to organize a trade union once. Nothing was going on at the APHA; they spent their time passing resolutions to which no one paid any attention. My observation is that the effectiveness of an organization is inversely proportional to the number of resolutions it passes. At any rate, if one is going to be an activist, it is a good idea to have some evidence to support the choice of direction in which it is proposed to act before applying one's emotions and energy to try to effect change. I thought much of the decision making in health care organization could be illuminated by research findings. It seemed that we didn't know much about the organization of health care or about how one could best articulate the various components. As far as I could see, in the United States we were headed on a collision course between the medical profession's perceptions, including the public health establishment and the public's needs. Services were likely to become increasingly inappropriate and costs to escalate out of control. At that time Ray Brown was a friend; he used to be on the Health Services Study Section and later we were on the National Health Services Study Section and later we were on the National Health Service Council together. We used to discuss these matters at length.

I gradully became increasingly disenchanted with "public health" and its

activities. I was on the Governing Council of the APHA and various committees, but I didn't think it warranted much more energy expenditures from me. It seemed to be having larger and larger meetings, and they were passing more and more resolutions, in violation of the rule I mentioned earlier!

Also, I was on the Council of the Association of Teachers of Preventive Medicine. I was asked to be secretary and probably could have gone up through the "chairs." I decided not to become more involved because the organization didn't seem to have much traction on the scheme of things, either educationally or organizationally. They were pretty well peripheral to what seemed to me to be some of the central problems and I couldn't help them get back to the central core of the need to change medical schools' priorities and the attitudes of the teachers. I believed there was a need to embrace the broader issues of medicine that related individual needs to population needs, and that saw epidemiological, behavioral, and health services research as essential components of medical knowledge.

In 1963 or '64 the National Public Health Training Conference met. I was on the organization committee and Bill Kissick was the executive secretary; he is now professor of community medicine at Penn. We conspired to select the committee members in order to get some of the training funds out of public health schools and into the medical schools. I was in a medical school at the time and not entirely disinterested. I thought the federal government should spread the available funds around a bit more but we didn't elicit much interest from medical schools. I think we got the first funds ever available for training in epidemiology and health services research into medical schools. That conference was a bit of a landmark. I still think it is essential to stimulate this kind of research in schools of medicine; indeed

that is one of my current interests in The Rockefeller Foundation.

About '64, I think, I was asked to be chairman of the Health Services Research Study Section, succeeding Cecil Sheps. I decided to take on the responsibility because it seemed to me that health services research was an important area that merited development. That became a major focus of my interests for several years.

We had to appoint a new Executive Secretary for the Study Section, and they asked me to help select an individual. I met with several candidates, and opted for Thomas McCarthy, who had a Ph.D. from Hartman's group at the University of Iowa. He had done a study of Blue Cross, and had taken an NIH trainee program for executive secretaries at Bethesda. He wasn't the most articulate individual of the group interviewed, but he was extraordinarily bright and most perceptive about people and institutions and above all he was very enthusiastic, whereas the other candidates were rather more passive and it was just a job to them.

So we had my first meeting as chairman of the Study Section. I started off by saying: "Now, I'm not going to be chairman of this outfit just to preside over the distribution of grants. We have to see what we can do to stimulate this field, and have to develop a plan and program of some kind to see it we can really move the field along."

I got Tom McCarthy to agree to work with me, and we had the unanimous support of the Study Section. There was one traditional health officer -- I won't mention his name -- who was the only member who expressed reservations. He didn't want to rock the boat too much or go too fast, he said. But we agreed to undertake three or four different enterprises.

The first was to commission a series of papers on health services

research. We were determined to get the best authors we could. Each was to prepare a "state of the art" paper in his or her particular field. Second, we decided to send two groups to look at health services and health services research in the United Kingdom and Scandinavia. We got the Milbank Memorial Fund to agree to publish these review papers and reports. Donald Mainland was to be the editor of the papers, published in two volumes. Mainland had been in the University of Manitoba at Winnipeg, which happened to be the place where I was born. Indeed, this recalls an intriguing coincidence. At one time the Health Services Research Study Section had five members from Winnipeg. Cecil Sheps and I were both born in Winnipeg; Ozzie Simmons, who is now at the Ford Foundation, had been born or possibly lived there; Nate Maccoby I think had been born there, and Donald Mainland, while not born there had worked in Winnipeg for quite a while, and all five of us were on the Study Section at the same time, for some odd reason. Mainland was a physician and anatomist who became interested in biological variation and turned himself into a statistician. He was one of the pioneers in medical statistics. He was working at New York University at the time he was on the Study Section, a kind of voice crying in the wilderness, rather excluded from the general scheme of things; much like Bradford Hill had been for a time. Mainland did some of the early classical studies on selective bias as a source of error in hospital-based studies. He examined the kinds of people who go to hospitals and the kinds of people who get autopsied compared to representative samples drawn from the general population. He wrote at length about clinical trials and observer error and related forms of bias. He had worked with clinicians a great deal on studies of the relative benefits and efficacy of various forms of treatment. In addition, he had become interested in health services

research: Does all this activity and expenditures of money really make a difference? What benefits can be attributed to specific treatments the doctor prescribes and how much benefit is associated with his caring about the patient? Again the theme of the powerful influence of the placebo and Hawthorne affects emerged.

Mainland and I made site visits together and I learned a great deal from him. He was a prolific writer; he wrote many interesting articles, including his famous mimeographed statistical notes for clinicians; these must be collectors items now. He is now retired in Connecticut.

Mainland agreed to edit the Milbank papers. We had two substantial conferences to critique and review the papers prior to revision for publication. The volumes took the state of the art up to about 1965 and staked out an array of problems that needed to be the object of investigation in the future.

Regarding the two groups to visit the United Kingdom and Scandinavia, we had unbelievable difficulties in getting their trips approved by what were known than as "the downtown people in HEW"; NIH included the "uptown" people! I must say the whole view of public health as seen through the eyes of some of these people was pretty discouraging. Jack Haldeman and Louis Block were a breed apart from all of this, but the other senior administrators seemed to have an unusually myopic view of the needs of the times. The view held by these senior administrators of the HEW towards sending groups over at public expense to look at health service in the United Kingdom and Scandinavia was that it was pretty close to being an enormous waste of public funds, and seemed to border on treason! I argued that we all know, or should realize, that health services in the U.S. would inevitably change. The country should

have an opportunity to learn about experiences with different patterns of health services in Europe. Even if these countries had made nothing but mistakes we could find out what the mistakes were. We might even be unable to avoid repeating those mistakes, and need not go down the same road. If there was anything worthwhile to be learned, we should at least consider adopting it or adapting it, while if there weren't, we could reject it promptly. At least we could have an objective look at some of these things. The HEW officials argued that there was "nothing to learn from these 'socialists'" and there might even be "communist" enterprises over there! It was un-American to think we couldn't invent things and develop our own solutions for our health services problems. So I said well, that was certainly part of the American heritage but it was my reading of history that it's a good idea to be aware of the experiences of others lest we repeat the errors of the past! So at any rate they finally agreed to the trips to Europe and we got the papers published.

The second item on the Study Section agenda was to organize a series of visits to a number of places and institutions involved in the provision of health services to see what was actually going on. We started with the American Hospital Association and then visited the California Health Department at Berkeley where Lester Breslow was then a senior official; it was generally regarded as the best health department in the country. Next, I thought we should go to Puerto Rico and see what they were doing with regionalization under the leadership of Guillermo Arbona and John Grant whose ideas were being incorporated in the plans for the new medical complex there. We also visited the Communicable Disease Center (CDC) at Atlanta and the National Center for Health Statistics in Washington; these were important

units of HEW.

We made all these visits and learned a great deal. People told us about their problems, how their activities were perceived and what was going on. I think these visits did a great deal to inform the members of the Study Section since this was said to be the first time a Study Section of this type had gone out to see things in the field and inform themselves directly. The final trip we planned was to the American Medical Association in Chicago. It was vetoed by the authorities in the higher echelons of HEW because they said that we were engaged in "public health" and that the AMA and "public health" were not supposed to get mixed up with one another!

This reminds me of an episode when I was at Vermont. I had indicated that we would develop an interest in the community's health problems and in the distribution and adequacy of their health services. I received a letter from a professor of pathology, who was an active delegate to the AMA, saying that it looked to him as if I might be transgressing the "code of ethics" of the AMA in some of my proposed activities and that I should be a little bit more careful in what I undertook. I replied that as far as I knew the "code of ethics" of the AMA had no jurisdiction over me or any department of the university. It wasn't clear to me what "ethics" had to do with these particular questions. So far as I was concerned, I didn't plan to pay any attention to it. If the AMA had any other views, they could express them to me directly.

I was a member of the AMA (and I continue to be a member of the AMA), and had served on AMA committees on the grounds that it is probably better to try to influence them from inside, although I must say I have been pretty discouraged about some of the attitudes of the AMA and their progress over the

years in many sectors. However, they had some excellent people and I have enjoyed many associations with some of their officials.

But at any rate, this trip to visit the AMA headquarters by the study section was turned down. So finally McCarthy and I said, "Well, let's just go there and we'll send the bills in to be paid! We just can't be stopped by this kind of thing." So we arranged it with Charlie Edwards, who was then head of the Socio-economic Division of the AMA and later, of course, Assistant Secretary for Health. Charlie arranged it all; we had an excellent meeting; everything went very well indeed. We met with many of the top brass and had a good discussion about health services research. The AMA subsequently set up a Health Services Research group of its own.

I was a member of the Board of the AHA's Hospital Research and Educational Trust at that time. That was when we got the journal, Health Services Research, started. We had a long discussion at the time of the Study Section's site visit about whether they would agree to that title for the new journal, because they wanted to call it 'Hospital Research.' We argued, "Why don't you broaden the base to 'Health Services Research,' because if you say 'Hospital Research,' it will just be an organ concerned largely with the internal management of the hospital, and while articles on that subject wouldn't necessarily be precluded, you may want to learn to relate the hospital's managerial problems to other aspects of health services." I suppose in a way the AHA was pressured into this broader perspective because it was the view of the site visitors from the Study Section that the funds should not be given unless they did embrace this larger view of health services; that's how Health Services Research got started.

While we're on journals, the journal Medical Care had been started in

England. I had been a member of the original Editorial Board, and knew Bram Marcus who had started the journal. It fell on difficult times in Britain. A question arose about whether it could be brought over to the United States or not; J. B. Lippincott, the publisher was interested in such a move. I was deeply involved at that time with Al Yerby of Harvard in negotiations with Lippincott. We succeeded in bringing it to the U.S., and Lippincott took it over; Yerby, Don Reidel and I negotiated this. They wanted me to be editor of it at the time, but it was just after I had moved to Hopkins, in about 1965, and I was busy with the new department and decided not to but agreed to be on the Editorial Board. We got Don Reidel to be the editor. I was involved with bringing Medical Care to the U.S. and getting it set up, at the same time I was trying to help Health Service Research get started. I also was on the Editorial Board of Inquiry for a number of years.

As long as we are on journals, I also got the International Journal of Health Services started with Vicente Navarro. Then the International Journal of Epidemiology started. I am still on the Editorial Board of that journal and still on the Council of the International Epidemiological Association. So I've been involved one way or another in five journals in the medical care and health services field. It's interesting to observe how the journals in this difficult area are succeeding in stimulating the application of scientific methods to complex problems bearing on the provision of health services.

Over the years, I became gradually less enamoured with public health, preventive medicine, and their participants. They seemed to be more and more removed intellectually, organizationally, and politically from the mainstream of medicine. What interested me particularly at Johns Hopkins was the

initiative shown in starting the new health care plan in Columbia, Maryland, the model town being created from scratch between Washington and Baltimore; it was being developed by the Rouse Company. In the early days we were asked to do some of the planning for health care in Columbia and several of us worked on it, but there were major conflicts between the School of Public Health, the School of Medicine and the hospital. We didn't seem to be making much progress either conceptually or operationally. Joe Sadusk was brought in; he had been working at the FDA at the time and, after he left Hopkins, went on to Parke Davis. There was a famous meeting held at Hopkins at which the faculty was to discuss the Columbia medical plan. They were not given to having frequent faculty meetings at Johns Hopkins during that period. Those in charge tried to give the illusion at this meeting that Hopkins is run like the PTA, while in actual practice it is run more like General Motors; democracy was not one of the methods used for decision making in those days. This was in the middle sixties when students and younger faculty were starting to challenge the status quo more openly. There were clearly troubles emerging in academia.

At any rate, at this famous meeting the dean and hospital president presented the Hopkins plan for Columbia. There were about four different groups who spoke against it. There was one group who said that this was just a vulgar association with a real estate developer in the hills of Maryland, and that we should not lend the good name of Johns Hopkins to a common commercial venture of this kind and should have no part of it. A second group said this is a kind of "socialist/communist" plot to start a prepaid group practice for providing medical care and implied that God did not mean us

to practice medicine in that way! At Johns Hopkins University we should have no part of such a plot. A third group said, "Look, the Johns Hopkins Hospital is situated in a slum, where probably the world's worst medical care is provided to our neighbors. We know more about rat control in Calcutta than we do about it in East Baltimore, and we should get things in order around here and give adequate care to our neighbors before we embark on a project in the Maryland country-side!" A fourth group said, "Look, if there's any spare energy around here, we should get the laboratory and the X-ray departments working efficiently so we can get reports back promptly and do the good things we do for patients better at the Johns Hopkins Hospital. We should have no part of this outlandish enterprise. Let's get our own house in order first."

When these four groups were combined, you had pretty well 100% of the faculty against the Columbia health care plan, and it collapsed. So Tommy Turner, the dean, and Russell Nelson, the hospital director, asked me to chair a committee to look at the Columbia enterprise and see if it really had any enduring academic or research basis to justify the university becoming involved. It didn't seem to be very attractive as a service activity alone, largely, I think, because that was the way it was presented. Essentially, the Columbia plan would have established a prepaid group practice in affiliation with the Johns Hopkins Hospital. This wasn't any great news to the people at Kaiser-Permanente; it simply showed that Hopkins could follow suit and do the same sort of thing. There wasn't anything innovative about it, if the main objective was restricted to the provision of good medical care by a prepaid group working in a new town.

So I chaired a very interesting committee. Among the members of that committee was Dick Ross, who was head of cardiology at the time. He is now

the Dean and Vice President for Medical Affairs at Hopkins. He seemed then to have a rather traditional view of biomedical science and cardiology, and was somewhat skeptical of the Columbia project. But he was also a scientist, and we examined evidence bearing on the kinds of studies that could be done at Columbia, including epidemiological perspectives that could be introduced and the health services research that could be undertaken. The research and educational possibilities at Columbia were debated by the group, who except for myself, were all from the medical school. We produced a final report emphasizing the potential for research and education at Columbia. It was discussed at a famous conference at Hopkins' Evergreen House where the assembled faculty reviewed our document. This second meeting followed about a year after the first unsuccessful one; it was to decide whether to go head with the plan or not.

We recommended essentially three things; First, that there be a center in Columbia City for the provision of health care to the entire population on a prepaid basis by a group practice affiliated with the medical school. Second, there should also be a similar center in East Baltimore close to the Johns Hopkins Hospital. Third, there also should be a student and faculty health service developed to serve both the Homewood and East Baltimore campuses. The students' medical care was poor and casual and the same was true for the faculty, for the most part; it was a source of widespread complaint that such a renowned medical institution should be part of a university where the medical care for faculty and students was thought to be so inadequate.

After much debate, it was also decided that these three new services should be associated with the newly established Health Services Research Center at Hopkins. Eventually the whole plan was adopted and implemented.

The health services research was carrid out with our Department of Health Care Organization for a number of years. The group practice, and especially its associated hospital at Columbia were not sized correctly in relation to the populations to be served. Either the hospital was too large or it was too small, but it was certainly the wrong size. If they planned to replicate what was done at Hopkins, the proposed facilities would not be able to do it properly; the hospital would be too small in that sense. If it was going to be simply a primary care place for people to lie down for a few hours or a day or two when needed, then it was probably too large. It was too small to be a replica of Hopkins and too large to be a primary care service center. If on the other hand, it was to be a community hospital, it was not the right size for that either; it was not large enough to serve the whole community of Columbia. So the building's size was inappropriate.

Anyhow, they went ahead and built the hospital in a rather fancy architectural style. There ensued an enormous row between the community and the Columbia plan authorities. It eventually became a community hospital. The group practice has now broken away from Hopkins. However, over six or eight years there was considerable health services research done at Columbia. Cliff Gaus, Dan Barr, Tom Bice and others did research over the years as did a number of our other graduates who are now scattered in different places around the country. I think it had some impact on our department's thinking about primary care, but basically the work there did not have much impact on the medical school, I would say. The East Baltimore Center arrangements worked out reasonably well in spite of stormy times, about who was going to be the director. There were many confrontations between the community leaders and Hopkins officials and both learned a good deal. On the other hand, The Johns

Hopkins Health Services Research Center has done quite well under Sam Shapiro; it has developed a national and even an international reputation.

That takes me to the third item on the agenda for the Health Services Research Study Section that I mentioned. The two conferences with the Milbank Memorial Fund papers and then the visits to a number of operating organizations being the first two. The third item covered the kind of financial support we would provide. We instituted a program of Research Career Awards for younger investigators. We initiated the idea of Health Services Research Centers with Bob Haggerty as chairman of the committee that developed the format. The Centers were to be analogous to the Clinical Research Units that had been so successful for biomedical research. We started a program to support these new Centers. We also had Program-Project grants. Walt McNerney had one of the first of these at Michigan, and there were a number of others in different universities. George Bugbee and Odin Anderson eventually, had one at the University of Chicago. Then Hopkins got one, although I had refused to endorse the first project put forward by the Medical School. It seemed to me they were just wanting to get more money to do more of the same. I said, "No, it's got to be something different." So eventually we got the Hopkins' Health Services Research Center funded and started.

Let's see what other things went on at Hopkins during that time. We got the Clinical Scholars program started then. Julie Krevans, who has just been made chairman of the Association of American Medical Colleges, and who is the Dean of the Medical School at the University of California at San Francisco, and I developed it together when he was at Hopkins. The Clinical Scholars' program originally was designed to train physicians both in clinical medicine,

initially internal medicine, and in health services research. The scholars completed a degree with us, usually a Master of Science and occasionally a Doctor of Science. We had to break many of the rules of both the Medical School and the School of Public Health to accomplish this! But one of the great things about Hopkins is that it is a remarkably free and open place; there is extraordinary academic freedom; that is one of the traditions that makes it great. They have only one rule that cannot be broken, and that is that "there are no rules that cannot be broken!" So you can always bend things, and eventually those in charge become quite flexible about them. This flexibility is essential when starting new programs, especially in an otherwise rather tradition-bound and conservative setting.

The Clinical Scholars' program worked well. I think we trained some interesting people who have gone on to do worthwhile work; most of them are important contributors to clinical medicine and health services research. But all this was done when the program was supported by the Carnegie Foundation at the Commonwealth Fund. When the program moved to the Johnson Foundation they went "public" with it in a much larger way and broadened its scope enormously. There was not the essential infrastructure to train the scholars properly in the principles and methods of epidemiological and health services research. The program has fallen on unhappy days, I think, and it does not have much of an impact at present. These are recent developments that have affected the program adversely, in my opinion. However, the Clinical Scholars' program did start at Hopkins and spread to Case Western, McGill, Duke, and Stanford initially. By going "public" in a much larger way it has, I think, enabled a number of physicians to dabble in economics, the social sciences, biostatistics and epidemiology but no real competence in these

fields has been achieved by most of the recent Scholars, insofar as I can tell. However, since a number have sought additional training in epidemiology particularly, probably progress was made.

In the mid-sixties the Health Services Research Study Section identified the additional need for some kind of National Center or Institute for the conduct of Health Services Research, Community Studies, or Community Health Services -- the names vary. I remember a meeting in the Palmer House Hotel in Chicago with Tom McCarthy and Gil Barnhardt, who was in HEW at the time, and Evelyn Flook, who is quite an interesting person, too, if you come across her. We were discussing what was needed to provide a funding arm for what was then the Division of Community Health Services in HEW. We cooked up an idea--I remember sketching it out on a table cloth, a checkered table cloth in the Palmer House--for a National Institute for Health Services Research, or something of this kind.

A few weeks later there was to be a National Conference on Community Health Services. I remember that we prepared rather hurriedly a statement about the need to support this kind of research. Nathan Stark was one of the leaders at that meeting. He was later appointed Under Secretary for Health and Welfare in HEW. We got him interested in our proposal and distributed a brochure setting forth our ideas at this meeting held at the Shoreham Hotel in Washington, DC. The idea met with general approval at the meeting. It was agreed that something should be done about research in community health services.

On behalf of the Health Services Research Study Section, of which I was chairman, we prepared a letter to the Surgeon General urging the establishment of a National Center for Health Services Research. I have all the documents

for this somewhere. We urged that a high priority be given to the matter. The Surgeon General at that time, I think, was Bill Stewart, and Phil Lee, I guess, was working at HEW then also. They discussed the proposal over some months and eventually legislation was introduced -- this would be about 1965 or '66. I remember testifying on the bill; I think it was before Congressman Rogers' Sub-Committee or perhaps one of the other House Committees.

Eventually, the National Center for Health Services Research was created by law. A long discussion then ensued about the Center's location with HEW. George Silver, Phil Lee's deputy, and he consulted with a variety of people and approached a number about what should be done. The question was whether the center should be attached to the Assistant Secretary's office or whether it should be at some lower level in the bureaucracy. There was concern about whether it should be highly visible or not too visible, and there was even discussion, I think, about whether it should be attached to NIH.

I wasn't privy, of course, to all of the discussions and internal arguments. But at one meeting with Phil Lee and George Silver, that a number of us attended, including Paul Densen, George Reader, myself and I don't remember who else, we discussed just where the Center might be located. In essence our advice was that it should be placed rather high up in the HEW hierarchy. Of course, I'm sure that all special pleaders and others with special interests say that their pet organization should be placed high up in the hierarchy and that's the place it deserves in the scheme of things! They eventually placed it rather low in the hierarchy, under Caruth Wagner in the Bureau of Community Health Services. I forget the exact term for the Bureau, but quite low in the hierarchy and none too visible. Certainly it was well below the Social Security Administration, below the level of NIH and below a

number of other both competing and potentially cooperating organizations within HEW. One could say that the location constituted quite a problem for those in the new Center; they had limited access to the power structure that paid for health services, on the one hand, or that did most of the basic research, on the other.

Eventually, Paul Sanazaro was appointed Director. He and I talked about what he should do. My suggestion was that the Center's staff be kept rather modest in size; that he get three, four, or five very capable people to join him; that they tell everybody it's going to take a long time to get any results, and that they are traveling over very difficult terrain. They should say that there is not much to be promised, but they should try to do something that will be useful fairly quickly. They should think rather carefully about what they should and could do and do it well and rapidly.

However, the decision was taken to move more aggressively and more elaborately. The Center's leaders proceeded to take over a number of "demonstrations" from the Bureau (or the Division) of Chronic Diseases in the old Bureau of Community Health Services, I think it was. I'm not sure I have all the terminology precisely correct here. Unfortunately, many of these demonstrations were rejected NIH research applications. They were sort of failed efforts in other quarters. This group of projects did not always have the best people doing the studies. The studies weren't always well designed and they weren't always concerned with important problems, even in health services research, let alone in the general array of health and disease problems.

Accompanying this quite large volume of grants, amounting to something like $40-$50 million as their financial underpinning for the Center initially,

came a host of clerks, administrators and other people involved in looking after the grants. This staff was not always of the highest quality compared to the full spectrum of HEW personnel. For the most part, they were not the same quality as the people in the National Institutes of Health. The transfer of the demonstration grants to the Center brought a lot of disgruntled investigators, who were on the other end of the process. It was quite an unhappy input for the embryonic Center; there was a great deal of administrative effort required to keep the whole thing going.

Paul Sanazaro asked me to be chairman of a Scientific and Professional Advisory Board to help guide the initial stages of the new National Center. We had quite an interesting group; it included Lester Breslow, and Bob Haggerty, who succeeded me as chairman of the Health Services Research Study Section. There was also Martin Feldstein, who was at Harvard and has been advisor to several presidents on national health insurance. We had Rosemary Stevens, the historian of health services and medical care. Ed Connors was on it and also Paul Ellwood, Al Haynes and I think Dave Mechanic. In fact, we had frequent meetings at which we talked about many matters including the organization and structure of health services. I remember one that Paul Ellwood attended in which we talked about something like an HMO involving hierarchically structured prepaid health service. We had a committee of about ten or fifteen people and met every two or three months for, I guess, a couple of years.

We proffered a good deal of advice, not much of which was taken; perhaps it was not worth much. I recall one meeting in which Paul Sanazaro came in and said that the National Center had just been given its marching orders; it had been given three areas to tackle. The first thing they were to concern

themselves with was controlling costs — cost containment was not the buzz word then, but controlling costs certainly was. The second mandate was to improve the quality of care, and the third was to improve access to care.

I asked, "How long do you have to do all this?"

Paul replied, "Well, we should have something to show for it in six months."

I asked, "Where did you get these instructions from?"

He said, "From Wilbur Cohen."

I said, "I don't think any of these things are do-able with the present arrangements; they're certainly not do-able in six months, and above all they are not do-able by anyone in sight at the present time. It seems to me you're going to be in a peck of trouble if these kinds of expectations are raised and you're not going to be able to meet them. You're overpromising far too much!"

Well, I think that was the beginning of what could gently be described as a difference of opinion, certainly between myself, and I guess, most of the rest of the Advisory Board, perhaps all the board, and Paul. We didn't have another meeting for a long time. I think that was our second last meeting; we had one more in which we got Gordon McLachlan over from the Nuffield Provincial Hospitals Trust in London. Later he and Tom McKeown made an insightful report on the whole Center, because Paul Sanazaro had not been entirely happy with our advice. I saw a copy of their report. It was never published or distributed, but essentially it supported the Scientific and Professional Advisory Boards' position on what should be done. It proposed that the Center have a much more modest set of goals in developing the field. In the report they emphasized how difficult health services research is, how complicated the problems are, how little we really knew about it all, and how

limited many of our methods are.

So the Center struggled on. I think I was involved in getting Bob Huntley there. I recommended him and also Ed Connors to Paul Sanazaro; Ed spent a year there. Also I recommended Bob Eichhorn, who was at the Center for a period. I had given all these names to Paul Sanazaro. He had great difficulty in getting first-rate staff and getting them to stay permanently. Tom McCarthy was his right-hand man. He was certainly vigorous in his many contacts with outside agencies, institutions, and particularly individuals. Tom was perhaps a little too enthusiastic in some ways and he had differences of opinion with various senior people -- those were in the early days of Mr. Nixon's first term, I guess about 1968 or '69. As a consequence, Tom took a sabbatical year off. He had one of the AHA fellowships, the Crosby fellowships, with Gordon McLachlan at the Nuffield Provincial Hospitals Trust, in London. It did him a lot of good; he had a good time and learned a lot. He expanded his contacts in Europe and the United Kingdom; he had already a widely developed network of friends and acquaintances in the United States and Canada. He is a gold mine of information about health services research on both sides of the Atlantic.

The National Center for Health Services Research went steadily downhill -- it was very disappointing to me and it's hard to identify all the factors responsible. I think basically there was a discordance between what was feasible and the expectations of the legislators who created the Center. They expected that health services research was going to solve all the problems of costs, quality and access, and that the Center was going to produce quick answers and ready solutions. The politicians seemed to think that things would be better if we just had more clear-cut alternatives for organizing

health services based on research. Of course, this was to some extent cultivated by the image of itself created by the Center and all its promises and activities. It was working on many different fronts. There really was no infrastructure in academia or the hospital system on which to build research. Health care organization was not research based; it had no research tradition; there weren't the research methods to apply. The epidemiologists, the biostatisticians, and the schools of public health had not really entered this field. The social scientists had only limited acquaintance with the biological and clinical aspects of health care. The hospital administrators weren't particularly used to research -- their introduction had really been largely through operations research and it hadn't really contributed much to the improved operation of hospitals. It had perhaps improved some systems and a few designs. In the United States operations research was different from the kind of operational research that was being conducted in Britain. The latter was concerned more with how you improve an organization, how you achieve the objectives, goals and targets that you set up for your organization, than how small sub-systems within a hospital work.

So health services research was not really in the camp of the medical schools, it wasn't really part of the public health schools mission as their leaders saw it, and it wasn't part of the academic courses in hospital administration. It wasn't part of the U.S. Public Health Service's tradition in the operating sense. The field has foundered, I suspect, because of this discordance between expectations and the capacity to deliver. Somehow or other it didn't seem to attract its fair share of the best brains in the business. As you know, it's gone steadily downhill since its inception. Jerry Rosenthal replaced Paul Sanazaro, and he's done a good job. He's a very

able fellow. I remember earlier when we gave him his first research grant at Harvard from the Study Section to do his study of hospital occupancy and costs. He was a bright developing young man then, and he's done a lot to try to help the Center since coming to it. However, I am reasonably confident that health services research will eventually emerge as a viable arena of enquiry and a socially useful, if not essential, enterprise.

About 1970, I was asked by Ed David, Director of the White House's Office of Science and Technology to head a panel on health services research policy for the President's Science Advisory Committee (PSAC) and for a year a group of us including Cliff Gaus worked on the problem. We had a committee that included Ed Connors, Dave Mechanic, Ed Pellegrino, Bob Haggerty, and many of the people who had been on the old advisory group to the Center. We prepared a report, in fact the last one prepared by PSAC under the Nixon administration, before they closed down the Office of Science and Technology. We were there the day that the staff were given the word to move out of the old Executive Office Building. They had two hours notice to get out on the grounds that security needed to be strengthened there. I suppose that was related to the Watergate fiasco and the tapes, but anyhow the whole office was hustled out and closed down shortly thereafter.

Our report was published. Another report done during the same period by a committee under Ivan Bennett, now the acting president of New York University, looked at medical education and manpower. I don't think that report was ever published or released; such were the political influences on science policy at the time. Their report was printed, because I saw a copy; it had an identical typeface and cover to ours except their's was red and ours was black, I think.

In our report we again looked at health services research, and related the

field to health policy formation, health statistics and health information systems. We placed all three components in a model which could really be viewed as a cybernetic or feedback system. The model presumed that the country had a set of health care policies and an adequate health information system; one could then ask questions about the way that a particular policy was being implemented. If a policy wasn't being implemented and achieving its explicit goals, those responsible might like to know the reason why. This loop could, of course, be repeated below the national level at state, local, institutional, and even departmental levels. We made a number of recommendations about broadening the framework for national health statistics and elevating that function in the HEW hierarchy. We also recommended establishment of an Office of Policy Analysis in the Secretary's office, and elevating the National Center for Health Services Research to the Secretary's office.

Our report was, I think, fairly widely read, but it didn't have much impact at the time because Watergate came along, and the administration changed. Many of our recommendations have been adopted by the Carter administration. Health statistics -- a field I've had an interest in for a long-time has been elevated to the Assistant Secretary's office, and there will be a new Center for Technology Research established. It seems to me that the latter may well overlap with the Health Services Research Center. The three Centers are now organized along the lines we recommend in our PSAC Report, with one deputy assistant secretary in charge of them all.

This discussion again brings up the question of why health services research has not been more successful in its application in this country. Our "policy" in this country has been to have "no policy" in health, basically. If

you don't have a policy, then you really don't have a set of targets or goals to achieve and you don't have any occasion to ask questions about whether you're achieving your objectives. As a consequence, health services research has been largely studying epiphenomena. It has been describing what is going on in the contemporary health care system, how people are working, the adequacy of access to care, the level of quality--good, bad or indifferent, and the costs and related outcomes to the extent they can be measured. The field hasn't really been seriously addressing how the whole health care enterprise could be organized to provide more appropriate, cost-effective and compassionate care. If you were running an air line you'd have an operations research unit or the equivalent. The operations research group would be concerned with developing plans for running the air line better. It would be asking whether you could do it more efficiently or more effectively, with less risk, with lower costs, with fewer or different types of personnel, with more satisfaction to the passengers, and these kinds of questions.

So this linkage between health information, health policy and health services research is, I think, now being generally accepted, certainly in other countries, if not in the United States. WHO is thinking along these lines, at least in some quarters of that highly political international bureaucracy. I think these linkages are also being recognized in England, Sweden and elsewhere in Europe. I know in Latin America PAHO is doing work on these problems. The Advisory Committee on Medical Research of the World Health Organization has subcommittees now on Health Services Research and on Health Information Systems; it is broadening the conceptual framework for thinking about such matters.

I am relatively confident that health services research will re-emerge as

a viable enterprise. It may take a somewhat different form once it is related to health policies or even specified goals for improving health status and health services. It may also be institutionalized in different ways in the future.

We might talk now about the International Epidemiological Association. I was asked to be treasurer of it about 1964. It is one of the organizations I have really taken quite an interest in over the years. I was on the Council, the Executive Committee, and eventually I was made Chairman or what is now called President, in 1974. I was succeeded in 1977 by Professor Jan Kostrewski from Warsaw, Poland.

The IEA is a world-wide organization of epidemiologists. It started as an informal club, and then became, in the view of some, a kind of semi-elitist organization. A number of us, including Lester Breslow, who was the President at the time--and a colleague from Nigeria, Addie Lucas, who was President between Breslow and myself -- thought we should apply epidemiological methods not only to infectious diseases, as has been traditional in this country, or to the chronic diseases as was largely the case in Britain, or to other health problems such as accidents, behavioral problems, drug abuse and alcoholism and environmental health hazards, but also to health services. We thought that epidemiology provided a rather simple set of concepts and methods that could be used to examine the adequacy of health services. These had really been borrowed from the fields of survey statistics, agricultural experimentation and demography.

In fact, the methods all had the same origins in what Sir William Petty, the 17th Century physician, economist and mercantilist, used to call "political arithmetic." He is a rather interesting person; indeed, another

one of my heroes. I might discuss him for a minute. He was a brilliant medical student and went on to do graduate work at Utrecht, Amsterdam, Leyden and Paris. He was made Professor of Anatomy at Oxford at the age of 28. But he had a deep interest in the adequacy and utility of the medical, and welfare services provided at the time. He was one of the early mercantilists and a precursor of Adam Smith as an economist. He was a member of the Royal Society and invented one of the first copying machines and conducted one of the first population surveys ever carried out. Generally, Petty is regarded as the father of demography, statistics, economics, sociology and epidemiology; they are all derived from the same kind of quantitative approaches advocated by Petty. He used to say that he liked to be able to "weigh, measure and count things." He felt that it was important to have quantitative approaches to evaluating social services and political efforts designed to "do good" and to "help" people, and to monitoring monetary expenditures which he regarded as symbols for energy expenditures. So Petty inveighed against the limitations of the health and welfare professionals of his day and looked at them critically. He was certainly one of the truly original thinkers in health statistics and epidemiology; the present generation should know more about him.

All these fields really had a common background; the demographers, the social scientists, the epidemiologists and the statisticians were separated from one another as a consequence of the organization of universities and the institutional barriers that faculties succeed in erecting between their intellectual colleagues in different departments. The disciplinary boundaries had nothing to do with the problems each was studying. They do have something to do with human behavior and the way universities and related institutions are organized and administered.

At any rate, we succeeded in broadening the base of the International Epidemiological Association and in increasing the membership substantially. Actually, we were able to expand it to well over a thousand. Eventually, we got the International Journal of Epidemiology started and it has flourished. There have been numerous conference proceedings published over the years. We also succeeded in getting a number of manuals published that dealt with health care planning, health statistics, primary health care and the teaching of epidemiology. Regionalization in Puerto Rico was the subject of another manual. It was a classical case study of the use of health information for the planning and development of health services, including the creation of a medical center and its university hospital in Puerto Rico. It is really a model of regionalization that needs to be more widely understood in this country. John Grant of the Rockefeller Foundation staff had brought ideas about regionalization from his earlier experiences in China. He worked with Guillermo Arbona, the architect of Puerto Rico's health services. Arbona and a colleague of his, Annette Ramirez Arellano, a very competent woman, used the Rockefeller Archives and local documents to reconstruct the history of regionalization in Puerto Rico; it is a classic and should be read by all students of health care. We did another manual on primary health care of basic health services in developing countries, from an epidemiological perspective. A third one which was largely written by George Knox of the University of Birmingham, England, dealt with the uses of epidemiology in health care planning. He prepared it under a contract between the HEW Bureau of Health Care Planning and the IEA. The book had an international set of contributors and four subeditors. It has sold well, because it takes a generic look at the use of quantitative measures in health planning that I

don't think has been done before.

We did another manual on the teaching of health statistics to medical undergraduates and methods for introducing quantitative concepts and principles to them; unfortunately, most physicians are largely non-numerate. They simply do not understand the application of numbers to matters of clinical judgments, to assessing the burden of illness in population, or evaluating the impact of services, to say nothing of assessing the efficacy of various forms of medical intervention.

The International Epidemiological Association was an organization I spent a lot of time on; I hope it has had some impact. We have worked closely with WHO Headquarters and its Regional Offices. Perhaps the IEA had less impact in this country, although it has had considerable in other countries. A lot of people in the U.S. epidemiological community have taken a rather narrow view of epidemiology and have felt that the IEA's efforts to broaden the use of epidemiological methods was undermining the purity of the discipline. There were those who thought that somehow its intellectual purity was being diluted by applying these relatively simple concepts and methods and letting anybody apply them who wanted to. But I haven't agreed with this view. It seemed to me that if you have a set of skills and useful methods, they could and should be applied to any problems for which they are suited. Most advances in science have been made by transferring ideas or methods from one field to another; this is the process of lateral thinking which brought us, for example, our understanding of DNA.

The other major area that I've been interested in for many years is health statistics, as I mentioned earlier. Ossie Sagen asked me about 1965 to be on the Public Health Records and Vital Statistics Committee, I think it was

called. It was an Advisory Committee for the National Center for Health Statistics, concerned with the type and scope of health statistics, particularly vital statistics, in the more traditional sense. Sagen was interested in broadening the field to embrace more than the registration of vital events. That committee was related to, but at a lower level than the U.S. National Committee on Vital and Health Statistics, which traditionally had been concerned almost exclusively with the International Classification of Diseases.

I had been a member for a year or so when in 1966 I was asked to address the biennial meeting of the National Conference on Public Health Records and Statistics. Again I think it was at the Shoreham or the Sheraton Hotel in Washington, D.C. Ossie Sagen and I had cooked up the idea of a Cooperative Health Statistics System in which there would be tabulations or aggregations of health statistics at the local, state and national levels, using sampling methods where appropriate, and full reporting where needed, as in the case of vital statistics. Such a system would link different statistical reporting systems at successive governmental levels so they had more of a cybernetic relationship to what one hoped eventually would be health policies for these different jurisdictions; the data could also be used for health services research. This meant taking, as the basis for the presentation of the statistics, a denominator derived from the characteristics of the general population, in contrast to the traditional preoccupation in health care statistics with institution-based statistics, where only the numerator part of the data is used. Institution- or practice-based statistics, in contrast to population-based statistics cover patients who have come to sources of care, they omit those individuals who don't seek or receive care and who might

benefit from care. Population-based statistics are a major source of measurements of unmet need and of coverage and distribution of services.

At the 1968 meeting I advanced these ideas to the assembled health statisticians of the country. That was how the Cooperative Health Statistics System was launched. Ossie Sagen prepared a brochure which was widely distributed. Unfortunately, the CHSS has fallen on hard times, for a variety of reasons, and its existence is threatened now.

About the same time, in 1968, I was still pursuing my interests in hospital statistics to which I had been introduced at Chapel Hill by Vergil Slee. When I wanted to introduce the PAS system in North Carolina Memorial Hospital, the proposal had been turned down by the Department of Medicine, on the grounds that as a medical school faculty we should know what we were doing and as a university hospital we shouldn't have to worry about external audits or inquiries about the quality of our work. In England I had encountered the Hospital Inpatient Enquiry (HIPE) as well as the morbidity studies that had been conducted in general practices. These had been done by W.P.D. Logan, when he was Registrar-General in Britain. He later went on to become head of statistics for WHO. The Hospital Inpatient Enquiry was a most interesting series of reports on a 10% sample of all the patients hospitalized in Britain; it provided data on diagnosis, lengths of stay, place of residence, and other characteristics of the patients. It really hadn't been studied much. I remember George Godber once saying that I seemed to have studied it more and know more about it than most of the people in England. It seemed to me they had never really exploited this important source of information about the most expensive component of the British National Health Service.

When I was at the University of Vermont I conceived the idea that we should try to introduce something like the HIPE in that state and put it into all the hospitals. I had a grant from HEW and started negotiations with Vergil. We had a great meeting in one of the motels in Burlington, Vermont. I presented the idea of population-based hospital statistics and indicated that we were going to fund PAS through our federal grant in order to install the system in all the hospitals in Vermont, albeit a small state of four hundred thousand people, or less. At least we should be able to get population-based statistics and compare variations in admissions, case fatality rates, costs and so on within and among hospitals and townships. In principle, he agreed with this idea, although, to this day, I am not certain that he really understands the difference between institution-based and population-based statistics.

We put in quite a lot of time on the exercise. We had to negotiate a separate contract with every hospital in the state as well as with the Vermont Hospital Association. We had meetings with lawyers and we had meetings with hospital administrators and we had meetings about meetings! It was harder work installing the system than it was looking at the data later. I left before that was completed, and state-wide analyses could be started. The actual completion of the installation was done by an interesting person, John Last, an Australian, who had been in London with Jerry Morris and then in Edinburgh with John Brotherston. He joined Brotherston after he left Vermont. I knew of Last through Jerry Morris. He had written a most interesting article in Lancet, "The Iceberg"; it is a kind of classic, showing the actual distribution and expected distribution in a general practice of the different kinds of medical problems a general practitioner might expect to see

in the course of a lifetime. I had asked John Last to join me at the University of Vermont; it was he who helped put the PAS system into the hospitals there. He went on to Edinburgh and then went to the University of Ottawa, where he has been Professor of Community Medicine for about ten years or more. He spent 1979 at Mount Sinai Medical School in New York revising the Maxcy-Rosenau book, previously edited by Phil Sartwell of Hopkins, on Public Health and Preventive Medicine. He completed the mammoth task of bringing this huge volume up to date; it is the leading text-book in the field. Last is an interesting person who has contributed a lot to epidemiology, health services research and public health.

Anyhow, the PAS hospital data in Vermont was later exploited by Jack Wennberg and his colleagues and was the basis for a widely-quoted article in Science on small area hospital statistics. In many ways Wennberg's work is related to the ideas that John Griffith has been working on in his studies of population-based statistics in Michigan. One of our troubles in Vermont, and even nationally, was that Vergil Slee never seemed to be able to see the potential for PAS in covering whole populations and using the same data not only for the purposes of institutional management of the hospital and for the quality of care/medical audit approach he was employing, but also for purposes of planning health services, for determining resource allocations and for setting priorities. He never seems to have been able to see this. Similarly, he had never been able to see the oppportunities for regionalization of his system by states or groups of states. We had many discussions about this when the PSRO was starting. I tried to get PAS into Maryland when I was at Hopkins and tried to get Vergil to do the same thing in Maryland we had done in Vermont. I said to him, "If you would only regionalize and cover a whole

state, then you could serve these multiple purposes. You might not get the whole country under PAS, but if you could stake out places where you could cover all or nearly all the hospitals in the state or a couple of states together, you could make a contribution that is far and above what you will be able to make now. If you expect to be the sole or predominant national health data system, I think you're going to be involved in great struggles and enormous competition with the Blues, Quest and the other data systems then in place to say nothing of the state and federal health data systems." It was all to no avail. However, later through state legislation related to the Maryland Health Care Cost Commission we did get complete coverage of all hospitals with Uniform Hospital Discharge Abstracts. We had conceived the idea at Hopkins that PAS probably had too much "data" and not enough "information." There was so much data collected, that as far as I could understand, it was never all going to be analyzed. I developed the notion that if we could have Uniform Minimum Basic Data Sets and could agree on a small number of items to be abstracted, we could then use these as the core for any health information system. So with a colleague at Hopkins, Jane Murnaghan, I arranged a conference at Airlie House; a series of prepared papers were discussed in detail and formal recommendations agreed. Ted Woolsey who was then Director of the National Center for Health Statistics, was there and so was Vergil; we had people from Britain and Canada and from all of the health data systems in the U.S. We published a Supplement to Medical Care on the conference. Then there was a separate volume on the Hospital Discharge Data Set. We got HEW to review our recommendations and to promulgate fourteen items for the basic data set. That was back in 1969.

We then went on, just to continue this line of thought, with two more

conferences, one on Ambulatory Care Data Sets and one on Long Term Care Data Sets. We got HEW to work on these also. They've just finished the Long Term Care Uniform Data Set and the Ambulatory Care one is almost finished. The idea being that these data sets would constitute a minimum number of terms and definitions agreed to by everyone. The sets would be analogous to those used on death certificates and birth certificates. They would be reviewed and revised in an orderly fashion every five or ten years. Certainly those who developed these sets were not in a position to say,"This is the 'right' set, or the 'only' set, or the 'best' set," but they were data sets that a knowledgeable group agreed upon and each should be used for a period and then revised on the basis of experience in an orderly fashion as we do with death certificates. Each data set would be used in constructing one component of the Cooperative Health Statistics System.

A struggle emerged with respect to PSRO and the uses of these minimum basic data sets over the following ten years from '69 to '79. The issue of their use remained largely unresolved by HEW. The control of hospital data has veered back and forth between the National Center for Health Statistics and what was previously the Social Security Administration and is now the Health Care Financing Administration. If one ever had any doubts that information is power, one should examine the history of this ten year period and the struggle over the control of hospital data. It is clearly providing information about what doctors do and what services hospitals provide. It is the basis for comparing each hospital to others with respect to type of admissions, diagnosis, lengths of stay, case fatality rates, adverse drug reactions, rates for operative procedures, caesarians, and a whole series of interacting factors that can be examined statistically.

I have always argued that we should use a behavioral or cybernetic model in organizing and using health information and statistics. We should proceed on the presumption that hospitals, doctors, administrators and others want to do the best they can and that information should help them to learn from experience and to modify their practices and behavior. This approach seems preferable to a policing or punitive approach. It also uses a population-based approach rather than an institutional approach to thinking about health services. By using a population-based system that covers all the health and hospital statistics for states or regions, one can then compare variations among regions, states, counties, and other "catchment" areas for such measures as admission rates, lengths of stay, case fatality rates and operative rates.

One can then ask: "What are the observed differences in rates due to?" One seeks explanations for the variations and these may be associated with many complicated factors. It may be true that morbidity is much higher in one place than another or it may be true that a particular institution attracts more difficult cases than others. Or it may be that a particularly gifted surgeon in one place attracts patients with complex problems from a wide geographic area. Or it may be that the reason the infection rates are higher in one hospital is because the resources are not adequate to provide the necessary staff or equipment. They don't have enough scrub nurses or adequate infection control systems. If one looks at these variations dispassionately it is not necessary to bell cats or say exactly which shoe fits which foot. The statistics can be made available to those who need to know and someone in charge can be made available to those who need to know and someone in charge can say: "Look, there is a difference among these hospitals, now we suggest

that the responsible trustees look inside their own hospital and see whether they can find an explanation for the difference observed." If they can't find an explanation at the level of the overall hospital administration, they can examine practices within clinical and services departments and eventually may go down to the level of individual practitioners. By adopting this approach one uses a behavioral model in contrast to the PSRO system, which starts with isolated individual cases that seem exceptional in some way. The medical audit tends to assume that the doctor didn't do what he was supposed to do or conform to some prescription or normative practice. There's usually an acceptable explanation for individual aberrant outcomes. Either the patient was not able to leave the hospital because the mother-in-law tripped on the ice and broke her leg, or there was no nursing home for the patient to go to, or in some way it was a "special case." The patient, for example, may have had not only heart failure but also diabetes and something the matter with his skin or two or three other things. There's always an explanation for individual case deviations from so-called "norms." When you look at things statistically, however, the laws of large numbers take care of individual variations; if you observe different patterns for different hospitals or different doctors, then you can look at them more critically. You start with large numbers and may or may not end up with individuals; if you start with the latter you meet with immediate resistance from doctors and have a high probability of being wrong.

Over the years, I became increasingly interested in health statistics and read Florence Nightingale's early writings on hospital statistics. She used to raise penetrating questions about the use of health statistics with the medical and political establishments of the day--this was 125 years ago. Here

we are 10 years after the Airlie House Conference and a 125 years roughly since Florence Nightingale introduced the idea and we still don't have a hospital discharge abstract system or even a minimum uniform data set adopted by the federal government of this country. They are still fighting about it and still haven't agreed on a set of fourteen items. The political turmoil goes on!

I have always been interested in ambulatory medical care, but as far as I could make out, we had absolutely no data about what went on in doctor's offices. It was a complete "black box," a mystery. I had been interested, as I mentioned earlier, in Bill Logan's statistics on general practice in which a group of volunteers in Britain collected data on the patients who came to see them in their general practices. Although it was a limited study it was also most informative. I had been stimulated by it and, with John Last, had done studies with a group of general practitioners to describe the medical problems brought them. We published an article in Medical Care in 1969. I started talking about the lack of statistics on ambulatory care when I was on the Public Health Records Committee with Ossie Sagen. I also talked to Forrest Linder a demographer, who was then Director of the National Center for Health Statistics, and to Ted Woolsey who was then his deputy and later his successor as Director. Forrest Linder thought this was an idea ahead of its time and he wasn't prepared to get into ambulatory care statistics. He was more interested in improving vital statistics. But Ted Woolsey said he was interested in the idea, and, when he became Director, he said he would like to see a National Ambulatory Medical Care Survey (NAMCS) introduced before he retired. I started to work with the staff of the National Center. We did a great deal of pretesting for the NAMCS. We used Lea & Associates, the drug

marketing outfit, now owned by CBS, I think, but we found their field work was not up to the standards expected by the National Center of Health Statistics. Everybody I talked to told me a survey of doctors' practices could not be done but we kept working at it. We worked with all the specialty organizations, with the AMA, the AAMC and the state medical societies and eventually got the support of all. Finally, we mounted the study and the National Center for Health Statistics has had a response rate from a national probability sample of office-based physicians of around 80%, between 78 and 82%; it is really quite remarkable. It would be nice to have it higher but when you remember that this is not just a quota sample of doctors who agree to play or who volunteer to participate, but a scientifically drawn sample. One has to respect the willingness of the profession to respond to a social need. The NAMCS has been going on for about four or five years and there have been at least three published annual reports as well as numerous small studies reported. Currently, I believe it is the most frequently requested set of publications from the National Center of Health Statistics. For the first time it is apparent that about twenty-four common problems account for about half the new visits to physicians in their offices and about eight of these are associated with pain. They are common pains one would think of: back pains; leg pains; arm pains; neck pains; chest pains; headaches; and I guess, a fair number of heartaches and responses to life events that take people to physicians. NAMCS for the first time has given us a description of what problems doctors see in their offices, how people present them, and what is done about them. We are trying to do the same thing in long-term care as well but it may take longer. Of the three basic elements of care, hospital, ambulatory and long-term, the survey of ambulatory care has been the most

successful. However, it seems to be inevitable that we will eventually have national hospital and long-term care statistics.

About 1974 or '75 I was asked to be chairman of the National Committee on Vital and Health Statistics. I took that on and we pursued the development of the data sets actively. During that period, we were able to get written into the legislation, (I remember testifying before Ted Kennedy's committee about it) a statutory requirement that there be an Annual Report to the President on the health of the nation. This became the volume Health: United States published first in 1977 and annually thereafter. This publication pulled together all the more important national health statistics in one annual volume. One reason for urging this was the need to get some coordination among the 168 different data sytstems in HEW. They seemed unrelated to one another; there was no basis for comparison among the different systems. It should be possible to use similar age breaks, recall periods, definitions and other standardizing attributes to enable one to compare data from the National Health Interview Survey, for example, with data from the Health Examination and Nutrition Survey, or from the National Ambulatory Medical Care Survey, or a hospital discharge survey with data from a long-term care survey and all of them with vital and mortality statistics. There should be a spectrum of measures which would enable one to look at health status and health services in a broader context. Such an integrated system would provide an information base for setting priorities, allocating resources and organizing health services. This annual volume has turned out to be very popular; the last version was just released recently and Julie Richmond was on television describing the highlights.

I remember when the first volume came out Dorothy Rice had just taken over

as Director of the National Center for Health Statistics. It was her first day in office; she got a phone call from the Secretary because this volume had made the front pages of the New York Times and the Wall Street Journal. The Secretary, is reported to have said, "What the hell is this document? I have never even seen it and here it is making the front pages? I'm getting calls from the White House, etc."

Well, as it turned out when she had exhumed the memos, the publication had been "signed off" by somebody in the Secretary's office, although the Secretary himself had never seen nor heard of it. He hadn't even been briefed on it. Since the Secretaries of HEW are said to read the volume themselves! Even more popular was the Chart Book to accompany the main volume; it provided graphic illustrations of the major findings. It too turned out to be a best seller. It was easily understood by politicians and people in other walks of life who cannot read a table but at least can see whether a bargraph is tall or short, wide or narrow, or a trend line is going up or down. Once they grasp the implications they send their aides to inquire further about what this all means. "How is my own constituency doing with respect to these changes?" they ask. It's a powerful way of enlisting the interest of politicians. I called it the "comic book" version of Health: United States but it has done quite a bit to educate the public and the politicians about health matters.

As part of my interest in health statistics I recall another concern -- a meeting that we organized again in the mid-sixties when the 7th revision of the International Classification of Diseases (ICD) was coming out, to try to get it oriented for use in hospitals and not solely for use on death certificates in classifying the causes of death. The WHO version is still

focused on causes of death, largely because the original people interested in disease classification were pathologists. They were trying to identify the anatomical changes associated with different diseases. Those who made the original recommendation that we count the dead, I guess in the days of Chadwick, had argued that if we were going to count the dead, we should try to find out what they died of; it was a reasonable way to look at things. So, the classification system was in the hands of increasingly specialized clinicians and pathologists working at the terminal end of the spectrum of health and disease for the most part; this emphasis continued for roughly seventy years from the ICD's origins in Paris and London. The ICD has evolved and it has served its purposes well, but death is only part of the spectrum of health problems. The question is whether we shouldn't now begin looking at what goes on in hospitals, at how people use them, and then at ambulatory care. Why not even start with the perceptions of people about why they seek help for their health problems and what their expectations are? However, this meeting, which I remember Don Reidel attended, didn't amount to anything and nothing much was done. Not only that, but the National Center for Health Statistics was unduly slow in producing an adaptation of the International Classification of Diseases for the United States and in publishing it in English. HEW had the responsibility for doing the adaptation, which was an attempt to broaden it a bit from the WHO version but it came out extremely slowly.

Well, by that time Vergil Slee was interested in having a disease classification that would fit his PAS needs in hospitals. The general practitioners in Britain had done a study in the early fifties which showed that only about 45% of the problems brought to a source of primary care or

general practice, could be given an ICD diagnosis. These were "problems" not "disease"; they were just aches and pains, and there were social problems, occupational problems; people "hurt," or they couldn't sleep, or they were "blue," or depressed, or anxious. Similarly the rubrics of the ICD were unsuitable for hospital use. So Vergil went to work, and he deserves a lot of credit for this, and developed his own H-ICD, which was his version of the official WHO version. He published it for use with PAS. We started then on a course which involved the use of ICD-7 and H-ICD-1 and then H-IDC-2; next we had ICD-8. We had four ICD versions going at one time in the USA with the WHO very slow to modify theirs for what was to be the ninth version.

I was on a WHO Expert Committee on Health Statistics in 1971, which John Brotherston chaired. I was made rapporteur; it was an onerous task because you work like the dickens for a week to grind out these reports. Anyhow, our report was an attempt to broaden the conceptual basis for gathering health statistics. We staked out new terrain for health statistics in that report. I think there are still ramifications of it that are having some influence. I did another study for WHO a couple of years ago on National Health Information Systems and Health Data. We thought we had resolved the controversy about these four classifications in the 9th revision of the ICD by asking that Vergil be put on the U.S. delegation to the WHO revision meeting in Geneva. At that meeting, he agreed to the proposed contents of the ninth revision and it was reported that he was satisfied that it met the needs of hospitals. At least this is the way it was told to me -- I wasn't there. I was asked to go, but I couldn't do so at the time. I was told that as a member of the American delegation, he, together with all the other members, had approved the ninth revision and everything was satisfactory.

Well, Vergil got back to the states and proceeded to produce a new ICD-9CM, his own version of ICD-9. Meanwhile WHO had promulgated its version. I asked when HEW was going to produce its adaptation because the American version was going to have to be produced promptly, if Vergil was not to capture the field. Well, HEW had no budget item for the ICD. The people in charge at the National Center had not been told about the ICD; there had been no provision in the budget to print it. We were in the position of having a disease classification scheme that had to be used by Medicare and Medicaid, for the PSROs, for planning and other public purposes that was being expanded in ways that might not necessarily always serve the public's needs equitably. I went to at least one meeting at Vergil's shop in Ann Arbor where various pressure groups expanded sections of the ICD without reference to the frequency with which the conditions to be labeled occurred. For example, the ophthalmologists were interested in expanding their disease categories in great detail. When one inquired about how often these particular diseases occurred, the answer was "not very!" In fact, I don't think any of them had seen a case of some disorders for which they were preparing labels. If you're really going to do research on rare conditions you need to get all the possible cases and then examine the medical records of patients in great detail. There isn't much point in having highly specific labels for the things that are of very rare occurrence. Interested investigators are not going to take the "say so" of somebody who has only seen one of these cases just every so often. There were disproportionate expansions of various classifications in Vergil's version. There were many other problems. It seemed to me that the detail used in classification of diseases and health problems should be based on the distribution of the problems and their

frequency in the population, rather than on the interests of the profession, and certainly less on the preoccupations of statisticians! You want to have the people's health problem related to public policy about health and health services; that is why a country has health statistics.

There were lengthy negotiations and eventually a committee was set up to deal with the persistent bureaucratic squabbles between the National Center for Health Statistics and the Health Care Financing Administration about who was going to "control" the International Classification of Diseases, to decide when it was to be officially adopted and how it was to be used by the PSROs, Medicare, and Medicaid. Certainly Vergil Slee deserves a great deal of credit for going ahead with the initial publication of his version of the ICD. On the other hand the ICD really is in the public realm, and I think it should not be a proprietary document. It should be paid for and produced under public auspices. I hope this will be the last version that will come out under nongovernmental auspices. I hope it will be revised and promulgated more rationally in the future.

That brings us up to other problems of illness and disease classification, particularly at the level of primary care. We did have a classification scheme developed for the National Ambulatory Medical Care Survey that was based on the one originally developed by the Royal College of General Practitioners in Great Britain. In the meantime the Royal College of General Practitioner's classification for ambulatory care had undergone revision by an international group sponsored by the World Organization of National Academies and Colleges of Family Medicine and General Practice, generally known as WONCA -- the acronym for this complicated title. Well, this group emerged following the London meeting in October, 1962 where, as I mentioned, I had used the term

"primary medical care." The same group had a second meeting in Montreal about 1963. In fact, I had several of the leaders down to visit us at the University of Vermont where we had a Family Practice Unit in my department. These were general practitioners interested in research from the Royal College of General Practitioners.

It included John Hunt, who was then president and founder of the College; he has since been made Lord Hunt. He had been down to talk to the American Academy of General Practice, as it was then called. He had thrown up his hands in dismay. He said, "These fellows are just interested in their payments from Medicare and Medicaid; they are obsessed with 'socialized' medicine, and so forth."

Little did they know that John Hunt, who had started the Royal College of General Practitioners, has never joined the National Health Services in Britain; he has always been a private practitioner. Hunt was not therefore "tarred" with the brush of the National Health Service in Britain, but was quite independent of it.

The GPs of those days in the U.S. could not understand the research aspects of primary care and the opportunities for studying this fundamental level of patient care; they missed the challenge and the need. Well, we had a wonderful visit from Robin Pinsent, Stuart Carne and John Hunt. The London and Montreal group had subsequent meetings in Salzburg, Toronto, and Melbourne. They formed WONCA and set up a research group and a classification group which have developed what has now become known as the International Classification of Health Problems in Primary Care (ICHPPC). The American Hospital Association published the first version of it in 1975. Mary Converse who is in charge of the ICD group at the AHA was responsible for accomplishing

that and it was widely adopted by family physicians and practitioners in this country, as well as in Canada, Australia and Europe. But still ICHPPC was not completely compatible with the ICD-9 so I arranged a meeting in Montreaux, Switzerland in 1978 during the WONCA International Conference. I got people from the ICD group at WHO Geneva and the WONCA group together and others from the U.S National Institutes of Mental Health. We contrived to get a second revision of ICHPPC developed that was compatible with ICD-9. This has recently been published as ICHPPC-2 or ICD-9 GM for General Medicine. It is now an official WHO Classification for world-wide use and is published by Oxford University Press. Now, for the first time the classifications for primary care, hospitals care and causes of death are compatible with each other. More recently we had a meeting to see if we couldn't do something further to restructure the underlying conceptual model for what I hope will be called the International Classification of Health Problems and Diseases - 10th Revision.

In 1980, we brought together a group including representatives from primary care, mental health and the ICD from a variety of countries around the world, to consider the best way to code psychological and social problems in addition to physical problems and diseases in a Triaxial Classification. We're going to field-test this in eight to ten countries and hope we can get the ICD turned around so that it can be used for classifying and coding the health problems of the living as well as the causes of death for the dead.

Putting this all together, how does it relate to other interests? Well, it takes me back to where I started from, that is the need for improvements in our understanding of the tasks of medicine. It seems to me, we should start educationally and organizationally with the recognition that each patient

perceives some disturbance in his or her well-being, feeling, or functioning, or experiences a sudden onset of something; the individual has an accident or is struck by a blow in the back of the head. There is some disturbance. There has to be a perception of the disturbance and its severity and of the need for help on the part of the individual or a surrogate. They can do various things. They can consult informal sources of care -- the neighbors, the family, the pharmacist or the local shamin; then they can enter a more formal part of the medical care system. Then the labeling process begins and the labeling has all been done by the medical profession in the past.

We tend to say, "If you've 'got' what we say it is, that is what you've 'got'." When we label 'it'; you've got 'it' and that's what we call 'it!" The label is written down and the problem even further reified. The label then is used as the basis for payment or "reimbursement" as we say in the U.S. The real task of the health care establishment, however, is to try to resolve or manage the people's problems, to help patients to cope with them, provide them with care, or provide them information, and where we can, to cure them, or control or manage the problem. Our task is not just to diagnose and label problems. So, first of all, we need an information system that is built up on the perceptions of individuals about their health, and what they call their health problem -- it should be based on a classification of "lay" terms. These should then be related to the classifications, labels and codes that doctors and nurses use for these problems. You want information about the distribution of health problems at the community level in lay terms and at the levels of primary, secondary and tertiary care. The information is also needed in forms that allow it to be aggregated into health or disease indicators. The information system should be designed so that the management

of medical organizations and institutions can so deploy their resources, knowledge and talents that the people's health problems are managed with the most effective, efficient and compassionate services possible. Information of the kind I have described is needed to set policy, direct operations, allocate resources and set priorities to meet the perceived needs of the people. Then you need health services research to tell you whether or not you're achieving those goals and the policies set for the system.

We need, especially at the level of primary care, a scientifically based, compassionate, caring service that deals largely with intangible health problems. As John Updike says, "Problems that have solutions are not problems!" A lot of the health related problems people experience are openended. They are not soluble; there are no cures, and there are no "solutions." It is true, we have more knowledge then we had thirty years ago. The statement I like to make is that only 10 and 20% of the things that doctors and nurses do for, to, and on behalf of patients are supported by any evidence that they are more useful and beneficial than they are harmful or useless. That is not very much. We still have the large "Hawthorne effect" which accounts for 20-40% of any improvement, and the "placebo effect" which accounts for another 30-50% of any benefit. Finally, there is an unexplained effect that seems to be a mystery, if not a source of waste. The purpose of health services research is to try to illuminate the relative contributions of these four elements. If the caring part is as important as it would appear, then we should call it by its right name. We should compensate physicians and nurses for listening to people, for caring for them, and not compensate them for the excessive use of diagnostic gadgetry, equipment and techniques of dubious efficacy for diagnosing or "ruling out" a whole series of diseases

which are highly improbable occurrences at the levels of primary and secondary care. After all "common" diseases are common and "rare" diseases are rare! We should stop paying, at least from private and public insurance funds, for those tests and treatments which are not of established benefit just because we can see, feel and touch them, in contrast to services that are much less tangible but which may be much more beneficial.

It seems to me that we have created a health care enterprise that has become a kind of reified structure. We talk about the health care "delivery" system. I abhor that work "deliver." It sounds as though you could deliver health to somebody as if it were a bundle of something or other, and that you could transfer or provide health in some concrete manner. We talk about "quality assurance"; I'm not sure you can really "assure" quality to anybody. Quality has, in addition to an individual dimension, a population dimension. What is the nature of the quality of care for a population when those people with headaches are led to spend their money for CAT scans to "rule out" the very low probability of having a brain tumor when they are really suffering from hangovers or disputes with their spouses, and when the kids in that population can't get measles immunization? What is the quality of care in that set of arrangements? Quality should have dimensions of equity, efficiency and efficacy for both individuals and populations.

All of that is by way of saying that I have had a continuing concern for the distribution of health problems in populations, and for the social and emotional aspects as well as the physical aspects of illness in individuals. I think one wants to build these conflicts and priorities into a health system and into its information system, into its health services and biomedical research, into management and administration that strives to implement

appropriate responses and services to meet people's needs. I'm not sure that any one person can understand all aspects of such a scheme by any means; certainly not I. Nor do I think there is any right or wrong way to provide health services; there are many different ways and there are choices available. One of the advantages we have in this country is that pluralism provides opportunities to experiment and to develop new ways of providing services. To take advantage of these opportunities I do think that somehow or other we have got to get better information as a basis for setting priorities and making choices. We've got to get a better understanding of the complexities of the psychological, social and biological approaches to health and disease and we've got to get away from this persistent preoccupation with gadgetry and equipment and misapplied technology. My guess is that things are going to get worse before they get better!

At Hopkins I had become increasingly impressed with the negative consequences of the separation of the School of Medicine and the School of Public Health. About 1975 I had decided in my own mind that if another opportunity came along I might like to do something else and I looked at a number of different possibilities in other settings, but none were too appealing either geographically or organizationally. At Hopkins we had a retreat to decide on the future of the School of Public Health. I argued that the two schools should be merged because the separation didn't seem to me to be dealing with the problems of getting the population-based perspective into the mainstream of medicine. Nor was the separation getting the best people in the mainstream of scientific medicine, that is those in the medical school, involved in the uses of epidemiology and other quantitative methods, or in the problems of organizing more sensible health services. Although it is true

that Sam Shapiro's Health Service Research Center, which is closely related to my former department by virtue of him and his colleagues being members of the department, has done very well. I decided that if something came up I might be interested in a change. Also there were younger people in the department and I felt they should be getting a chance to move up. I'd stepped down as chairman, and Phil Bonnet who was chairman would retire shortly. He had joined us originally to start a program in hospital administration. We really had not gotten very far in that unfortunately. For a variety of reasons that was a great disappointment for me but Phil is a fine person and he ran the department after I stepped down. I thought, however, that I had probably made all the contribution I could make at Hopkins.

In the summer of 1976 I went off to Australia and New Zealand. Gordon McLachlan had been on an advisory committee to Joe Terenzio at the United Hospital Fund in New York. They had been talking about setting up an Institute for Health Policy Studies or an Institute for Health Care Studies. They had a second meeting with the advisory committee; I think Paul Densen was on it, Dave Mechanic, myself and Gordan McLachlan and maybe one or two other people. At any rate, Joe Terenzio offered me the job as head of this new entity, it was to have a fairly reasonable starting budget and there were promises of additional funds. We were to undertake health policy analysis and health services research. I was never quite sure what health policy analysis meant, so I was more concerned with health services research and what could be done about developing that field. I had also indicated that I would take a broad view of health policy even an international one. We would certainly not limit our interests just to New York City or parochial problems.

Tom Bice was my deputy there and a good colleague. We had wanted to

undertake a number of studies. For example, I was interested in looking at the use of barium enema x-rays in New York hospitals. The preliminary studies suggested that only one out of about every 100 barium enemas or even more than that were positive. That sounded like pretty poor guessing on the part of the clinicicans with respect to the potential yields from these tests. It is not an entirely benign procedure. In fact it is quite a stressful test for the patient and not without its deleterious effect on older people; it can even result in death. Certainly the examination can be embarrassing for many people. So, if it's not an entirely harmless procedure and if only one x-ray examination in 100 is abnormal, you wonder how useful all this negative information is. We are going to look at this. I wanted to employ a very bright woman who has since gone on to greater things. I made arrangements for her to join us. Joe Terenzio had agreed to this. He then changed his mind and said we couldn't find the money for the salary that we had agreed upon for her. He never really turned over the budget to me completely and I felt that he went back on his promises.

So after about a year I decided this wasn't going to work and Joe Terenzio and I reached an impasse. I started thinking about what I would do. John Knowles had asked me earlier and Ken Warren asked me again if I was interested in joining the Rockefeller Foundation. I weighed that against three or four other opportunities to return to universities or to join another foundation. I decided on Rockefeller as the most interesting prospect. I always had some interest in foundations and in the Rockefeller foundation in particular. We had had funds from the RF at the University of North Carolina in the 1950s; they helped us to start epidemiological and health services research in the General Clinic. I had been particularly interested in some of

the initiatives taken by Rockefeller, particularly in establishing schools of public health. The Rockefeller Foundation had supported the first school of public health at Hopkins when it was proposed by Welch, who was then Professor of Pathology, and later Dean of the Medical School there. He had argued that there were problems of tropical diseases, on the one hand, and of hygiene and environmental sanitation on the other, that were neglected by medical schools. As a consequence there was an urgent need to train personnel who could organize public health services; to do this he proposed that a separate school be established, apart from the medical school.

There had actually been quite a difference of opinion between Harvard and Hopkins as to what should be done. The proposal at Harvard was that its School of Medicine should collaborate with MIT so that the teaching of so-called "preventive medicine" would be carried out primarily in the medical school. The preparation of engineers, however, would include training in medicine and biology at the Harvard Medical School and in engineering sciences at MIT. The latter group would become a new breed of sanitary engineers to deal with the environmental aspects of health services, at least that is the way I understand it. That proposal was turned down by the Rockefeller Foundation. There was a good deal of academic and political intrigue between the various groups at Harvard and Hopkins and in their relationships to the Rockefeller board and some of its members; or so it is alleged. Some of this has been recorded by Greer Williams in material for a chapter for a book; it was never published. The material is in the archives of the Rockefeller Foundation; several years ago I got Willaims to publish a shortened version of the material in the Milbank Memorial Fund Quarterly.

So, the first school of public health was set up at the Hopkins and then

subsequent schools were established in London, Zegreb, Tokyo, Toronto, Rumania, and I think in Bratislava, Czechoslovakia. The question now is not whether subsequent schools were established in London, Zagreb, Tokyo, Toronto, Rumania, and I think in Bratislava, Czechoslovakia. The question now is not whether this may have been a sensible choice, given all the circumstances at the time, but whether this is still a useful or even viable arrangement.

When John Knowles became president of the Rockefeller Foundation, I had written him and said, "Why don't you look into schools of public health because I've seen the inside of these places and there is an urgent need for change." I also had come to know the Harvard School of Public Health quite well because when Howard Hiatt took that over as Dean I was on his informal advisory committee. We had a number of discussions about the arrangements there and the need for change. I had also been offered deanships in five or six schools of public health in different places and approached about jobs in most of the other major schools. So I had had a chance to look at them in considerable depth but I had never really wanted to take on a deanship in a school of public health because it didn't seem the right institutional arrangement for tackling many of the contemporary problems of health that interested me.

When joining the Rockefeller Foundation I said to John Knowles, "Why don't we have a look at the schools of public health and departments of community medicine and consider the future of this area of growing national and international concern. The person to do this is John Evans who was the founding dean of McMaster University Medical School and subsequently president of the University of Toronto."

John Evans and I had talked some years ago and he had, I suspect, partly at my suggestion, but also I'm sure for his own good reasons, merged the school of public health and hygiene and the school of medicine at Toronto.

The merger involved Burns Roth who was teaching hospital adminstration with John Hastings and others. There was much screaming and yelling and I guess some blood on the ceiling, but they did put the two schools together and everyone is relatively happy about it. It has done very well, although perhaps not as well as some had hoped. John Evans was running for political office in Canada and might well have been the next Prime Minister of Canada if things had gone differently. But fortunately or unfortunately he wasn't elected. I knew he was available, and we persuaded him to help the RF as a consultant in order to undertake a study of schools of public health, and of programs in community medicine and in health administration. He was to look at the problems and issues on a worldwide basis. We will have a report from him in 1981, followed by an international meeting and I hope it will make some kind of a difference.

I don't feel that either the Kellogg report or the Milbank report on the same problems really came to grips with the issues. I urged at the time that the Kellogg and the Milbank commission members get together and do a joint report but to no avail. What is this health administration business anyway? You can't really have the institutions run separately from the rest of any health care organization. To the extent that the traditional public health people are involved in administering Medicare and Medicaid, as well as all the usual categorical disease programs, and that they're responsible for PSROs and quality control, they are clearly concerned with health care organizations, if not with health services systems. Similarly, the administrators of health care institutions are involved in systems of care. They both need information. They both need policies. They both need managerial skills. The intersecting educational needs for the functional merger of public health and

hospital managers has never really been tackled. I've been around some of these institutions and organizations and wherever you go you find the same needs. First, you've got to have managerial skills to run the institutions and organizations in an integrated purposeful manner. Second, you've got to have a quantitative awareness of: (a) what the perceived health problems of the people are; (b) what can be done that's useful and helpful and has some scientific evidence to support its alleged benefits; and (c) what impact the organization or institution and its services have on the health status of the people served. The latter amounts to the monitoring of services. So "measurement, management and monitoring," are the three sets of skills needed. John Evans, I and many others think these should be the educational focus for those who are responsible for developing the health services of the future. What institutional framework can best accomplish this is up to each institution to determine for itself. I believe that both the Kellogg and the Milbank reports were lacking in their perception of what the overall problems are and that they might have provided a much stronger report if they had merged the two commissions.

I see John Evans as a kind of latter day Flexner, although he is looking at a different set of problems. Flexner brought the natural sciences into medicine by, in effect, saying: "Look there are the basic sciences of biology, physics and chemistry that need to be incorporated into medicine; no longer can we rely primarily on a doctor's 'say so' and empirical clinical experience to guide our activitites." Now, seventy years later, there is a need to bring the social and measurement sciences of demography, epidemiology, economics, sociology and statistics, and the behavioral sciences of psychology and anthropology into medicine and to try to understand this other part of the

health care enterprise. We need to merge both aspects of science, the natural and the social and behavioral, with the best of technology and the best of managerial skills. So that is one of the aspects of population-based medicine that we're currently interested in at the Rockefeller Foundation.

Also we are trying to get clinical epidemiology introduced into medical schools. We hope to develop this discipline in departments of medicine and other clinical departments so that we can encourage clinicians to think more broadly. They need to think not only about cellular and molecular events, and about the one-to-one relationship between patient and physician, but also about the population and the social environment from which patients come and about those factors, other than individual care, which make a difference. Once this population-perspective is internalized within the professors of medicine (and perhaps other clinical professors) who are usually the strongest individuals politically and intellectually in medical schools, with lots of variation of course, the medical students and younger faculty members will adopt it. Professors of clinical medicine tend to be the centers of power in most medical schools. You then have a base for developing concerns that have been lacking as a result of the creation of separate schools of public health or even departments of preventive, social or community medicine. This separation has really allowed medical schools to abrogate most of their responsibilities for the organization of health services, for the running of hospitals and other health care institutions, for concern about people who don't seek care but who may need it, or for the equitable distribution of resources. The medical schools have largely ignored these responsibilities and argued that there was "a school across the way" that dealt with these matters. "We've got a school or a department that's in charge of all that;

the important things are these molecular and cellular events," they would say. Well, these aspects of medicine are important, there is no question about it, but they are not the only things that are important in medicine.

Some years ago one of our students, a graduate, Stan Aronovitch, and one of our faculty members too, who hadn't been with us very long (I forget his name) were offered jobs by a prepaid health plan in California called HMO International -- this was in the early days of HMOs, the Health Maintenance Organizations. These two colleagues went to work for this new organization. Shortly afterwards one of them asked me to have a look at what they were doing. This HMO was essentially an investor-owned health care plan; most of the ownership being vested in a small partnership of three physicians--one of them was an osteopath and the other two were, I think, a general practitioner, and a surgeon. It was structured in a rather interesting way with respect to the financial arrangements. HMO International had constructed about twenty-five clinics in the Los Angeles area. They had worked out modules for the sizing of the facilities and for the staffing patterns. The organization was using community hospitals for secondary care basically, and using university hospitals such as at UCLA, and other specialty hospitals, for tertiary care. The had enrollees which at that time were about half Medicaid and Medicare recipients. The others were enrolled through union contracts and several management contracts. There were about 120,000 enrollees altogether in 1973. After my visit I was asked to go on the board of this corporation. I thought I should have a buddy join me so I consulted various people. I had met Nathan Stark when he and I were on the AMA Undergraduate Medical Education Committee for a number of years, and we had talked about hospitals and health care frequently. He, of course, had been largely responsible for reorganizing

Crown Center, for developing the Truman Medical Center in Kansas City and for helping to start the new medical school there headed by Grey Dimond. Nathan had been vice president at Hallmark Cards; he then became Vice Chancellor for Health Affairs at the University of Pittsburgh and more recently became Deputy Secretary for Health & Welfare. (This will be an interesting assignment for him.) At any rate, we went on the board of HMO International together. We didn't go on until I had had an independent survey done of the quality of care by Bob Huntley, Peter Lee and a professor of medicine at the University of California, S.F. I forget his name now. They and I looked at the clinical records of patients and did surveys of the staff and patients and their records both in the clinics and in hospitals. The organization had quite good operating statistics. I inspected the clinics and hospitals and was reassured that this was a reasonable proposition to get involved in. So, we went on the board; we really had a series of most enlightening experiences. The whole health care system was geared to providing a full range of services and even had an insurance company that was underwriting much of the risk in a rather unusual way within the HMO plan itself. We developed some novel ideas with respect to negotiating for hospital beds by estimating our needs using epidemiological methods and getting them wholesale rather than retail. We could do this by predicting the number of beds that would be used by the population served and could lease them from hospitals. Thus the hospitals knew what income they were getting, and we didn't have to run the hospitals. There were a number of other interesting developments that were possible in that kind of organizational setting. Unfortunately, we soon uncovered a number of "shady practices," if not outright chicanery, on the part of some of the people who were leading the corporation. Stark and I were able to deal

with a fair number of these. We put the president on an annual contract and we eliminated the staff's credit cards. It had been run a little like a "Ma" and "Pa" store. It wasn't as far out as some of the other notorious California HMOs that were in vogue at that time and did not involve the "ripoffs" that were going on in many settings, but many of HMO International's practices were clearly unacceptable corporate practices.

The Kennedy Senate Subcommittee sent a group out to look at the California HMOs and were horrified by what they found. They were critical of the whole concept and asked some tough questions. I think they had a rather biased view; ideologically a medical care organization that was investor-owned or a so-called "for profit" entity would automatically be regarded as of doubtful public benefit. But subsequent studies done by the Social Security Administration and even by the California State Department of Health showed that the quality and performance of services by HMO International were quite satisfactory. In fact, HMO International compared favorably with other nonprofit prepaid health care plans including Kaiser Permanente. Studies of the quality of care were done unbeknownst to me, not only by people in my department such as John Williamson, but also by people at UCLA. The studies had shown the services to be clinically satisfactory in all respects. I was impressed also by the fact that the receptionists and clerks would talk to people, would smile, and were courteous and kind to them, an entirely different reception from the sort you usually get in some rather distinguished teaching hospitals. There are some I am familiar with where the clerks and receptionists are anything but cheerful, smiling and kindly. HMO International was so structured that there were no incentives for any of the physicians to overprescribe or to overutilize diagnostic test, x-rays or

hospitals because they had no way of benefiting personally from such behavior. Each physician could only flourish if the whole organization flourished. There was even a system of providing stock options if the whole enterprise flourished by satisfying the enrollees, maintaining high standards of care and using resources prudently.

To make a long story short, a proxy battle developed with dissidents who thought HMO International was not being properly run and that the president was behaving, I guess one could say, dishonestly. Nathan Stark and I had some inklings of improper behavior and had taken steps to control abuses when the proxy battle broke out. These problems had Stark and me going back and forth to California every ten days. Finally, I said, "I don't have any more time for this." He said the same, so we resigned from the board. It was a most interesting experience. Long legal battles ensued. Finally, when the smoke cleared, a capable young lawyer, who had been General Counsel for the company and one of the most stable persons in the management, emerged as president. Several other senior officers are out of the company entirely and the organization was purchased by INA Insurance Company; and more recently by Hospital Corporation of America. They have changed the name and are buying up other HMO's in other parts of the country. The company expects to expand nationally very soon.

The thing that interested me--and I've had talks with the president of the company and with others from Hospital Affiliates Inc., which is the INA subsidiary that first purchased HMO International--is that the organization really is cost-effective. They sharpen their pencils carefully and everything is considered in relationship to cost and quality, but it is also considered carefully in relationships to marketing satisfactory services of high quality.

Manpower, resources and equipment are all scaled to the distribution of medical problems in the population served. Common problems are common and rare problems are rare! Even before the recent takeovers, HMO International required two opinions before a patient was admitted to a hospital; not only was a full workup by the family physician required, but also a second opinion by a consultant. I think the system has a great deal to commend it. Certainly it equals anything I've seen in most neighborhood health centers or in hospital outpatient clinics and it has the great advantage of not being hospital-based.

I think one of the limitations of the Kaiser Permanente system is that it is hospital-based, whereas HMO International is community-based and hospital-related. It is in the community but is tied to a hospital at the secondary care level. Many criticisms have been voiced by people to the effect that HMO International used community hospitals for most patients requiring hospitalization. In the view of many academics the only "right" hospital is the university hospital, if you really want high quality care. This overlooks the fact that there are probably 5000 community hospitals around the country. They vary in quality; (if you see one you don't see them all) most are perfectly adequate for secondary care. HMO International used UCLA and other teaching hospitals for tertiary or sub-specialty care. This system is interesting in that it is emerging from the people and their problems in the community up through the whole hierarchical hospital structure.

It is the prototype, I think, for what I call vertically integrated health care systems, which I would argue are preferable to the un-integrated horizontal cartels that constitute the bulk of the current U.S. "non-system." We seem to have a nursing cartel, a medical cartel, a hospital cartel and a

nursing home cartel. They are virtually all horizontal and independent; rarely do you get integrated systems organized in relationship to the distribution of medical problems in the population served. I like to use and have used since about 1966 or 67, the air line model. My own view is that we should organize the health services of this country in the same way as we have organized our air lines. We should have rigid standards for performance, safety and quality, under the aegis of an entity analogous to the FAA, but we should allocate the markets through an entity analogous to the CAB. I would encourage the aggregation of integrated and balanced systems through loans or other kinds of tax and financial incentives. We should stimulate the creation of systems to deal with large numbers of enrollees. I think that a cost-effective balanced health care system requires a minimum of about a million enrollees in order to generate an adequate distribution of common and rare problems so that the whole system can be scaled rationally. You can then let the demand and not the supply side of the system guide the allocation of resources in relationship to the perceived market and the distribution of problems. You would only put one CAT scanner for a system of a million people, you wouldn't have four or five lying around! You would only have one open heart surgery team for the entire group. The airline model provides opportunities to have competition and choice among health care systems that are not confined by geographic lines. Obviously, there has to be some relationship between primary, secondary and tertiary levels of care and travel distances. The important point is that we don't have to regionalize by geography, which is the present way we're going with the Health Systems Agencies or HSAs. You can regionalize by health care systems, so you can have competitive health care systems that initially are local or "regional" in the

air line sense. You might eventually have half a dozen national systems. You would start with local and regional systems and then gradually aggregate them by mergers as we've done with the air lines, but you wouldn't foreclose your options. If eventually you want to have one national health service just like Britain has, you could do that. Even with the post office we have found it pretty useful to have United Parcel Service as a competitor or Federal Express to deliver air packages, and so forth. Regionalization is essentially a functional concept not a geographic concept. Problems of balance and scale are more important that those of geo-political jurisdiction and distance. I would wonder what a single national health service would look like in the United States if it were all run from headquarters in Washington or even from each state capital! It is a horrendous thought.

I think the market place has something to be said for it, and constructive competition can be a useful and creative force. This approach to the organization of any health services could create a uniquely American system; it has not really been tried on a large scale, although, of course, I think Kaiser Permanente is too hospital-based; it is too hospital-related. What we need is broader-based community and decentralized primary care centers closer to where people live so that travel time is reduced.

Now, I realize that in horizontal cities like Los Angeles, people often work and live in different areas and there are transportation problems that influence their choice of sources of care. Not all aspects of life and essential services come together easily; but I don't think they come together very easily with the present arrangements either. One alternative is to encourage these vertically integrated systems. They could be started by insurance companies, nonprofit voluntary hospitals, municipal organizations or

unions, for example. A number of corporations, such as Reynolds Tobacco, are starting what they are calling corporate HMOs. However, although the financing is prepaid and may cover all medical risks, the facility is restricted to an ambulatory clinic and it is not related to the community hospital and tertiary care levels of the systems, as I understand these entities. On the other hand, there are innovative ways of starting HMOs with the sharing of both clinical and financial risks. There are prototypes in Minneapolis and in Washington State where the risk for primary care is assumed by a general physician. He can then share additional risks for secondary hospitalization and for tertiary care to the extent that he is prepared to assume both the clinical and financial risks involved. There are, therefore, a variety of ways of putting the essential components together. I think the new breed of young physicians is prepared to work in new kinds of environments and organizations. They don't find it distasteful to have a guaranteed annual wage with suitable fringe benefits. Why the traditional medical profession should be so obsessed with piecework is not clear to me but you can't help thinking that there is a pecuniary motive involved. Perhaps all the talk about free enterprise and the sanctity of the entrepreneur is misplaced. The term in not restricted to an individual who practices on his own; you can have an entrepreneurial organization or even an organization started by an entrepreneur, that could provide a full range of clinical services. I think this INA or Hospital Corporation of American development is going to be interesting, particularly if they really go national, or even international as I believe they plan to do.

Perhaps we should discuss national health insurance now. I've never really taken much interest in national health insurance. I remember having

discussions with Ig Falk about this. We had a Sun Valley forum once on the subject and he and I have talked about it at other times. He was, of course, from the days of the Committee on the Cost of Medical Care, concerned with the costs of care and their payment through insurance. He has been a major participant in the group concerned with national health insurance. Starting with the Committee on the Cost of Medical Care it seemed to me they went off on the wrong track. What they did was to get a bunch of professors from medical schools to define the ideal standards for treating particular diseases. Elaborate specifications of tests and interventions measures were established. Most of which at that time, and this is about forty years ago, had absolutely no scientific evidence that they were more useful than useless, or even harmful. No one ever blew the whistle on these fellows and pointed out that the emperors had no clothes. On what basis were they making these recommendatons? They were talking pretty well off the tops of their heads, as far as I can make out. Then the costs of their disease-specific standards were extrapolated by multiplying them by the number of people estimated to be suffering from the diseases. Many of the estimates were hospital-based, not community-based. Of course it's going to cost an enormous sum if you have professors who see a very small, even minute, one-in-a-thousand proportion of the people's medical problems. The notion that payment for quality care should be determined by tertiary care experience seems to have had its origins in that distinguished committee; I think it was somewhat misguided. If they had started at the other end of the spectrum where people present their problems initially, at the level of primary care, and followed them through the secondary and tertiary levels of the system they might have built up a "market" or epidemiological model of the demand for different types and

complexities of care. After all, there is a difference between a water tap in the kitchen and the Hoover Dam -- you don't need Hoover Dams in every kitchen! You've got to scale things in relationship to the nature of the problem, its difficulty or complexity and the specific service required. I felt that a better appreciation of the nature of medicine's task was needed. As long as there was a preoccupation with costs and with money and a focus on the end stages of diseases, rather than with primary care or ambulatory problems, we could never really tackle the issue of national health insurance. The best ways to reduce the costs of medical care, as far as I am concerned, are to keep people out of hospitals, keep them away from doctors, have early management of problems, self care, and home care; educate people better about their bodies; and provide adequate numbers of primary care physicians. I think the family physician is undergoing an amazing renaissance. The contemporary departments use a new approach to behavioral medicine. It involves earlier management of problems, the use of simpler forms of intervention, and it requires a good deal of wisdom and an empathetic, as well as scientific approach to care. The emphasis should be on keeping people away from hospitals; hospitals should only be used as backup and when really needed. We have 25% of our hospital beds in this country in university intensive care types of beds. The best figures I can get are from British Columbia. If you look at what the people there say, (and the analysis has been done for that province's population of 2 million), one to two hundred beds per million people will take care of all the tertiary care. So, we're just off the wall with the contemporary epidemic of intensive tertiary care, university hospital beds in this country and, of course, places like New York City are loaded with these extremely expensive facilities.

Now, if we're going to start paying for all this from public, or even private funds, we will have a fool proof recipe for bankruptcy, no matter how it's done. Until we get it straight in our minds as to what the nature of the task is, we're unlikely to get at the roots of the cost, quality or equity issues. It needs to be clearly understood that there are twenty-four common problems which account for half the initial visits to physicians in this country, that the common problems are common, and the rare problems are rare, and that we must understand the central role of psychological and social factors in the genesis of ill health.

Another person who had an indirect influence on me (there are two biographies of him) was Sir James MacKenzie. He is known to cardiologists, but not terribly well known in this country. The first biography is called The Beloved Physician and the second is a more recent book by Alex Mair entitled Sir James MacKenzie, General Practitioner, 1925-1953. MacKenzie was an indifferent student at Edinburgh and went into general practice in Burnley in Lancashire. It so happens that the father of a colleague of mine at Chapel Hill, Tom Gibson, who moved to Vermont with me and who is still there, practiced in the same house as MacKenzie and hence knew a lot about him. MacKenzie practiced in the early years of this century following his patients with meticulous attention and keeping careful notes on them. He was responsible for revolutionizing cardiology between about 1905 and 1920 roughly. He showed the benign nature of many heart murmurs and irregularities of the pulse. He resurrected people who had been confined to their sick beds by other practitioners, and did it all before the electrocardiography was developed. He used a polygraph for measuring venous and arterial pulses that he developed himself. His initial publications were not accepted at first in

Britain. But, to make a long story short, he eventually wrote a number of books, at least a half dozen or more, and introduced modern cardiology. He was invited to London, was knighted and became the principal consultant on heart diseases in Britain and even Europe.

One story recounts how an elderly patient was being presented to him in London by a house officer. The house officer said, "Yes, Sir James, you were once this lady's GP," whereupon MacKenzie is reported to have drawn himself up to his full height of six feet or more and said, "Young man, I want you to get this straight. I had the honor to be this woman's family physician for twenty years when she was younger!"

He also wrote books on the future of medical education. He retired to the University of St. Andrews in Scotland and established an institute for studying the natural history of symptoms and disease on the grounds that, until we understand how people experience their initial symptoms and signs at the earliest stages of ill health, we can never really understand the largest part of the ill health, we can never really understand the largest part of the iceburg of concern, anxiety, and pathology, or the preventable factors that initiate disease processes. MacKenzie believed that this knowledge could only be obtained through research in family medicine and the efforts of general practitioners. These early sensations and events precede the later manifestations experienced as the end stages of disease seen in hospitals.

I remember one year I suggested to the U. S. National Committee on Vital and Health Statistics that we stop counting the dead for a year and focus on some of the earlier expressions of ill health. The suggestion was not well received. MacKenzie also introduced the term "the new epidemiology" and discussed the application of epidemiological principles and methods to the

problems of general medicine or what we now refer to as primary medical care. There is some interesting correspondence that Alex Mair has in his biography of MacKenzie. He also includes an account of MacKenzie's visit to the United States in 1919. There are a couple of pages which I used to pass out to the Hopkins students and others, in which MacKenzie says: "From Cincinnati we went to Baltimore and were shown over the Johns Hopkins School. It was reckoned to be in the very forefront of medical schools, having enormous endowments, so that medicine is broken up into a great number of specialties and the students have to learn an enormous number of different methods, but in conversation with the authorities, I never was more surprised to find such a stupid outlook as they possessed. I could say with confidence, that we were far better taught in Edinburgh in our student days than the men are today in such places. But what struck me above all, was their absolute conceit and complacence, and when we discussed certain phases of medicine in which they pretended to being most up-to-date, I found them extraordinarily superficial. So far as my own work is concerned, they had not even realized the elementary principles necessary to guide them in understanding the meaning of the symptoms which their numerous methods revealed."

Here they were at Hopkins with this new brand of medicine in which they had created all of these subspecialties and superspecialties, in attempts to understand all the aspects of disease processes in enormous detail, but ignoring the earliest manifestations of ill health, the natural history of disease, and the prognostic importance of early symptoms for understanding the management of the patient's problems. MacKenzie predicted that this paradigm would be inadequate and that one day it would have to be changed.

The Royal College of General Practitioners memorializes him in the annual

James MacKenzie Lecture. The RCGP is the one professional organization that has stimulated the study of family medicine and primary care; it is now the leading society in WONCA, the global organization concerned with family medicine which I mentioned earlier. There is now emerging in this and other groups concerned with family medicine a cadre of people doing epidemiological research and health services research in primary care. They are also investigating the decision-making thought processes used by doctors. We don't know much about these processes. How do doctors make their decisions? How do they make patient management choices? What kinds of information do they use? They are beginning to investigate these phenomena now and analyze them in quite sophisticated studies. What kind of hypotheses do physicians develop? How do they go about gathering information to test a clinical hypothesis? How do they label patient's problems? How do physicians decide how to manage their patient's problems? How do they communicate with their patients? Do they really understand their patient's problems? As this new field of enquiry emerges, I think we will develop a new and different view of the task of medicine. In turn this will lead to different ways of paying for medical services. I think we'll have to move to prepaid schemes in which there is global budgeting for the care of defined populations and the assumption of clinical risks, financial risks and managerial risks by health care enterprises, or organizations that provide a full range of services, either directly, or indirectly in a responsible way, by contract or other kinds of formal arrangements. I think until these fundamental ideas about health and disease are better understood we're just in for more trouble with respect to costs, equity and quality.

Talking about the financing again, Nathan Stark and I were interested in

the way HMO International was financed through various leasing, equity and debt arrangements. They were all a part of the fiscal wizardry practiced by the management of that enterprise; it was quite dazzling in some of its manifestations but nevertheless interesting. A couple of years ago we put together a conference on capital financing at the University of Pittsburgh; a book edited by Gordon MacLeod and Mark Perlman included the papers and discussion. Among the people we had there were a number concerned with capital financing of hospitals. It included people from Ziegler who does a lot of mortgage financing as you know, and others from Kidder Peabody, Nuveen and Merrill Lynch. One of the things that came out of the conference was the huge proportion of capital financing coming from tax-exempt revenue bonds. By the middle of the 1980's it looks as if three-quarters or more of the capital financing of many, if not most, of the hospitals in this country is going to be on that basis.

When I realized the implications of this development I asked, "Who buys these bonds?" They are customarily sold in a deal where a community hospital of a couple of hundred beds in a town of 100,000 is expanding its capacity. The local banker, the hardware merchant, the lumber merchant, and the department store owner are each asked for a contribution of, for example, $10,000 in cash and in addition, each is supposed to buy $50,000 or $100,000 of these tax-exempt bonds on the side. Everybody wins! You take the $10,000 off your taxes as a charitable deduction and you get tax-exempt income from the bonds. Well, just think what effective cost containment policies are going to do for that kind of financing! When the hospital's cash flow stops, and the debt service on these bonds is cut off, there's going to be screamingand yelling from Ma & Pa who bought the bonds that's going to be

something to behold. I think that we may have a whole series of bankruptcies from moderate sized hospitals in the next few years. What's going to happen then? Well, the problems are really going to concentrate the attention of the Boards of Trustees. They're either going to have to close the hospitals or run them under different auspices and with new priorities. There are going to be exactly the same people on the boards of hospitals but they're going to have a lot more constraints put on them. This is going to make people think; the hospitals of this country are going to go through some kind of a wringer just as they did in Britain after World War II. The alternatives are a national health service or some kind of rational competition and choice among balanced integrated health care systems.

There are limited sources of capital as I understand it. There's "philanthropy"--you take a gift off your taxes. Then there is "social capital" which comes from government loans or grants, again, that is paid from taxes. There is also "equity capital" in which the capital constitutes a share in the organization. Finally, there are loans which are "debt capital" from people who lend money in return for mortgages, or bonds. There are only these major sources of capital, as I understand it, and they all have to be accounted for in some way; eventually it is the people who pay in one way or another and they need to understand more about our nation's health care system.

In our 1972 PSAC (President's Science Advisory Committee) Report we recommended that a National Commission on Health Services be created. I pushed this in various other circles such as the Institute of Medicine, but all to no avail. We've had commissions on health facilities, on hospital efficiency, on group practice, on financing, on manpower, and a whole series of things, but we never really have had one on health services themselves.

If you're going to talk about health facilities you have to, it seems to me, get straight to a discussion of what the services are to be, what and whose problems they are for. We need a clear idea of the real burden of illness in the community, and what these problems are called both by the people themselves and by those to whom they are brought initially. Until we start this sort of thinking, I just can't see our chronic medical care crisis getting any better - it is going to get worse. If we have catastrophic health insurance it may provide a temporary respite, and, perhaps, help some people, but it will also certainly encourage the use of expensive inpatient tertiary care and the expansion of diagnostic virtuosity at the expense of early preventive services. It will probably perpetrate the cruel business of keeping people with terminal illnesses alive in settings in which neither they nor their relatives want to sustain life further. It seems to me that comforting and caring services where people can be cared for in their own homes and die in peace and tranquility with their families are much to be preferred. I think we've got a medical-industrial system that's out of control now. The new "holistic medicine" approach is a reaction to the current excesses. There is some quackery in it -- but there seems to be demonstrable benefit from biofeedback. There is much to learn from the Eastern views about health and disease and there is much to be said for the benefits of meditation; we need to know more about them. I wouldn't knock transcendental meditation; it may be another form of prayer; of being quiet, relaxing or of comtemplating the nature of man and the universe. There are many aspects of health and disease younger people are looking into now that have great promise for the future. We are learning that the placebo effect, and perhaps acupuncture, may be mediated through the beta endorphins and that

immunologist mechanisms, as well as biochemical and physiological pathways are involved. If these beneficial outcomes can be evoked by individuals themselves through simple relaxation measures or for example, by watching video tapes, or through the ministrations of nurse's aides trying to help people, we may discover powerful new therapeutic measures. Certainly if such simple measures can be used for management of the psychobiological and physiological stresses or responses to distress that are associated with the twenty-four common problems seen in primary care, we have a way to reduce the soaring costs of contemporary orthodox medical care, to say nothing of the humanitarian and comforting aspects of such approaches.

So, I am not persuaded that cost containment proposals as articulated at present by the politicians or the health care establishment constitute a rational approach. At the very least we need different information to help us understand the true burden of suffering and what can be done to ameliorate it.

Health education as practiced by "health educators" has not been a conspicuous success. Certainly there is a lot we don't understand about human motivation, attitudes and behavior, about the perversity of people who indulge in practices that promote their own self-destruction, and about the effects of our personal interactions with other people. There is a great deal to do here and many people are engaged in health education efforts. However, I don't really know of many real successes to tell you the truth.

Certainly, family physicians should be concerned about health education. I think the so-called "encounter effect" which the pediatricians have identified is a key to the constructive motivation of patients. It arises when an individual seeks help from a physician for one purpose and the occasion is turned into an opportunity for providing other kinds of

information, guidance or preventive measures. The patient is emotionally moved to consult a physician with some question--maybe just for a cold, or ache or even an immunization, it could be for an annual examination (although the evidence that they make much difference is pretty slim) and is, therefore, receptive to other suggestions or "education." Whatever the reason for the encounter, the physician can take the opportunity to discuss other matters with the patient and use the motivation that's brought him to the office or clinic to broaden the patient's understanding of the factors that promote health. On the other hand, there is probably a hard core of 10 or 20% of people who never go near a physician, but who seem to thrive and look after themselves. To some extent we've kind of medicalized the whole population as Illich suggests and made them overly dependent. If anything upsets them they manage to see a doctor right away. It's not clear that's going to be productive. We know such behavior is going to be expensive under the present arrangments and you may or may not be helped by them.

Some people say "Aren't you discouraged with the present state of affairs?" I guess the answer is yes and no. When one considers that general practice and family medicine were declared dead about fifteen years ago, one has to be encouraged that now there is a flourishing renaissance in family medicine and primary care. When one considers now that epidemiology has a much broader range of applications than its concerns with communicable diseases, and when one observes younger physicians working together in HMOs, one has to conclude that progress has been made.

I was talking to Charlie Edwards a couple of years ago--I guess he'd just finished his term as Assistant Secretary of Health and he'd been talking to his counterparts George Godber of Britain and to Venidictov of the U.S.S.R.

They were comparing the "Forward Plans" for their respective countries for the next years. They found that all three were almost identical. Each country was concerned with problems of equity, distribution, and costs, each was concerned with quality, and with research, and how to balance all three components. It occurred to me, that if you compare the United Kingdom, the U.S.S.R. and the United States since World War II, each opted for different priorities. The Russians opted for distribution - they trained feldschers, emphasized distribution of services in the rural rayons and urban polyclinics and getting care out to people. The British really went for quality in the sense of training consultants. Their consultants are trained to work in a higher level than our specialists, and they put them in the district hospitals throughout the country. The British didn't do much for general practice initially but they did at least keep a floor under the quality of care in the provincial and district hospitals by strengthening the consultant services. The United States opted for biomedical research and our emphasis was on support for NIH and laboratory research. We developed superb tertiary superspecialty care but we did it at the expense of primary and secondary care and rational distribution. All three countries are now concerned with similar problems: How do you distribute services rationally? How do you allocate resources in relation to priorities? How do you organize balanced health care systems that incorporate biomedical (and I would add behavioral) research, which we certainly need, with concerns for maintaining and improving quality, and the effective distribution of primary care so that it is available to all? This is one of the lessons that has emerged in these thirty years. Biomedical research alone is not enough, any more than primary care alone is enough. A balanced mix of all the essential components is required: Primary,

secondary and tertiary care, as well as biomedical, behavioral and health services research at all three levels.

It's this lesson of balance that came out of our international study done in collaboration with the World Health Organization. This involved twelve areas in seven countries. We took considerable pains to collect the data in a uniform way so that it was comparable. The collection of data was done independently so the results were unknown until the data were analyzed. There was no way you could predict the findings. Our measures of "needs," "resources" and "uses," produced balanced configurations between these three elements in some areas and striking imbalances in others. The study areas where there had been policies of rational planning and regionalization over the years, for example, in Saskatchewan, Canada, Liverpool, England and to some extent in Helsinki, Finland, showed considerable congruence among these measures which, although done with care were nevertheless somewhat gross and aggregate. So balance is the key; resources need to be matched to needs if use is to be optimized and costs minimized. I don't think we're going to get it by tinkering with a supply side which is based in the medical schools and universities. They are just going to keep turning out more of the same people to do more of the same. Maybe the pressure from Congress and the allocation of funds for training family physicians in primary care will make a difference but I believe we need to emphasize the demand side by organizing balanced health care systems that will determine how many physicians of what types are needed.

I once asked a senior professor at Hopkins: "Do you think there is any problem with the distribution of specialists?"

He said there were terrible problems; "There is gross maldistribution,

both by types of specialists and by geography."

I asked, "Well, what should Hopkins do about it?"

He said we shouldn't do anything about it because there's no maldistribution of our graduates. There's always a need for them.

This is the prevailing medical school attitude on the supply side. I think the real place to exercise leverage is on the demand side. Kaiser Permanente says: "Look we don't need any more cardiac surgeons for our three million enrollees and we don't need any more thoracic surgeons. What we need are primary care physicians, and generalists, and a few supporting general surgeons." So, if you have an organized, integrated, balanced system then it will start buying equipment and hiring people in relationship to the market it serves and the budget generated by the premiums it charges.

We pay piece work rates now for passing fiberoptic instruments of one kind or another. We pay for gadgetry; we pay for elaborate instruments with brushed aluminum dials and LED digital displays, and so on. But we don't pay for listening and counselling the patient, or for the supporting and caring services. These may be, in the long run, much more cost-effective than many diagnostic tests of doubtful efficacy. We don't even pay physicians for assuming risks. The epidemic of malpractice suits is really a reflection of the breakdown of the patient-physician relationship. Dichter, a marketing specialist, thirty years ago, did a study for the California Medical Association. Only a brief summary report was published; the full version was never released. The substance of the report, as I recall it, was that where the relationship between the patient and the physician was continuing, long-term, caring, supportive and personal, the risk of malpractice was very low. Where the relationship was transient, episodic, technical and

specialized, the risk was high. An incompetent generalist might be doing horrendous things, but, if the patient perceived him as caring, the risk of malpractice was low. On the other hand, a highly competent, superspecialist who was perceived as providing transient, episodic and technical care had a very high risk of being sued. Today the situation is infinitely worse and I conclude that we are experiencing a breakdown in the relationship and trust of the public in the health professional generally. They tend to say "I love my doctor but I hate doctors in general and hate the whole apparatus." The epidemic of malpractice suits must be saying something to us. I think until we get out health care system structured so that it provides both continuity and caring under the umbrella of an efficiently managed organization or enterprise, which relates our resources and services to people's need, uses only those forms of intervention that are demonstrably efficacious and cost-effective, recognizes the therapeutic power of the physician to heal through the placebo and Hawthorne effect, and combines comparison with credible scientific knowledge, matters are likely to deteriorate further. But I am optimistic about the future; these ideas have been around a long time and will prevail in due course.

Interview, Ann Arbor

December 11, 1979

INDEX