**Interview with Charles Flagle**
**Conducted by Edward Berkowitz**
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**Berkowitz:** I notice that you came to the field of health services research from engineering, which is somewhat unusual, I think. Could you talk a little bit about how that happened?

**Flagle:** Actually it was a very special kind of engineering. I was on the staff of the Army Operations Research Office, which was administered by Johns Hopkins University. We were in the middle of the study of the history of the battle of Falaise Gap from which von Runstadt was allowed to escape after the Allied invasion of Europe. We were trying to track down, from a study of records, where the communications failed, what logistical failures took place that prolonged World War II. I had just finished a doctorate in operations research and industrial engineering, and my thesis was on the subject of random demand as a factor in organizational design. In a meeting of an assortment of deans, presidents and whatnot of The Johns Hopkins University in which the president at that time, Lowell Reed, was trying to integrate the disparate activities of the university and find out what common interests there were that might bring certain segments of Johns Hopkins together, the director of the hospital observed that much of what we were doing in Operations Research had to do with logistics, communications, the study of

operations, usually of mass movements of people, and the hospital had many problems of that sort. The clinics operations, problems in surgery, anesthesiology--there was a constant flow of people, either as patients or as doctors, and there would be thousands of individual transactions going on in the hospital simultaneously. They felt they didn't understand much of that, so the director of the hospital, Russell Nelson, proposed to the director of the Operations Research Office, Ellis Johnson, and to the dean of engineering who was at that time my academic boss, that there should be an operations research activity in the hospital. Since I lived in Baltimore and was teaching at the university, I gave up my affiliation--I shouldn't say gave them up immediately, I continued them for some years after--going to Europe to study the readiness for combat, things that worried people at the time, all part of operations research. They set up a group in the hospital to study the problems that were plaguing the institution at the time, and most of them could be described simply as problems of shortage. The shortage of nurses--that was the most acute problem. And judging by the congestion of the dozens, even a hundred or so people seen sitting in the clinics waiting for service at any one time, there was a problem of either shortage of resources or disorganization of resources. So I agreed to come here and was made Director of Operations Research, reporting to the director of the hospital, and immediately began studies of

the nursing shortage.

**Berkowitz:** This was what year?

**Flagle:** 1956. I had just received my doctorate in 1955. The nursing department had just set up an in-house educational program to permit them to continue giving service by delegating downwards some responsibilities of professional nurses to nursing aides who would be trained within the hospital. That led to problems of rapid turnover which, in a sense, exacerbated the original problem. My first students who came to the hospital worked on that problem, the problem of nurse staffing. I don't know whether I should go into great detail on that activity, but it was the thing that almost immediately attracted some national attention. My boss in the hospital, Dr. Nelson, became the president of the American Hospital Association, and he introduced me not only to most of the government people who were concerned with the implementation of the Hill-Burton Act of 1946, which was the post-war effort to rebuild and renovate hospitals. As a matter of fact, that legislation supported most of the research. It was amended in the early '50s and expanded greatly in areas of research. Much of that research was directed at the construction of new hospitals and the operation of hospitals. The saying was that hospitals were old before they were built, because not enough study goes into how they should be built. So within the Public Health Service, administering the Hill-Burton Act, there

was a division called Hospital Medical Facilities. I was introduced to the Assistant Surgeon-General who ran that service and was immediately invited to be a consultant to them on a particular project called Progressive Patient Care. The reason that I decided to do that was that "progressive patient care" implies that the patient population was studied and classified according to the level of its need, and the concept of intensive care units in the hospital, the talk of self-care units or continuing-care units was a way that the Public Health Service saw that the shortage of resources, in terms of both nurses and hospital beds, could be solved. It was another version of the solution we had arrived at here where patients, instead of being moved from intensive care to routine and to self-care, would stay where they were, and we would simply organize the nursing resources primarily to match the level of needs of patients classified as intensive care. It was something that had been done more or less intuitively, of course, forever, from the Osler Clinic in which we were doing our studies, but somehow the process had never been formalized to the point that it was recognized by administration so that administration could take care of allocation of resources at a central level. At any rate, thanks to Russell Nelson, I was catapulted into affairs at the national level in the government and also in the American Hospital Association, where I was a member of the Association's

Council on Education and Research. From that grew the first journal called *Health Services Research*, and I was on the first editorial board of that.

**Berkowitz:** When did that start?

**Flagle:** It wasn't immediately. I don't think that occurred until the '60s, the middle '60s. But a lot happened in a very short period of time.

**Berkowitz:** Let me ask you how it is you got to that position, to be asked to work in operations research in the hospital? I see that your education was all at Johns Hopkins. Are you from Baltimore?

**Flagle:** I was born in the little town of Scottdale, Pennsylvania, but my mother's family is in Baltimore, and we migrated back to Baltimore when I was in the early grades in school.

**Berkowitz:** And where's Scottdale, Pennsylvania?

**Flagle:** Just south of Pittsburgh. After I got my degree, I migrated right back to that area, to the Westinghouse Company, which is in Pittsburgh.

**Berkowitz:** After you got your Master's degree?

**Flagle:** I was gone for years. It was straightforward, honest engineering--design, development--I got patents on jet engines. Somewhere along the line, I became pretty much concerned about the fact that the applications of new technology--nuclear

technology, electronics and even some of the more sophisticated techniques in mechanical engineering--were developing products that the users really didn't understand and were not able to adapt into their standard decision processes. So after I graduated with a bachelor's degree in engineering in 1940, I went to work six days later with the Westinghouse Company in Philadelphia. What was being designed at that point was a turbo generator for power plants and for marine propulsion. I was hardly there--I think I began to work there on the 10th--and on that very day Italy entered the war on the side of Germany. Immediately mobilization began, and I had happened to be trained almost directly to take part in the rearmament of the country as it began, with the design and construction of marine power plants for shipping. I had all sorts of adventures. I went down the Delaware Bay on tankers at the time the submarines were out there. We had to shake down these boats and make sure they were ready to carry oil overseas, and we'd go back into Chester where they were built only to find something towed in with a hole in the hull the size of two floors of a building--very dramatic days. I was taken from that job--I really had no choice in the matter--to work with a secret group on the development of new forms of aircraft propulsion. And this was jet propulsion. My first paper, which was around '46, was on the economics of jet propulsion.

**Berkowitz:** The *economics* of jet propulsion? Cost efficiency?

**Flagle:** Yes. I remember--I couldn't tell you exactly the moment, but it was sometime during the war--I actually had a folder in which I was listed as an ensign in the Navy, and it was stopped because of the work that I was doing. Because we did build, really, the first of what was called axial-flow jet engine in the Allied world. We knew what the Germans were doing, and they had very cumbersome machines and were even more stupid in the use of them most of the decision-makers I was aware of on our side in the military. Hitler ordered that the jet engine be used for bombing. The poor things had a 30-minute range and were useless in that respect. So it was essential for us to know what the capabilities and limitations were of the technology. The economics at that time really had to do with accomplishing a mission rather than saving dollars, but the kind of calculations you do are really pretty much the same. You're still expending resources and you want to do it in the most effective way possible.

As you know, one thing leads to another. The paper that I wrote with a colleague was picked up by the investment field. Reynolds and Company, one of the large investment bankers at the time, saw that paper and decided to republish it in the context of where aviation was going. I somehow came into the purview of the investment banking industry and was offered a job in

Connecticut, which is a small wire house which no longer exists through the various mergers that took place.

One year in that business was enough for me to see that that's not what I wanted to do for the rest of my life, and another emergency arose. My father had a business in Baltimore by that time representing a number of companies that manufactured equipment for schools, libraries, and laboratories, so I left and came back to Baltimore helping, at the time, to rebuild the business. It turned out to be quite lucrative, so I was free enough to then go back to school and pick up where I left off almost 10 years before. I was then invited to join the research activities of the university, something called the Radiation Laboratories, which was what the university had been doing during the war. By that time, the scope of the university had changed. The university was relied upon by the government to do research, much of it of a military nature. It still goes on.

**Berkowitz:** Which wasn't true when you were an undergraduate.

**Flagle:** No. An example of how it had changed: the university was a nine-month operation, and the summers were pretty much free. Faculty were paid for the nine months. But with the university becoming a major contractor for research, faculty were offered the opportunity to work on research projects for which they were then hired year round. It's a topic worth studying, because it did change the nature of the university, because the

faculty worked year round on those projects and combined teaching and research. It also funded the students who were doing graduate work. Certainly the research university became a different kind of creature than it had been.

**Berkowitz:** Yes, at least in those technical fields. English professors probably still went to the Adirondacks for the summer.

**Flagle:** Yes. Well, it was nice to marry a rich woman. Your rich wife had a cottage in the Adirondacks or, better yet, Maine or Cape Cod. There was some academic snobbery because of that, even in my undergraduate days, because much of the physics and engineering faculties were hard working people, trained to be hard working people, and the ivory towers all tended to look down on each other. It's an atmosphere that I remember, although I can't say that it bothered me a great deal. We're off on another track here. After the war this continued, at Hopkins particularly when Milton Eisenhower became president It was a leap into grantsmanship and receiving government money. That's the current heritage of this university.

**Berkowitz:** Right. The only people who did better were the people at MIT. Some of them were actually inside the government giving the money away.

**Flagle:** It wasn't long before, within that division of Hospital Medical Facilities, that I was asked to serve on the study section which doled out the Hill-Burton research money in 1954.

Some of it was doled out to demonstration hospitals, and we had judgment on that as well, because there had to be something worth demonstrating, that had an innovation or a research quality to it. I was really torn in all directions. I was, at one point, still in the Army Operations Research Office. I remember going to Europe in 1959. I think that was probably the last of the major activities on a project of this sort, that dealt with the capacity to wage war, whether the facilities were adequate. Of course, it was the height of the Cold War by that time. I always said it felt like the depths.

**Berkowitz:** Let me see if I have the chronology straight. You went back to school around 1950 something. You got a Master's degree. Did you know that you were going to get a doctorate from the beginning?

**Flagle:** At that point it occurred to me, yes, because an old professor, A. G. Christie, was still there. In fact he had tried to get me, after my undergraduate education, to go directly to a doctorate, but I didn't want to do that. I'd really never done anything but go to school and do odd jobs in the summer. One odd job the last summer was leading the orchestra, a jazz band, five-piece combination. Traditionally that group had contracts with the _____ Line, so I took them to Europe in 1939. I played trumpet. We had the wonderful experience of leaving on August 3rd and had planned a circle tour through Belgium, France, and

Germany, and back to Holland to catch our boat home in September. We were actually in France at the time France mobilized, so we accompanied the French Army across France to the Maginot Line and then went into Germany. We were there just a few days. We checked into the consulate in Frankfort and were told to get out. After some dramatic adventures, we got out into _____. We got a train into Brussels and then went from there to Antwerp to catch a boat to get home.

It's funny how little things shape things. That was in my senior year, and by that time things were really rolling in anticipation of the war, the country coming out of the Depression. I was offered a job by General Electric and by Westinghouse, both of whom were revving up their turbine and propulsion branches. It turned out that the person who interviewed for Westinghouse was from Holland, a very well known engineer who had done dramatic things like designing the telescope at Palomar. Having just come back from Antwerp-I never got to Holland--there was sort of a natural bond, so I took the job at Westinghouse, which led ultimately to the jet propulsion, which led to military operations research, which led to hospital operations research. There's something orderly about the process.

**Berkowitz:** At least in retrospect.

**Flagle:** In retrospect, yes. It's odd how major events in one's

career turn on almost *chance* events. If the interviewer had not been the man from Holland, I might have gone with General Electric and had another career. And if the president of the university hadn't decided to bring all the branches together to find common grounds for sharing resources at the time he did and with the people he did, I probably wouldn't have been invited to come into the hospital at all.

**Berkowitz:** Let's see if I've got this now in a linear sense. You went back to school, you took a degree in engineering. When you were studying engineering were you studying about jet propulsion? What were you studying?

**Flagle:** I thought I would be studying the design of equipment that relied on mechanical principles, and I was interested in the higher tech things and that led to high-speed rotating machinery. It was that kind of knowledge that not just I--Lord knows, I was the youngest of the bunch, and this Dutch fellow was one of the key persons in conceptualizing the gas turbine as a means of aircraft propulsion. It would take the cold air in, compress it, which makes it hot, put some fuel in and ignite it, which brings it up to a very high pressure and shoots it out the back at what would be supersonic velocities. Since Newton, we know that for every action there is an equal and opposite reaction, so there would be a tremendous thrust. The characteristics of it were quite different from conventional propeller-driven. That's why

we had to get into all of it, not just the mechanics of the engine itself, but the mechanics of its utilization for aircraft propulsion and from that into the characteristics of the aircraft and what it would be used for.

**Berkowitz:** Yes, I never thought about that. The engines are different than the aerodynamic part of the plane.

**Flagle:** The principle has been known for a thousand years, but why it couldn't be brought to fruition was that there were no metals that could withstand the high temperatures for a machine whose rotor was going 18,000 r.p.m. But my job ultimately there was designing control systems.

**Berkowitz:** This is your job at Westinghouse you're talking about?

**Flagle:** Yes. By the way, control systems are closely related to the whole idea of cybernetics, system stability, system control. And that turned out to be a very important aspect of the practical application. At the time, my job was installation flight testing these things. We installed one of the installations within the tail gunner's slot the B26. The Navy had some version of it. That was our test. We had to take that thing up in the air and test for susceptibility to flame-out and other problems.

**Berkowitz:** So when you went to graduate school, were you studying this kind of thing?

**Flagle:** Believe it or not I had the same professor, still teaching the same courses. It seemed that in ten years, nothing had happened, as far as the professor was concerned. That professor retired and a new professor came in to head that department, and at that point the field changed. It changed from something that was quite practical to something that was quite theoretical, and the department actually split into two pieces. Mechanical engineering had been the academic department from which the notions of industrial engineering came. You had not only to design something, it had to be buildable. The minute you get into building it, you get into dealing with the human beings who are going to build it, and this brings you into all kinds of activities such as the design of manufacturing facilities and various human aspects of manufacturing. It turned out that the professor in that area was Robert Lloyd, who later became dean of the School of Engineering, and I shifted my focus of attention to that department because, at that time, the academic attachment of the Army Operations Research Office to the school was through that department. There were many parallels of interest. It was the Operations Research Office that was the industrial engineering of military activity. Many of the principles that applied to manufacturing--manufacturing of everything from newspapers (that was one of our major areas) to physical products--were present in military Operations Research, which I

think is why I was offered the opportunity to work there. As a matter of fact, basically all of my doctoral studies were funded by the Army Operations Research Office.

**Berkowitz:** So you were simultaneously as a graduate student an operations analyst at the US Army Operations Research Center. And that wasn't an academic job; that was an actual job.

**Flagle:** Because it was part of the university, the senior people had adjunct appointments as faculty. The Operations Research Office had been removed from the military so that it could do its studies independent of bias from the Army. It made it independent of what had been considered by the members of that organization, when it was in the Army, as biasing of the objectivity of their work. I think, perhaps, the same was true of the applied physics laboratory, although that group's wartime activities here were in the university and were just expanded to a larger activity. It was too big to stay here so they moved it over to the Skaggsville area. At any rate, we built operations research, as an academic discipline, into the School of Engineering of the university, and many other Schools of Engineering have done the same thing.

**Berkowitz:** You were sort of in a mixed-age environment after the war. You were a doctoral student, but the ages were all confused. It was a rather interesting mix of returning veterans.

**Flagle:** It was. My appointment was called "Executive

Assistant," and I got into the areas of hiring and firing and bringing people on, and got into much controversy because many of the hiring rules and the salary scales and wage increases were based on the assumption that everyone went to work the day after he got a bachelor's degree. So here are a bunch of us coming back *ten years* after we got the bachelor's degree, and they wanted to pay us thirty-two bucks a week. So there were some tough times in there before we could get a formal recognition of the fact that some of the people were even older than the younger faculty in the school. It worked out OK.

**Berkowitz:** When you went to this meeting that you described earlier and then you eventually got to be head of the Operations Research Division, what was the academic appointment that you had?

**Flagle:** I had just been appointed as an assistant professor.

**Berkowitz:** So after you were an operations analyst and you got your degree in 1955, you got a job as an assistant professor.

**Flagle:** I was appointed as an assistant professor in the department from which I had just graduated.

**Berkowitz:** Engineering. That's unusual, yes?

**Flagle:** Well, it's another subject, the notion of inbreeding and when it's good and when it's bad. At the time, I didn't think it was such a bad idea. There was no Operations Research; I really invented the courses. I invented the course called Stochastic

Processes and Waiting Lines. I taught a course in Queuing Theory. It originated out of the telephone industry but was very useful in military operations research. Lord knows, queuing up was a problem in this place. Then I also developed courses on theories of value and decision, because decision processes are always value-laden. We had to introduce just not monetary costs but other costs. The notions of utility and game theory came into play at that time. I was on hand for, really, the *birth* of that subject in Johns Hopkins in the School of Engineering. In the meantime, it was already further advanced at the University of Pennsylvania where one of the key figures was C. Les Churchman in the department of philosophy. Case Western Reserve was another.

**Berkowitz:** Who was the person at Case Western? These are all engineers that you've mentioned. Some are economists, too, right?

**Flagle:** Oh, yes. A lot of the technique is basically from economics and from statistics. Those were the parent disciplines. When you apply it in health, then you introduce demography and epidemiology. You're talking about the academic disciplines that underlie the field, and that was where, I think, I became involved and aware of general systems theory, which is something that can embrace an awful lot, including the notions of the equilibrium of systems and control of the systems, and

embraces economics as it applies to subjective values. We're getting off on the side here, but that's how it works.

**Berkowitz:** This is an amalgam of engineering.

**Flagle:** C. P. Snow and his idea of the two cultures. All of this at that time was going to be that people could help join the cultures.

**Berkowitz:** But also fueled by the military funding.

**Flagle:** Fueled by military funding, yes.

**Berkowitz:** So when you went to this meeting where you got to be Director of Operations Research at Johns Hopkins, you still maintained an appointment in the Engineering School.

**Flagle:** Yes, sure. I taught courses there. As I say, I was split three ways and I quickly gave up the military operations research. Within a few years, I had to make a choice of staying on the academic side or staying on the medical side. By that time, it was a no-brainer for me. I was so involved, I think through Russell Nelson and all of these high level associations in government. By that time I'd gotten into WHO and international work, and that's what I wanted to do. And the academic department veered toward, in fact, changed its name to Applied Mathematics, and there ran into another pecking order, because it was looked down upon by the Department of Mathematics because it was too practical. And it was looked down upon by me because it was not practical *enough* to where anybody could do

anything useful over here. One way I could approach this history was where all my students came from. When I started, they were all from Engineering, and the first degrees they got were doctors in engineering. But the students began to come from other places. I had students from Public Health. One of them was very important to my own career at the time; it was Dr. Michel Lechat. He was a leprologist (deals with leprosy) and ran a leprosarium in the Congo. He was a fugitive, actually not from the trouble that occurred there later, from Graham Green, who had just written his novel, *The Burned Out Case,* using leprosy as the analogy of being spiritually burned out. He used as his model, the hero in that book, a cross between Albert Schweitzer and Michel Lechat. Lechat, who was a garrulous fellow, gave Graham Green lots of material--all on the QT, all confidential--all of which appeared in the book. It was impossible for him to stay there any longer, so he came to Hopkins to the School of Public Health to get a doctorate in epidemiology. He signed up for one of my courses, I guess the course in queuing theory. He had brought truck loads of data with him, and we collaborated on a number of studies on optimal strategies for screening for leprosy. It was a case, again, of shortage of resources. Traditionally, pathologists would take about fifteen tissue smears from a suspected leper and analyze them. Some new drugs had been developed called sulfa. It actually was developed in

1941, but their use had been forestalled because of the war, and it wasn't until the late '40s--'50s actually--that they were put into effect. When they were, the number of people appearing in the clinics under the trees increased tenfold, and the laboratories couldn't handle them. So the question was, what do we do? You either handle only ten percent of the people and let the other ninety percent go back into the bush and infect others, or you compromise your screening techniques and capture as many as you can with imperfect techniques, knowing that you're sending some people back who are infected. So there was an ethical problem there. The logistical problem as it was solved, as it worked out, was that one tissue smear, I believe it was either the nasal mucosa or the lesion itself, caught eighty percent of the cases, so with the limited resources they had at the time, they could actually detect more cases and put them under treatment that way than if you curtailed access to screening. Of course, recognizing the inadequacies of that and striving to get more resources, you're dealing with Third World economies--in that case, the Congo, a benighted area of the world, as we're all learning these days, at least in literature.

**Berkowitz:** I saw the movie with Audrey Hepburn. I know about that.

**Flagle:** I'm not talking about that book.

**Berkowitz:** But it was about that same kind of problems.

**Flagle:** I'm talking about the book on the role of King Leopold of Belgium. Have you seen that? It's quite dramatic. I saw an article on it by the same author, who does a beautiful job of summarizing it. You get the environment in which these heroes like Albert Schweitzer and Michel Lechat were trying to do what it was they wanted to do.

**Berkowitz:** You said that you eventually decided to leave Engineering and go over to the hospital.

**Flagle:** That was not leaving engineering; that was leaving the Engineering School.

**Berkowitz:** And that was around 1963?

**Flagle:** '63, yes. By that time I'd been promoted to full professor. It was pretty fast, believe me, at the time. Again, I think having to do with being "elderly" when I arrived on the scene to do graduate work. I was first appointed full professor in the School of Engineering, then came to the Department of Mathematical Sciences and was given a joint appointment in Public Health because the dean there--there's another hero in my life, John Hume--who was at the time the chairman of the Department of Public Health Administration, had seen what was going on in the world and how that department should be expanded in its scope. He wanted to bring in economists and operations research, sociologists--he really did build the department tremendously. Another key moment was after I had received *my* joint appointment

in Operations Research, he said to me, "There's this field of medical care that's developing within the American Public Health Association, and we want that represented here in the department. Who do you know in that field?" Of course, by that time the study section was in full swing, and I knew a lot of people, including Kerr White. And I told him, "Kerr White is the guy we ought to have." He said, "Get him. What do we need to do to have him come here." And that's what happened immediately thereafter. Whether I just made the introduction, I don't know, but within a year Kerr White and his group had come here and were very quickly established as a separate department. The department had become so cumbersome that it broke up into a Department of International Health, we brought Sol Levine and his group in from Boston University for sociology, behavioral sciences. That was spun off into a department itself. And our own department then became the Department of Health Services Administration. I stayed in that department and headed the Division of Operations Research, which is now, through several transmogrifications, the Department of Health Services Research.

**Berkowitz:** Tell me about that study section. You said that you started in 1956 or so on the study section?

**Flagle:** It would have been closer to '59.

**Berkowitz:** And you said this involved the Assistant Surgeon General. Who was that?

**Flagle:** Jack Haldeman, another hero. This time, I'm afraid, unrecognized. He was the one who really made the bold steps. The rather innocently sounding term "progressive patient care" had a strategy behind it--it was essentially Kerr White's strategy--which was to break up the medical fields, to organize a hospital, in Haldeman's view, according to the levels of need of the patients, rather than according to diagnostic classifications. Let's say the fields of medicine are medicine, surgery, pediatrics, with all kinds of subdivisions in between. In those days you still had practically only one professor for any department. That followed the old German school, pre-World War I. As you know, the big four here were pretty much trained in Germany and tried to recreate that system here, little knowing that within less than 50 years people from here would be going back to Germany to help recreate our system there.

**Berkowitz:** That's a major intellectual theme actually, that in the beginning everybody went to Germany in lots of fields--economics, physics--and by the end of the century *they* come *here*.

**Flagle:** In my early days in engineering, especially at Westinghouse, we did things we thought we would be scolded for because it was incorrect. We were imitating the German accent, "Ja. Ve do it dis vay, you know." There was much mimicking of Hungarian accents and what not. I think since most of us were trained, and even *worked* in that environment, we were the guys

who broke the bounds of it in some way. Haldeman was attempting to do that with these notions of progressive patient care, which ultimately prevailed, but in a very compromised way. Sure, we have intensive care units, but do they serve all the patients across all the disciplines? No. Medicine has its intensive care, surgery has its intensive care. We just made a matrix of things in which the levels of care now were a dimension that was administratively recognized, but the old disciplines are recognized as well and are still there, of course, thriving.

**Berkowitz:** So Kerr White was on the study section, you were on the study section. Who else was on the study section?

**Flagle:** I don't know whether you have that with you, but somewhere in that particular file is a diagram of who was sitting in the table at that time. It was a very interesting and varied group of people. Many of them were administrators. Cecil Sheps was one, and Cecil was appointed the first head of that section. There was a time when if I ever walked into a room and Cecil Sheps wasn't sitting at the head of the table, I'd think I was in the wrong room, because he was a major spokesman for health services research, although he himself was an administrator. He was Canadian and he was of the same spirit that Kerr White had, and that's the spirit of social medicine, that the primary purpose of the hospital and the health care system was to take care of the people, to be population centered. And that, most

certainly, was *not* the purpose of a research school of medicine at that time. The patients were divided into two classes, private and ward. The ward patients didn't pay because they were the subjects of medical research. My first research here was blocked entirely by the notion that we should be allocating our resources on the basis of the immediate forecast we had of the needs, which had to deal with the intensity, the severity of their problems. That was fought tooth and nail by the medical faculty. One idea was that, if Osler Six had an overload of critically ill patients, and somebody appeared in Emergency, which was the source of admission for the Osler wards, they should look around and see that Osler Two only had one critically ill patient and admit the patient there. Well, it turns out that the docs assigned to Osler Two, Three, Four, Five and Six were studying certain problems. They were interested in certain problems and the next patient that comes in with that problem goes there no matter what the state of the ward is and it's up to us, we representing the administration at the time, to see that the staff is there. So I had to design these systems for floating nurses. The idea was that some of the nursing staff was considered to be a pool that would be assigned where they were needed according to our forecast for the next day. That's all described in an article I brought for you to take a look at, because we did get written up in a conference magazine early on.

**Berkowitz:** So you had administrative in addition to research duties?

**Flagle:** Well, I worked for the administration. Russell Nelson was the Director of the hospital and I was a part of his administrative staff of about six or seven people, one for outpatient services, one for nursing, and I was the one for Operations Research.

**Berkowitz:** So there's a Johns Hopkins Hospital and there's a Johns Hopkins Medical School that's different.

**Flagle:** Yes, a separate corporation. It wasn't until Stephen Mueller's time that the head of the hospital and the head of the School of Medicine became the same. He was head of the university *and* the hospital. The governance was primarily by a joint Board of Trustees; there were four trustees from the university and four from the hospital. Any issues that required a decision went to them first. If they were unanimous in their decision, then that was binding on the two boards. It was kind of a mediation thing. If it was *not* unanimous, then it was back to the boards and more arguing and more allocation of funds, whatever it took to solve the problem.

It wasn't long before the Medical School became aware of what I was doing, and it happened first in pediatrics where the administrator of pediatrics invited us to do a study of what was called the PET, the pediatric outpatient service, where kids came

in by the hundreds. After they'd been milling around in this common area, they were screened into contagious and not contagious. Then they sat until the doctor looking at their records called them. They'd go through a torturous process before they ever saw a physician. So I had a few students to work on that problem, and it was a problem of queuing theory.

**Berkowitz:** These were basically people off the street, right, that were coming in?

**Flagle:** Right. These were the poor people coming in off the street. These were the ward patients. They weren't charged. There was either a dollar or two dollars a head per visit paid by the state. In fact, I was told one day by the treasurer that the thing I ought to study was the economics of outpatient care, because his study told him that you'd be better off if you had someone standing at the door with two dollars and you give it to the patient and tell him to go someplace else. A very cynical view of things.

**Berkowitz:** You said this was problem of queuing theory.

**Flagle:** "Congestion in the Outpatient Clinic" was the name given that study, and it resulted in a reorganization of the administrative services. But those services involved the doctors, the screening physician, the nurses, it involved all the caregivers. I was invited to give a seminar for the Department of Pediatrics in which the chief appeared and simply edicted that

that system would be put in place.

**Berkowitz:** Was that Robert Cooke?

**Flagle:** Bob Cooke, yes. How all these things tied together! He was closely tied to the Kennedy family, and it turned out that the chief resident in the outpatient clinic was Fred Richardson, an Englishman who was a specialist in cerebral palsy, and Cooke got a lot of money from the Kennedy family to build what is now the Kennedy-Krieger Institute. It had been the old Children's Rehabilitation Institute. It competed with other people, Sinai included and Hopkins included, and won the building across the street, which my boss, Russell Nelson, didn't want to happen. I have to say this, if you were interviewing Russell Nelson, you'd find I was not the top-most popular guy in his book, because many of my recommendations were not what he really wanted to hear. He didn't want to hear all the things I had to say about the nursing staff and what needed to be done there, but I must say he did things that were downright heroic. I pointed out to him that we were losing people from our nurse-aide training program. The turnover was unbearable and it was costing us a bundle. He said, "Where are we losing these people?" I said, "They're going to other hospitals. The other hospitals need them." He had to recognize that his nurse budgeting scheme was all out of whack, and most of my recommendations, instead of costing less, cost more. What made it tough, I think, for him was that, having been

told that, he really effectively couldn't ignore it. He couldn't ignore some of the things that were happening in the ward services that were, in retrospect--I don't want to use the word "unconscionable," because I don't think that people were deliberately doing things wrong, but there were people doing things they weren't trained to do. It was a shortage problem. It was making do the best you could with untrained people.

**Berkowitz:** When you did a study like this where your audience was Johns Hopkins, was it also published in journals?

**Flagle:** Always, yes. You see, that was the thing. I was thinking coming down on the train, how could I divide my career into pieces, like Picasso's blue period or something. I could definitely say that there was a period which was absolutely Johns Hopkins focused that I didn't get at that time. I wasn't even aware of it. I was not part of this world remember. I just came over here and was immediately involved in on-site, out in the wards, the Emergency department, the outpatient services, observing with a gang of students who all came from engineering.

**Berkowitz:** I bet that you were bitterly resented. You're sort of like the time and motion guy.

**Flagle:** "Oh, you're an efficiency expert," they would say. It's an interesting thing. My supporters often came from medicine. That's because they saw me as the guy who could get to the administration and tell them about the things they were doing.

That was their view of the whole matter. I think I was most resented by the administrators around the Director, because almost every recommendation I had had to do with the way they were running those departments. How on earth could they--unless they were saints--react kindly to that, especially when I was coming in there with all kinds of what they thought was money, money, money?

**Flagle:** Phase one in this activity was totally Hopkins centered. Nelson said, "Look, we're supporting you here, but we would like you to get money to support your students and to build this department." So I wrote grant proposals and the first one was called "Analysis of Congestion in an Outpatient Clinic," and several students wrote Master's essays on that topic, and it did result in a reorganization of patient flow through the clinic that was later picked up in other ways. Through Bob Cooke, I met people involved later on in adolescent pregnancy as well as the CRI [Children's Rehabilitation Institute] which was changed to the John F. Kennedy Institute for *Habilitation*, the argument being that you couldn't *re*habilitate somebody who was never habilitated in the first place. There were semantic overtones to all these things, and the Kennedys were involved, and later, when I got into adolescent pregnancy studies with Janet Hardy, we had national meetings where the word abortion couldn't be mentioned. Eunice Shriver sponsored that.

**Berkowitz:** She was Bob Cooke's patron.

**Flagle:** Sure. Eunice Shriver was that. I met with Eunice Shriver and Cooke and Janet Hardy--all of our people. At one point I felt like Zelig. Remember the Woody Allen movie, Zelig? I was sitting over in Geneva one night and I went to see Zelig, and watched Woody Allen go to a ball game and suddenly start to wear a Yankee uniform. You can't work with people in these specialties where their whole lives are devoted and be drawn into the latest thinking without somehow becoming identified with the field. In their minds, at that time, they would like to monopolize people they would talk to about new ideas or how to solve problems.

**Berkowitz:** So that was the Hopkins period?

**Flagle:** Certainly the "Congestion in the Outpatient Clinic" and its extension to Janet Hardy's work in the adolescent pregnancy clinic was the Hopkins period. Although the latter emerged somewhat later. In the case of the nursing problem, the staffing problems in nursing, the little conflict I had with the physicians who took the posture that nothing could be changed that would affect the way that they were teaching, that that was sacrosanct, a lot of them, it turns out, really didn't believe that. In fact, the noisiest of the bunch, Dr. Harvey, showed up in my evening class in management science. He thought that might be the way for a physician to go who had to be in an

administrative position, that because he was a doctor, he shouldn't sacrifice all administrative privileges to a gang of administrators who, he thought, didn't exactly have the same set of objectives in mind that he had yet had control of the resources. Control of resources was very much affecting the way he lived his life. One of the residents of the Osler Clinic invited me to attend what was the inner workings of the Osler committee, which was all the docs that practiced medicine in that clinic. As a resident, he was also the secretary of the committee. I was to sit there and tell about some of the things that we were doing, and see if they showed any signs of interest. I sat alongside of Wagner, and as he took the minutes, he wrote down, as the first topic, "The committee deplored the following," and then just sat back. "I think it's a disgrace to this institution," was the first thing they would say, "that we can't get distilled water." The problems were, you'd think, mundane, but not to them. I ran into it today. The water wouldn't turn off over in the ophthalmology clinic where I was volunteering to test elderly people, and the director was complaining bitterly. He said, "If this were in my house I'd fix it myself, but around here I'm not going to be a damn plumber." So that kind of thing still goes on. At any rate, most of the problems had their origins in someone deploring actively enough to get things moving. Henry Wagner then specialized in nuclear medicine. He

had an appointment in public health and also an appointment in radiology, and he invited me to meet with the faculty of radiology. Russell Morgan was the chief of the department at that time. By this time, the regional medical programs had been created in the National Institutes of Health, and they were going to do what the evolving section of the Public Health Service--it had been medical facilities, expanded into health services and mental health administration-- it had grown in its scope--and ran smack into the NIH. (Real conflict there; that would be another topic of conversation.)

**Berkowitz:** Is this after 1965?

**Flagle:** Yes. Getting into the 1960s. This would have been before Regional Medical Programs, there were studies of heart disease, blood and cancer that took place in the early Johnson administration. A lot of that was really headed by Michael Debakey. NIH was going to get at these same problems through regionalization.

**Berkowitz:** Yes, Debakey had a commission.

**Flagle:** Right. The commission resulted in the Regional Medical Programs, and that was the doctors' answer, the medical schools' answer to the very kinds of problems we had been involved in through administration. Progressive patient care was organizing services, having people go wherever the resources are that they need, was really at the heart of regionalization. The big

medical tertiary care center would take care of the serious problems, and the routine problems at the community hospital, and then on out to the boondocks. The chief of radiology, Russell Morgan, got into that looking for radiology's role in this picture. I remember him telling how he had all the expertise here, but out in the boondocks you have horseback riding radiologists, appearing one day here, one day there, and that we ought to be able to do this in a better way. There ought to be better ways to get our expertise out into the counties. I'm recalling this right now, and this goes back, I'm *sure*, into the early '60s. I don't think I had even gotten into the School of Public Health yet when Henry Wagner and Morgan and I became involved. I'd have to look at the bibliography to see if I had written anything at that time. I don't think I had, but I became aware of Regional Medical Programs, and I must say I was very happy. I had made a little promise to myself that if, by the end of a couple of years, I was not involved with medicine, but only with administration, that I would probably look elsewhere, because I somehow felt the overbearing presence of the medical school and the demands of the medical school, which had to be met by the administration almost as lackeys to the School of Medicine. There was that feeling. And I think there could have been a little bit of resentment on the part of the administrators that, not only did I come in here looking more or less over *their*

shoulders, but that in the process would be drawn into working with the docs and maybe appearing to be their nemesis, making demands upon them. Whatever, it was not an easy roll to fill, and especially to fill the obligations still to a university that was producing students, the criteria for whose doctoral research was totally separate, increasingly separate, from the objectives here. You could pretty much see what happened. It happened naturally. I began to get grants here to train people in operations research--from NIH--but that's later on. We're now back into the '60s and '70s. And so my pure Hopkins Hospital focus was pretty much long gone by that time. There's a list of students I had by name. Up here you'll see "Doctor of Engineering" right down the line, and then, suddenly, "Master of Science in Biostatistics" in the School of Public Health, "Doctor of Biostatistics" in the School of Public Health where, by the way, I had done a lot of my doctoral studies. So I knew the faculty there and was a student in that department, back and forth across town. I think one of the reasons John Hume saw that he ought to have operations research in the School of Public Health, is that the students were going outside the school for some of their courses and finding their research studies there, and, in the process, creating variables that statisticians in the school generally didn't look at. In fact, all of these severity classifications, which was sort of at the heart of an OR, were

just not anything that they had ever looked at. That kind of categorization of people just wasn't in any way a tradition of what they had done, but it very rapidly became a tradition as the forms of reimbursement for health care services began to take that into account. One of my early students was John Thompson at Yale.

**Berkowitz:** The DRG guy later, in the School of Nursing?

**Flagle:** Yes. That rascal, I loved him like a brother because that's what we really became. He had to come down here because there was no one at Yale doing that kind of work, studying these classifications. I must say, he did me one better. He one-upped me on that, because he was not interested in the day-to-day control system, classifying today for allocation of resources tomorrow. He was interested in the whole episode of care, the resources that would be consumed for an appendectomy or whatever. My counterpart for him was Bob Fetter, who was an engineer and a computernik, and he developed what, I think at the start, they thought would help to find the characteristic resource consumption for various diseases. But when he applied his grouping technique, it turns out that for a given pattern of resources he'd captured all kinds of other diagnoses which contaminated the idea of the diagnostic-related group. Who knows what would have happened to it if the government hadn't seized upon it as the basis for case-per-case reimbursement. They never

thought that would happen. They were really interested in capturing the outliers. They were really looking at the appendectomies that fell outside those length-of-stay and other things so that they could be reimbursed appropriately or not reimbursed at all, or reclassified. It's interesting, again, how what appeared at the time as just a decision of "Let's not only look at this, let's look at something else." And Thompson chose to look at something else. I had the joy at one point, when he was unable to attend a conference in Oslo, of giving his paper there. It was an interesting way in which inter-institutional relationships developed.

**Berkowitz:** Let's talk for a minute about health services research itself. I think I understand the pattern of your career, and I understand you were in the study section and that was the genesis of a lot of the health services research. When did you start thinking of yourself as a health services researcher?

**Flagle:** When that study section--which, by the way, had renamed itself Health Services Research before Kerr White appeared on the scene, though he certainly picked it up and gave it a lot of impetus--took the initiative--I think it was around that time--in developing the notion of Centers of Excellence, Centers of Health Services Research at various places around the country. Certainly the University of North Carolina was one that developed

that notion, and eventually that became the place where Cecil Sheps went. But the leadership at the time came from others who were there. The University of Illinois set up a center, actually in connection with the American Hospital Association, and, as I recall, the people there were Steve Shartel. He was certainly among the pioneers, because he stayed there, and the guy who really developed it there set up a center at Northwestern and then was wooed away by the University of Pennsylvania to set up the center in the School of Business there. We knew we had to do something here. Kerr White's job--more or less assumed by him--was to develop such a center. His department was called something like Hospital and Medical Research. Then the centers program was set up. Kerr began to develop it here and had some very definite ideas on the subject, ideas that were pretty radical and weren't happily received by the hospital administration. I could say they weren't happily received by anyone. We felt, frankly, Kerr was a driven man, very dedicated and very autocratic in the way he ran his department and in his view of what he thought the other people in charge should be doing by way of accepting his ideas, and they were definite characters themselves and didn't want to do that. About that time, Russell Nelson left the hospital and Bob Heysel was brought in, and he decided that the research center should be developed within the hospital side of things, and he brought in a fellow

named Malcolm Peterson and designated him as the guy who would set up the Health Services Research Center here. Somehow Kerr White reluctantly accepted the position as chair of the advisory committee for that center and, in a sense, was put in charge of demolishing his own activities and building something that would be acceptable to the new administration of the Medical School. Kerr left. Where did he go? He had come from the University of Vermont; he was developing health services research there.

**Berkowitz:** With the government maybe?

**Flagle:** No. Did he go there or did he go straight to Rockefeller?

**Berkowitz:** He went to Rockefeller.

**Flagle:** You look at the chronology of Kerr's career, and, in a sense, I took his place on the advisory board at the developing Health Services Research Center here. And that's where you are today. I went to the advisory board, but almost as an outsider. I was given the job of chairing the committee that would search for the director of the Health Services Research Center--I think Malcolm Peterson was leaving then--so I chaired the committee that ultimately found Sam Shapiro. That's how we brought Sam here to be the head of the Health Services Research and Development Center.

**Berkowitz:** An interesting choice because he has no PhD.

**Flagle:** Right. I remember Kerr White pontificating: "The very

least we could expect is a physician to chair this, and not just any physician. His credentials must be...." Oh, God, I can remember the conversation. "And he must be able to eyeball the physicians here." We were going down the list of candidates and we came to the name Sam Shapiro, BB?. And someone said, "I think Sam is movable. His activity with the hospital he was with up there is winding down. Let's see if we can bring him." I think Kerr White was the first one to yell, "Let's get him." So all of us went to see Sam, I remember, everybody talked to him. I met with Bob Heysel, who, by that time, was nominally the person in charge, and Sam agreed to come down. And Kerr, about that time, left for--are you sure it was Rockefeller? In the meantime, with health services research more or less ensconced in the university as a hole, one thing that we all wanted, because it was created this way and Sam insisted on, was that he report to the Provost of the University. This was a university-wide activity; it would be supported by the School of Medicine, the hospital, the School of Public Health. By some quirk of contracting, because the initiative for this had been given to the School of Public Health which had submitted the grant in the first place, all the indirect costs went to the School of Public Health. The contributions from the other schools were direct and didn't have feedback to that indirect cost. What effect that had was that when--Heysel with the administrators of medicine and the hospital

were quite willing to let that go on with us over here getting all the indirect costs--the new dean came along, Donald Henderson, and saw this research center, which was in his bailiwick administratively with its director out somewhere in the hospital with a faculty appointment in our school, he put on a campaign to bring things back together, to bring Health Services Research back from a university-wide activity to something within the School of Public Health, and even *there* within a department. So, the department that became Health Policy and Management became the administrative home of Health Services Research. The School of Medicine and the hospital more or less washed their hands of it. My activities divided between hospital and the School of Public Health. Kerr White sitting in the hospital with the Department of Hospitals--all that vanished and came in under the umbrella of Public Health.

**Berkowitz:** One last question: I'm looking at this paper that says "The Economic Analysis of Filmless Radiology," so at one point did you become interested interested in this health technology assessment kind of field? That must have been another phase of your career.

**Flagle:** I just want to say, we're dividing things into phases, and none of them were ever allowed to remain pure. In a lot of ways I realized that much of what we were doing in developing administrative systems for the hospital through a paper and

pencil mechanism, checking this off, calculating that, could easily be done by computer, that it would ultimately be mechanized. In fact, my own students were going out getting good jobs in other hospitals and invariably picking it up in computer applications. But quite apart from that, in my role in study sections, my study sections began to get names like the "Health Technology Study Section" and the "Health Systems Study Section." And they were split off from the Health Services Research Section which kept the polits, so-called, the political science people. We had always been this quite varied group of people, and, I must tell you, it was much less fun for me to see the thing broken into groups in which you didn't have the behavioral science outlook on things.

**Berkowitz:** Like the fellow at the University of Pennsylvania, who was involved with the Social Science Research Council.

**Flagle:** Nate _____ was our behavioral scientist, and lots of our administrators were in departments that remained health services research. I suddenly found myself on the study section for Health Systems and later Systems Technology. And it turned out that, as money became available for research in medical applications--many of them were demonstration projects, really--they insisted on an evaluation component. It was not enough just to punch some buttons and something comes up on the screen. There had to be some objectives for the thing and some systematic

way in which an evaluation would be designed. We had already had to do that for the development grants for hospitals. In fact, many, many of the research grants had a demonstration component--an evaluation component--to go with it. So I began to get calls from people I'd never heard of before--Morris Collins who was Director of Research for Kaiser-Permanente, Oscar Barnett who was the head of the Computer Systems Laboratory at Harvard and the Harvard Hospital--all asking if I'd consult with them and help them design the components. I was kind of a dumbbell in computers. I came along too late, really, to have computers be part and parcel of education. So I had to catch up, to some extent, on that. It was a wrenching experience for my career here, because I found myself with training grants in mental health, largely populated by the faculty of the Department of Mental Hygiene, and they themselves, behavioral scientists, locked in bitter dispute between the psychiatrists and the psychologists. For me, that was a much less compelling thing than the possibilities of computers and their impact on medical practice as well as administration, so I took a definite swerve in that direction. I joined with them to form something called the Society for Advanced Medical Systems, which, by the way, was founded through the Engineering Society. It had its genesis not in medicine but in engineering and made so much progress in engineering that many of the docs, often political docs, from the

Medical Society of New Jersey or something like that, somehow thought that if they couldn't lick them they had to join them, and that's where this engineering and medical union came about. That was quite separate from ideas of administration and quite separate from most of the interests of my colleagues here at Hopkins. I once wrote a paper on medical _____. Who are these people, what is the nature of their organization. And I pointed out that if you looked at them at their meetings, they were from everywhere, very few of them from the same institution. Their mind and interests, or visions, were not prevalent in their own institutions, so they managed to find ways to get together. And by that time there was plenty of money going into that area. But I would say my later years as an active full professor here, except for my course in health systems analysis, were much more closely aligned with medical informatics people from other institutions. And you began to see their names appear. Don Lindberg, who was one of our grantees, who had a health services research center grant at the University of Missouri, was the chairman of that, but they each had a focus of interest in the centers and his focus was technology. He was a pathologist and he was interested in medical language, the classifications within language. He and I became close collaborators, have been ever since. I had lunch with him the other day at the Institute of Medicine. We worked for years together, after I retired, on full

text retrieval systems for the libraries. That was another direction or evolution. The sequence of a career was a Zelig problem; I couldn't be all those guys. I couldn't wear the Yankee uniform. I felt terrible standing up and being photographed in the World Health Organization as an expert in occupational medicine, which I had been drawn into by the chief of that division who came here to study. That was one way this Zelig business happened. They would come here and study and learn about operations research and health services research and think, "We've got to have this in Poland. We've got to have occupational health in Latin America." They were pulling me in all directions. So maintaining sanity and some feeling of doing a decent job of things meant that I had to make conscious decisions to go certain ways and relinquish others. Relinquishing was not a problem in most of them, because the students who were training fit in right here in the fields that are identified with health services research. I have an assistant professor down the hall who's part of the Operations Research program, a vice president of the hospital--*two* vice presidents--have been graduates of the program in Operations Research. But meanwhile, here I was barreling down this highway that was hardly, and still to this day, not well represented at Hopkins, and that is medical technology with a focus on its assessment. I have designed the stuff and I've had a hand in

assembling it into systems, but mainly for the purpose, then, of evaluating that system.

**Berkowitz:** I think that's a good place to end because it brings together the themes.

**Flagle:** Chronologically you can see it in the bibliography. I wrote these things, often very brief because we had to learn to get it out in public. One of the outpatient clinics I studied was Opthalmology and the resident there at the time was a Dr. Stuart Wolfe, and our program attracted enough attention at that time to be featured in this special issue of the Hopkins magazine where you see such people as Ike Eisenhower and Harold MacMillan. So we shared it with the journals. I'm going to give this thing to Dr. Stuart Wolfe today, who, I think, will join us for lunch. It's little anecdotes about the early days of operations research.

**Berkowitz:** That's very interesting. Thank you.