



VOICE EXPEDITION INTERVIEW TRANSCRIPT

The Oral History of Nephrology

FRANK GOTCH, MD

Interviewed by Dugan W. Maddux, MD

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DWM: Today is Thursday, February 14, 2008, and today I am talking to Dr. Frank Gotch. We are conducting this interview in a conference room at the Renal Research Institute in New York. For his training, Dr. Gotch attended the University of California School of Medicine in San Francisco and he stayed in San Francisco for his residency. Dr. Gotch has dealt with defining and understanding dialysis adequacy through the quantification of therapy throughout his distinguished nephrology career. In 1972 he was the Chair of the NIH Hemodialyzer Evaluation Study Group and in 1975 he led the NIH Conference on Adequacy of Hemodialysis. He was a contributor to the National Cooperative Dialysis Study and the Hemo Study. He continues now to actively consult on dialysis technology with the Renal Research Institute.

Dr. Gotch, thank you for letting me meet you in New York to talk to you about the history of nephrology.

FG: Thank you.

DWM: I want to start with just talking about where you were born and raised.

FG: I was born and raised in Humboldt, Iowa many, many years ago. I actually came to California during the early war years. My father died and it was just my sister, mother and me and she wanted to reunite these remnants of the family so we moved to San Francisco and we never left.

DWM: How old were you then?

FG: I was 16.

DWM: 16. And about what year would that have been?

FG: '42. It would have been '42.

DWM: '42. So you're 16 years old in San Francisco and where did you go to college then when you graduated from high school?

FG: I went immediately to UC-Berkeley.

DWM: Okay. And when did you decide to go to medical school?

FG: It was when I was no more than six or seven years old. Why I wanted to do that I have no idea but I always wanted to be a physician. Actually, no, there was a choice in my mind. Did I want to be a veterinarian or a physician? The veterinary because I was raised on a farm and it was really terrible to see sick animals and I wanted to help them; the ones I really was close to. So it's always been a choice between the two.

DWM: Well how did you finally decide to go to medical school?

FG: I don't know that either except that perhaps I had been transplanted to the urban environment of San Francisco and medical school was very close in the town.

DWM: Yeah.

FG: So I think I positively made that decision.

DWM: So how was medical school for you? Was it good, exciting?

FG: It was tough. I guess the thing that made it hardest for me was that I'm not gifted with a real photographic memory and learning all of the anatomical things that we had to do was difficult. I worked very hard.

DWM: Well I wanted to go back just a minute because you know you spent a lot of your life working on mathematical equations so what did you study in college? What was your undergraduate degree?

FG: It was just the "pre-med degree". It was liberal arts I think it was that we had and some biological science, you had to have zoology and a physiology course. Then I did take just one semester of differential calculus and one of integral calculus. After I knew I had been accepted to medical school, the last year, I could branch out and have fun.

DWM: So was math fun for you then?

FG: Yeah it was although you run up against a much higher wall when you do calculus. Everything before had been so easy but that was difficult; but it was so powerful.

DWM: Yeah. So you graduated with a premedical degree from college and went to medical school.

FG: Yeah. Actually I was accepted in the third year of undergraduate school in Berkeley so my fourth year of undergraduate school was the first year of medical school.

DWM: Ah. Okay. During medical school what kinds of things interested you?

FG: Clinical medicine and physical diagnosis and interacting with patients was really, really a high point although the things that I was always attracted to were things you could measure and quantify. Salt and water metabolism was always very interesting to me and particularly in residency and internship.

DWM: So, yeah, salt and water measurement, and this would have been I guess the late 1940s.

FG: Yes, and it's hard to think back but at San Francisco Hospital where I was a first year resident the laboratory measured sodium concentrations once a week. You could not get a serum sodium except on that day that they measured. They had a flame photometer that had been set up which was quite a new thing.

DWM: So you were making clinical decisions?

FG: Decisions without measurements, that's right.

DWM: Right. So you are interested in salt and water. Was there such a thing as nephrology when you were . . .

FG: It had not been named. When I finished residency, Izzy Edelman who's now disappeared, he's dead. I think he would still remember. But he was a major person in this field in the 40s and 50s and 60s in fluid and electrolyte metabolism, body composition studies. I became a fellow of his but not from a nephrology, _____ because that didn't exist, an American Heart Association Fellow. The research I did was on body composition and we measured the distribution of deuterium oxide in rabbits and I collected the gastrointestinal tract from probably 20 autopsies to measure the volume of water and salt within the GI tract, which was one of the body composition compartments that we were studying at that time.

DWM: Amazing. What year would that have been you think?

FG: Let's see, this is embarrassing, it would be '51 I graduated from medical school so this would be probably be 1953.

DWM: Okay. So Izzy Edelman.

FG: He was my hero.

DWM: Your hero.

FG: Yes. He was a very clever man and I got to know him very well.

DWM: So these studies of body composition . . .

FG: And generally salt and water, yeah.

DWM: Salt and water.

FG: Then subsequently went on to membrane transport because in the early days of his career Francis Moore at Harvard, it was fashionable at that time to define body composition and that's what he got into. Then later in life he got into membrane transport and we always thought he should have a Nobel but he never got one.

DWM: Yeah, because these studies of salt and water led the way to really what we understand about management of fluids today.

FG: Right. They're the cornerstone of nephrology.

DWM: Yes. So what happened after you finished this residency and had all this research experience with Dr. Edelman?

FG: It didn't take me long to make this decision but I was interested in both research but really I love clinical medicine and so I went into practice in San Francisco and was a clinical faculty member at UC. For a couple of years I continued doing fluid and electrolyte metabolism research and then I became totally in practice. Except then what happened is that Izzy was made chief of, what ever we called it then; I don't think it was nephrology yet, at UC up at Moffitt Hospital it was called then. So I then inherited the Division of Nephrology at San Francisco Hospital, which was the county hospital at the other campus. So I then spent the next 10 years doing a lot of clinical teaching, seeing patients with fluid and electrolyte problems and gradually working into kidney problems per se.

DWM: So in the late 1950s you were sort of doing a little bit of private practice and then inherited the San Francisco county hospital. Would it have been called the Division of Nephrology then?

FG: I don't think so. I think we called it fluid and electrolytes.

DWM: Fluid and electrolytes.

FG: _____

DWM: And so you were running this division.

FG: Right.

DWM: Overseeing the faculty.

FG: Right.

DWM: Teaching medical _____.

FG: Essentially it was a clinical teaching job.

DWM: Were you doing any research still at that time?

FG: Not at that point. We did some things with osmotic diuresis for poisonings and that became fascinating to me; to get these huge urine flow rates and defining the osmotic driving force. The county would get a lot of overdoses even that many years ago and often they were one or another form of barbiturate that you could manage to greatly increase renal excretion with very high urine flows and we did but I don't think I did much other than that during those years.

DWM: Okay. So when did you get interested in kidney disease and how did that happen?

FG: Okay. That was all kidney disease, I mean in the sense it finally got named. But the big change in my career occurred in 1961. John Carbone who was Chief of Medicine at San Francisco Hospital, at that time, wanted to have an artificial kidney at the hospital and so he asked me if I'd be willing to do that. So I went to my mentor, Izzy Edelman, and I said, Izzy you know should I get involved in this? He said, don't do it. He said, we know now that the heart-lung preparation is critical and important but the kidney's never going to go anywhere. I've never forgotten that. It points up the continuing rift today between the gadgeteers who do dialysis and the more sophisticated elegant nephrologists.

DWM: Yes. Yeah. So Edelman gives you advice and says, don't do it.

FG: Yeah, that's right.

DWM: So what did you do?

FG: I did it. I just did it. I just thought this was very interesting. I remember one of the first things I did when I decided to do this and knew we were going to get a twin-coil kidney and I'd have to be responsible for it, I went to the library and I looked up everything I could find on urea and I found it very frustrating because there just was no understanding at all of how to think about this. I was already thinking of, how am I going to define this therapy. How are we

going to do that and that was a very frustrating time. I read all the early ASAIO journals and looked at the stuff that Jack Maher and George Schreiner and Scribner, all those things. I just felt I was at sea after I finished and so we just rolled up our sleeves and starting dialyzing patients with this twin-coil kidney and it was all acute renal failure. The cannulas hadn't really come along yet. I guess in '61 the first cannulas had been fashioned and implanted in Seattle and so that was starting at that point. But where we were at, in San Francisco, at that time with that twin-coil kidney we did only acute renal failures. It was such a different world because you had to do a cutdown every time you did a procedure so that you only had three or four shots at the patient and so you'd watch every day carefully to look for how uremia was clinically progressing and what the blood chemistries were doing and what the potassium was doing and then you had to make a decision that we have to dialyze again. We saved a fair number of acute renal failure patients, particularly the ones that were mild, with say hypotensive, renal ischemia, but you had to make these decisions. The other thing I remember so vividly in those years is that, you know, always what you were really after with those dialyses were you'd let things build up until you had to do it; you wanted to get a certain amount of fluid off and you wanted to get the BUN down to a low level. With that kidney it was always a guessing game. The ultrafiltration was poorly controlled. It was always a moment when you weighed them after treatment, did you get the 3 kilos off or did you just practically get nothing off. Sometimes the BUN would drop to 10 or 12 and other times it only dropped 10 or 15mg/dl and that I remember as being the most frustrating part of those years; that you had a device and you had to be there. A dialysis was a six-hour time investment for the physician. You had to make up the baths and make the changes. What I always did, there were six bottles of chemicals and I always counted them up to be sure I hadn't missed one, and would change the bath but it was really a six-hour time commitment to do one treatment and then you may have taken off weight or you may not have taken off weight and you may have gotten a big drop in urea or you may not have. It was a very frustrating time with a device that was totally.. We couldn't measure blood flow. We had a single motor pump and it wasn't until the people in Seattle invented this simple little bubble racetrack but we didn't have that with those kind of tubing at that time so it was exceedingly primitive and very demanding therapy. I do remember one other thing that sticks in my mind; this must have been around '65 or '66 _____ I managed to get a kidney put in UC up at the Medical Center so we could dialyze private patients that would come in there with acute renal failure. I remember one night going up to the 11th floor where Izzy's office was and dragging him down to look at this kidney; now this is what we're doing Izzy. This patient would have died otherwise.

DWM: Otherwise, yeah.

FG: Yeah.

DWM: It's an amazing story. It's amazing to hear you think back about that time because we've come so far, you know, with dialysis that we forget I think what people really were going through when they first started this therapy.

FG: Right.

DWM: I want to go back where you said that you went to the library and looked at the literature on urea and that there was not much there. I mean what kinds of things were even written about in the 1950s? What do you think was the understanding at the time that all of sudden we had this device that could remove urea and, as you say as somebody who is interested in quantifying things, it sounds like people were just starting this therapy without much in the way of a notion about how to quantify that.

FG: That's right. That's right. As I think back _____. You know the big part of my life had been urea and quantifying it but I was going to say, at that time, it was just what the BUNs were before and after the treatment and there were already questions how much of a role urea played as a toxin and actually I found this in something that George Schreiner wrote. That the connection of urea with uremic symptomatology was some time in the 19th century, the middle part of it or so, but within 15 years Bright had published a paper that it didn't make any difference, the urea level was not related to the degree of symptomatology so this controversy was there of course and it had been already for a century almost. I think when I really started working in this field to quantify it is I remember distinctly a patient of ours, in probably '67 or something like that, a home patient because then we established a home program at San Francisco Hospital. This was a patient who lived in Sacramento, he was a Japanese-American probably in his 40s, and he came in for one of his routine follow up visits and I remember in the office I saw him in the examining room and he had a little sense of numbness in the second toe and my first thought was, oh my God I've got to increase his treatment time two hours. Nothing else was there and nothing else to see or feel and I remember so distinctly walking back to my consultation room, how do I know that. What do I have to really say I should commit this man to another two hours of dialysis? And that's where my quest for this began of trying to really measure it. Then I was very fortunate because during that whole time I was working with Ben Lipps and John Sargent from, what was it, Dow Chemical Company originally, and we gradually worked through this thing. I think the most satisfying moment in my life is, and it was about in the same time period, we had two patients, they were both female patients, both in their 50s probably, and one of them it was just so striking the BUNs once a month it was measured and it'd be 90 and the next time it'd be 30, 50, all over the place and the other patient the BUNs essentially were in the normal range predialysis; they'd be 10 or 12. How can this be? What is this? One day, just for fun, we already had figured out how to use this to measure PCR, protein breakdown, I plotted the BUNs on a PCR axis and they all fell on a single line and so I thought, what can that mean then it suddenly dawned on me, it's the dose of dialysis that was constant. It was Kt/V and so that plot, it still sticks in my mind seeing those points all over the map form a line. That was one of the most satisfying moments I think in this whole business for me.

DWM: I can only image, that was a moment for sure.

FG: Yeah. It really was.

DWM: Let's go back to those early years in the 1960s when you were dialyzing patients. You were talking about the twin-coils and acute renal failure patients; I mean that's it in the early 1960s.

FG: That's all it was.

DWM: What kind of patients were you seeing? What kind of diseases were bringing them, or illnesses, were bringing them to you? And you were pretty successful you think in this?

FG: Yeah, fairly successful. I don't even know now what our survival rate was but it was certainly, I think reasonable. Unless it was just a, you know, just diffuse intravascular coagulation problem and just overwhelming sepsis, it was the comorbidities that got them usually. I don't think we lost too many because of pure uremia. But it was a county hospital, a lot of it was trauma, it was a very active emergency room and so a lot of it was trauma. We also saw the overdoses and hypotension and combined poisoning and uremic failure. Of course we probably also saw some of the irreversible acute... see I've been away from clinical nephrology so long. I would say that most of what we saw was post-traumatic acute renal failure. We saw some crushing syndromes. We saw some myoglobinurias and those kinds of things, which do fairly well if there isn't a lot of other stuff.

DWM: Would you say that you could recognize, even at that time, that these would have been patients who would've likely died before you ... ?

FG: Oh yeah, I think so. I think so because, well, we didn't dialyze them unless they had become severely acidotic and very high BUNs and very high creatinines and threatening potassium intoxication.

DWM: So that within 24 to 48 hours they would likely have died?

FG: That's right.

DWM: Without this treatment?

FG: That's right. Because we had to carefully take our shots because we only had a few.

DWM: Yeah. Yeah. I mean three or four.

FG: Yeah, and that was it.

DWM: You were done. Yeah. You mentioned you were using the twin-coil dialyzer.

FG: Yeah. That was the workhorse I think in the United States at that time. The Kiil dialyzer which Scribner brought back from Norway or Sweden, where was it, that had not really made an appearance in the U.S. yet. Everybody that did have a kidney then, and there weren't that many around, but who did have it, wanted something you could take out of the package, plop into the canister and do a cutdown, hook up the blood lines and start.

DWM: Did you have a big tank?

FG: Yeah, a big tank.

DWM: Yeah.

FG: It was a 100-liter tank, I think. We usually drained it three times probably during the run for a six-hour dialysis. Oh God. that brings back another memory. One of the acute renal failure patients who died was really uremic. He probably had a pre-dialysis BUN of 280 or 300 and we set about to dialyze him and he started seizing, developed cerebral edema with acute drop in urea and that really wasn't a recognized entity then; that you could do that to people. That sticks in my mind. That was a young man too. But they were mostly traumatic and the twin-coil was the kidney of convenience and that's why it was used but it wasn't a very good kidney.

DWM: When and how, in San Francisco, did you move from just dialyzing acute patients to beginning to dialyze chronic patients?

FG: Okay, that's an interesting story too. One of our patients, I'm trying to remember now what his medical disease was, I can't remember but it was an acute renal failure patient, who was referred down and very uremic, from Sacramento. He was in the State Attorney Generals Office, he was a lawyer, and he survived and we dialyzed him several times. We did a lot of cutdowns on him but he did survive. At that time, in the legislature, was a bill to establish two regional dialysis centers for home dialysis in California; I think that must have been about in the late 60's, '66 or '67, and he gave testimony and it was brilliant testimony of the great need for this bill. So, I wrote up an application. There was going to be a northern California and a southern California center and we got the funding for the northern California center so now all of a sudden we really were in business. We had a nice floor down in one of the old infectious disease wings of San Francisco Hospital, 10 stations and we were set up to... when the Kiil kidney ... we learned how to make that... we had a laboratory and everything so that's where it started.

DWM: So when you really are beginning to scale up and you have 10 stations, you're moving from the twin-coil dialyzer to the Kiil dialyzer and by that time, what kind of access are you using?

FG: That was all Scribner's shunts when we started there but these miserable things they would keep clotting all the time. Do you remember when the Cimino-Brescia, what year was that?

DWM: That would, I think, have also been the late 1960s.

FG: Yeah. And when that came of course then that solved so many problems, when we finally got surgeons that were trained to do it and create the right size fistula. In those early years even with the Kiil it was a much easier device, once you had them built and everything, but we had no control over flow. We could measure it but it was often passive flow until we finally got blood pumps. Then with bubble racetracks and blood pumps we could prescribe a certain blood flow and a certain dialysate flow. Then much of my time over the next years was involved in quantifying dialyzer performance.

DWM: Let's talk about the bubble racetrack. Describe that to me.

FG: What this was, was a measured length of racetrack and you had to be sure at least the beginning and the end were at the same level so there was no gravimetric asynchrony. So you'd inject a bubble, start a stopwatch and see when it reached the other end and then you had a little chart you could transform that racetrack bubble time to a blood flow rate.

DWM: So it was a length of tubing that was level from end to the other.

FG: Yeah. In the blood line.

DWM: In the blood line.

FG: Yeah.

DWM: And it was somebody's visual look at...

FG: Right. Right.

DWM: A stopwatch and looking at the bubble and timing it _____.

FG: Exactly.

DWM: How accurate do you think that was?

FG: Oh, I think it was remarkably accurate. Well it depended. Then they wanted to shorten it. But if it was 150cm, as I think what it originally was, you could measure blood flows; we did some studies on that too. What did we do with that? We did something, I remember it being clever, but I can't remember what it was. I don't know. But it was accurate. You'd certainly get very precise blood flow rates with that thing.

DWM: Who figured out that technique?

FG: That came from Seattle.

DWM: Seattle. So were you talking to Scribner? Did you know Scribner?

FG: Yes, I knew Scribner and Scribner and I ended up on very opposite sides of thinking about uremia.

DWM: In what way?

FG: Well Scrib, even to the end, he wrote an "editorial" with Dimitri Oreopoulos just three or four years ago, I can't remember where it was published now, it was a new term to describe dialysis and essentially it was pure time. There is a large group of nephrologists still who feel that it's been terrible that we've gotten more efficient therapy and shortened time. Scribner and Oreopoulos and many people think that it's wrong to not dialyze like they were dialyzed with the old Kiil kidney, six or eight hours. Of course we got involved with more efficient dialyzers, we wanted to know how to use them and we developed the concept of the Kt/V, the exponent that determines the drop in BUN, and successfully shortened. At Davies, where we moved the whole program in the early 70s in San Francisco, we had 14 stations, I think, and everybody was treated six hours across the board and by the time we finished we had a mean treatment time of around 2.5 to 3 hours and so we could double the number of patients that we could see. There was still a great shortage of treatment facilities. But, at any rate, I became a proponent of efficient, high-efficiency, dialysis and Scribner always thought that was wrong and there are a large number of people out there, still, who think that it's a crime to do this. It's driven by industry and greed and it's just one of those things. I don't know if I want this to go on record but it's sort of like they're born again in their thinking about this.

DWM: They're zealots.

FG: Yeah. They're zealots. Exactly.

DWM: In those early days in the 1960s, late 1960s, where everybody's got a big learning curve, new technology, changing technology, I can only image it was a fairly small community of folks who were talking ...

FG: It was. It was.

DWM: Yeah. So you would've been talking to Scribner, seeing Scribner at meetings?

FG: Yes.

DWM: Debating some of these issues, I'm sure?

FG: With the Artificial Kidney Chronic Uremia Program; that was the biggest thing that ever happened to nephrology. Are you familiar with it?

DWM: Yeah. Why don't you tell me about that?

FG: May I get a glass of water?

DWM: Oh of course. Yes. Thank you.

FG: You know animosities develop in everything in life I guess including fields.

DWM: Yes. Differences of opinion can lead to ...

FG: You could call it that. Strong differences ...

DWM: Can lead to animosity. So, you were surely talking to Scribner.

FG: Sure.

DWM: And who else would've been sort of your ...

FG: Jack Maher and George Schreiner.

DWM: Yeah.

FG: And John Merrill. I probably talked to John Merrill more than anybody except Jack Maher. Merrill was one of the real leaders in the early days of dialysis in this country and he really suffered from the negative view of academic nephrology about this business. Franklin Epstein, who was one of the great salt and water people at that time. Merrill told me that one of the nicest things that he had ever had said to him was that, I have to admit that while we were masturbating with fluid and electrolytes you were out there saving patients lives. (I don't think you want to put that in).

DWM: But it brings up that thing that you were talking before that there was this rift between the gadgeteers ...

FG: And the thinkers.

DWM: The thinkers, that's a good way to put it. So you would put John Merrill on the thinker's side.

FG: No. No. John Merrill, he was the dialyzer.

DWM: He was a dialyzer.

FG: And Franklin Epstein was the thinker.

DWM: Was the thinker. Yeah. Why was there that rift? Why do you think those two sides stayed so far apart?

FG: I don't know. It just was such a different discipline. Like Izzy Edelman, he could not see this as being something worthwhile to be involved in. It was gadgets and part of it was because it's true that the dialyzer was such a primitive way to think about therapeutically to approach such a complex thing as clinical uremia. I mean it's true that you could manage, well the big thing you could manage fluids, salts, urea and uric acid and other things that are kidney related but you know it was size discriminatory. You couldn't select the stuff you wanted to get rid of; it was all a function of the pore size of the membrane. I think it was because it was hard to see what it did and how you could think about this in a "scientific" way. It was just taking a broad shot at the illness and hoping that it would help. But it just wasn't really a very respectable thing to have for a career except in isolated places like even the Renal Division at Harvard at that time it was because Merrill was big enough, in Georgetown there George Schreiner had a big program and in Seattle, of course, Scribner but most of the other places around the country were, you know it just... And the other thing is that in those days the survival rate was terrible with chronic renal failure. They had these enormous successes with three patients in Seattle and then there were reports from Georgetown and from Boston and other places, you know, that they all died. I mean it just wasn't... And of course the others used the old twin-coil instead of trying to get a device that was more controllable. But it was just... I don't know why. I think it was poor therapy in part and in part it was something that you couldn't really analyze very well.

DWM: In reading some of the things that were written about the time, it certainly sounds like there were criticisms of ...

FG: Oh yes.

DWM: Even Scribner, the criticism being that, you know, what he was doing was really learning by clinical medicine. He was learning by observation and trial and error.

FG: Right.

DWM: And not in the true sense of elegant medicine, measuring and doing research.

FG: That's right.

DWM: That, you know, it was sort of dialyze and see what happened, make an observation, make a change based on your observation.

FG: Yeah. That was the experience I had with this one patient from Sacramento. When he had a little numbness and I walked back to... how do I know that I should do that, increase that two hours. That's exactly right. That was a big part of it.

DWM: So do you think your practice, even somebody who is very committed to quantification and all of that, your practice you would say at the time was also observation and...

FG: Oh sure.

DWM: Yeah.

FG: Yeah. I mean you felt comfortable because you knew what adequate dialysis was. You'd look at a patient and maybe there was _____ or whatever it was but you'd think, I'm not sure that his therapy is adequate so you'd almost always twiddle the time because in those days with home dialysis you didn't have a blood pump.

DWM: No. What choices did you have?

FG: Not many and you only had one kidney.

DWM: Yeah. Were you going to the ASAIO meetings at that time?

FG: Oh yeah. That was the highpoint of the year, ASAIO.

DWM: Why was that a highpoint?

FG: Well because that's the only place in the world that there really were papers on dialyzers and clinical papers. And then, actually that's interesting, there was a rebellion because the artificial heart paper started to over... No, was it ASAIO or was it against ASN, I can't remember now. But there was a big rebellion of people. There were a fair number of people now really

doing dialysis and wanted to have a forum, a place where they could present their observations so the Clinical Dialysis and Transplant Forum was built. I don't know whether it was rebellion against ASN or ASAIO but there were about 10 years that George Schreiner published the CDTF, which was an annual meeting and a collection of papers.

DWM: Right.

FG: Yeah.

DWM: Were you going to the ASN, as well, then?

FG: Yes.

DWM: I wanted to talk also about the fact that you are recognized as having done the first dialysis in San Francisco in 1963.

FG: Yes, I'm not sure that's really true because there was a kidney that was being used and I think it had almost folded up. Who did this? Somebody had come to San Francisco from one of the southern states but I don't remember where, and had set up a twin-coil kidney at Presbyterian Hospital, which is what it was called then. No, it was the old UC-Stanford, when Stanford was still in San Francisco. So I don't really think I have the privilege of having done the first dialysis. I certainly did the first hollow fiber kidney dialysis.

DWM: Right. Which, we're going to talk about in just a minute.

FG: Yeah.

DWM: Would you have gotten recognition from the community? Would there have been things in the newspaper? Do you think that the people in San Francisco were interested in this new technology? Was there some publicity about that?

FG: Yeah. When we opened, got the state funding for the Northern California Center we did have a write up in the paper on that.

DWM: That would have been 1967?

FG: Yeah. Then when we opened the center at Davies, when I left being really full time, clinical full time at UC, and opened that center where I could have more control over where everything was being done, we had reporters there to see that because this was a really high level state of the art center so there was. You know, it was a long time before anybody really got involved in doing much dialysis in San Francisco. It was only after Medicare came through with that first

funding mechanism that then some units started being operated. But a number of nephrologists didn't want to have anything to do with it.

DWM: Right. And when you started doing chronic dialysis in the late 1960s were you able to treat everyone or did you have selection issues like they talk about in Seattle?

FG: Oh God, no. One memory that sticks in my mind is somebody called me about a patient from some place down in Central Valley in California and wanted to know if we could set him up for dialysis; he was 86 years old. I mean that was just so unthinkable that we would put one of our stations to use for a patient like that. I mean it just; you couldn't do it.

DWM: So how were you selecting patients?

FG: We first started at San Francisco Hospital and we had a group of us; it was modeled after Seattle. We'd look at, as a group, a small group, the complete workup on the patient and what was their home situation because we did nothing but home dialysis at that time. Virtually nothing...

DWM: Home hemodialysis?

FG: Yeah. Home hemo – long term.

DWM: Right.

FG: We did acutes but long term was only at home. So you'd look at it very carefully. Could they be trained? Did you think it was feasible to do it? Of course the younger people had priority and the people without major _____, like multiple myeloma and things that were going to kill them anyway. So it was a group decision. And we were, by no means, able to treat all the people who came along.

DWM: So you did have issues of turning people away.

FG: Oh yeah.

DWM: And they then had a fatal illness.

FG: Absolutely.

DWM: You know when we were talking about what it takes to set up a dialyzer, a membrane and a tank in the 1960s, it's sort of amazing to me that you could teach people and that people could go home and set up a dialysis machine.

FG: Yes.

DWM: What did it take to get people ready to go home?

FG: A lot of hard work and staff that could really work well with people. It had to be a very stable relationship between the caregiver and the patient. They had to be able to understand it well enough not to make mistakes. It was mostly very rote what they learned to do but they did it. They had to do it and learn very precisely and a lot of them did. But it was such a hard, hard thing. That's why I think, you know, there's a real push to do home dialysis again.

DWM: Yes.

FG: Maybe I'm just too old now but I'm pessimistic about that because it's such a huge commitment of time and so many of our patients are now 60s, 70s and even older that I just think that it's not going to ever be possible to do that without some really radical new breakthrough in terms of simplifying the technology.

DWM: Yeah. When you were sending patients home I know that just north of you all, in Portland, the Drake-Willock machine had been ...

FG: Yeah _____. That was our delivery system. Yeah, it was the Drake-Willock.

DWM: So once you were sending people home, you were using the Drake-Willock machine.

FG: Yeah right. Right.

DWM: Which used a Kiil dialyzer.

FG: Yeah, right. That was our system.

DWM: Yeah. Did you have patients then going home just in the San Francisco area or would you have had patients ...

FG: Oh yeah. We had them all over the state, all of the northern part of California.

DWM: Yeah.

FG: From near the Oregon border, Central Valley and we had them from all over.

DWM: How did you keep an eye on them?

FG: We saw them all once a month. They managed and they had free access to us on the phone. We did try to refer them, and did refer them successfully in some instances, for a lot of their care locally but it was such a specialized area not many physicians really felt comfortable with it. So any therapy-related problems were always referred back to us but we managed to see them. I can't remember how many patients we grew to. It wasn't really huge.

DWM: One of the things that, in talking about this time where there was a lot of innovation and there was a lot of change by observation, you know, it would work in Seattle and so it would be tried other places. I've heard from a lot of these early nephrologists that mistakes were made and when mistakes were made they would call up everybody and say I tried this, it didn't work, don't do it. Was it your experience that there was a lot of discussion about mistakes and ...

FG: I don't remember much of that. I don't. Maybe because I'm more isolated as a person but I don't remember that kind of networking going on in my experience.

DWM: Certainly today as a practicing nephrologist, you know, we're all very, very cautious about doing anything new.

FG: Yeah.

DWM: Doing something that hasn't been tried and proven.

FG: Yeah, and rightfully so.

DWM: But I don't know that you all had the luxury of that.

FG: It was so clear cut. If you didn't do it, they died. And you know it wasn't really much of an option there so I think that drove it more forcefully than it would be otherwise. But I'm sure a lot of mistakes were made but we were just learning. The young man with the huge BUN and seizures and cerebral edema was a huge mistake.

DWM: Well not one that you could have ever foreseen or understood at the time.

FG: No, you just didn't know about it then.

DWM: Yeah. Yeah. So how and when did you meet Ben Lipps and John Sargent?

FG: I met Ben; I insist that it was 1966; John insists it was '67 but it was one of those two years.

_____ I want you not to move too far away from me, you're going to have to come back a little closer to me, I don't want to miss a word of this.

FG: At the time we had gotten approval and were building the Northern California Artificial Kidney Center for Chronic Dialysis which must have been about '64 or '65, something like that. At that time Ben was a young graduate, Ph.D. from ... What's the ... the technology?

DWM: Oh, MIT.

FG: MIT.

DWM: Yes.

FG: That's terrible... His thesis was actually in clotting in dialyzers and he had done some very good work and he got a job at Dow Chemical because they were making fibers and were interested in developing a kidney. So Ben came to work at Cordis Dow, I think in 1966. Then they had done some first feasibility studies with Dick Stewart in Wisconsin and now they were ready to start doing some, you know, regular continuing therapy and try to begin to scale up the process and understand it better so they were looking for a place to collaborate with. It was just wonderful timing that we then had the chronic dialysis unit funded by the state and we were ready to go. But the reason that we became apparent was because of Phyllis Ojai, who was a chemist at Dow, and she was given the job of working and trying to get grants from the Artificial Kidney Chronic Uremia Program to help support the Dow development. She asked her urologist for a recommendation for this and that's how I got involved with this whole thing. Her urologist knew me and said, why don't you see Frank Gotch and so that's where the collaboration started.

DWM: So this woman, Phyllis; spell her last name.

FG: Ojai.

DWM: Ojai. And this Cordis Dow grant, was this a government ...

FG: No, it was only Dow then.

DWM: Dow. And it was trying to use funding from the government and industry together?

FG: Well yeah. I don't think they paid the full charge of how much was being done. The Artificial Kidney Chronic Uremia Program was goal-oriented and it went on for about 10 years and they had an annual budget of around 4 or 5 million dollars and it just funded so much. There was such a huge spurt in development in this field then because of that contract or contract research. They had an annual contractors conference and that's where all the interaction occurred because all the contractors, and there were probably 50 or 100 different contractors doing things, they all met in Bethesda once a year in January. Ooh. And that's

where all of the talking and interaction of what's happening and what's going on because there were clinical studies as well as hardware studies, design studies, clotting studies; the whole field, the whole spectrum of therapy in dialysis and kidney disease was covered. They were very productive meetings. I really looked forward to them.

DWM: You said that the Artificial Kidney and Chronic Uremia Program was goal-oriented.

FG: Absolutely. They sent out RFPs for specific things they wanted to do and they contracted the research. They did not accept investigator-initiated projects. They could respond to these requests for proposals but it was a very controversial approach in those days and nobody wanted that. Investigators wanted, by and large, to think up their own ideas and do their own research.

DWM: So who decided what the goals should be? Who decided what areas of research?

FG: They really had some very good administrators in that program and they had an advisory committee of nephrologists. I was on that committee later on, not initially. But all of the areas were looked at carefully and what was the most impressive thing that needed to be addressed; so they got advice from the community.

DWM: That is sort of an interesting; I mean to have it driven by a group of people who will define what they're looking for.

FG: Yes.

DWM: And then demand that the investigators meet that need.

FG: It's a very different approach.

DWM: It is. That sounds very powerful.

FG: Yeah, it was and it was very useful in getting this new therapy off the ground, of how to use the kidneys and how to make the kidneys, well dialyzers. We used to call them artificial kidneys; I guess you call them dialyzers now. But I think it was a very powerful ... Somebody else you should put on your list to interview is Michael Lysaght.

DWM: Spell the last name.

FG: Lysaght.

DWM: Michael.

FG: Yes. He has just retired at Brown as the Head of the Bioengineering Division but he is one of the smartest, I think he is the smartest person I've ever really known. He's very articulate. He can recapture much of this for you, I think. In the interaction of the Artificial Chronic Uremia Program field he would be a wonderful person to talk to.

DWM: What was his involvement?

FG: He was a chemical engineer from MIT also. He was involved with the development of the big open membranes for hemodiafiltration. He was involved in all the original basic research in that field.

DWM: So here we have Ben Lipps involved with Dow and they have money from the Artificial Kidney Chronic Uremia Program.

FG: Right.

DWM: To study this hollow fiber dialyzer.

FG: Yeah, to develop hollow fiber. They had the technology to build these hollow fibers for other reasons, for reverse osmosis membranes. So they were ideally set up to do this and of course Ben recognized immediately the enormous advantage of having those little tiny fibers, so much area with so little blood and such short diffusion paths. The potential for making a far more efficient kidney was there and that's why he was so anxious to go to work at Dow.

DWM: So what were these early kidneys like and how did you integrate them into your treatment?

FG: They were monsters. They were just terrible and the problem was that they clotted. It was so frustrating because some patients would run perfectly well, the kidney looked like it had never seen blood at the end of the run but unfortunately at least a third of the patients would either clot so much that they were losing dialyzer function and leaving a lot of blood in the kidney or it'd clot even before, they couldn't even get a treatment out of it. So that was one of the most difficult and exciting periods of my life in the field because Marsha Keene and I were working in San Francisco. This was mostly all done at San Francisco Hospital after we had the Kidney Center and John Sargent and Ben were working together in Walnut Creek at Dow and so we went through endless iterations of change in design, change in headers, change in the tube sheet that the fibers were buried in and try to understand what the clot... We measured all the clotting parameters and actually all the sophisticated clotting parameters. The only thing that really made a difference, well platelet function made a difference, but prothrombin. If the patient went on Coumadin they didn't clot the kidneys. That was really very clear. But God you can't give people Coumadin to run a dialyzer. And so this was a very, very frustrating time but it was very exciting too because there was always something new looking at it. And there were

rationales for it. They made the headers long so the blood would slow down and not slam against the tube sheet and they tried very hard to avoid any collapse of the fibers because that would lead to stagnation and clotting. So they finally came upon a potting material that wouldn't squash the fibers and they worked out and we had very good geometry in those kidneys and they still clotted. I still think this is one of the greatest insights, certainly that I've ever seen, is we had Herb Perkins who was the director of the blood bank in San Francisco, Ben had him over to give a talk. He was a hematologist and talked about clotting. He talked about intravascular coagulation and Ben made the connection, are these kidneys seeing endotoxins or some contaminants and that that's doing it. You wouldn't believe how those early kidneys were fabricated. They were fabricated and then fiber bundles were chopped off at the end and they were thrown in a battery jar without sterilants, without anything, and they'd almost have mold growing on them until they finally cleaned them up and finished them and polished them. But those surfaces were loaded with pyrogens and endotoxins and, you know, things that stimulated clotting. So he said, we've got to get rid of them; that may be the problem. So they totally changed the process. The kidneys never saw anything except the highest purity of water during the fabrication and they were stored under sterile conditions and, my God, it was just all of a sudden 95% of the problem disappeared. It just was incredible. We finally were able to absolutely prove it because we had studied maybe 30 kidneys and we've followed the pressure drop across them and clotting and we had two groups; ones that were not exposed and the group that was tap water exposed. It was just unbelievable. You could see the ones that weren't exposed, pressures never changed across the kidney and they never clotted and these lines were flat. The others, some of them wouldn't clot, some of them would go up like that and that really.. It was 1973 is the first time I really believed that that kidney was going to work. It was already on the market but we weren't using them at San Francisco Hospital because I didn't think they were... I thought it was just too much blood loss and too much trouble.

DWM: So how long was it that you all were working together for that ...

FG: Five years.

DWM: Five years. I mean surely you got discouraged in the middle of that and just wanted to just chuck the kidney.

FG: Yeah. Yeah. But it was such a wonderful device in terms of geometry. I mean, I've got a picture of one of our little hollow fiber kidneys sitting on top of this 60-pound Kiil and the hollow fiber kidney had three times as much clearance as the Kiil. So it was just such an appealing goal that you just, everyone kept at it.

DWM: Good for you all sticking with it.

FG: Yeah. You ought to talk to Ben too.

DWM: Yeah. Actually I'm beginning to think I should definitely do that. As you're talking about this five-year period of time working with the hollow fiber dialyzer, it sounds like there was a lot of measurement going on.

FG: Yes.

DWM: That this was at least a beginning to be able to get some control over the therapy and understanding what's happening within the member itself. Would you say that's ...

FG: Yeah. The momentous observation that enabled us to decide, because they couldn't make many you had to reuse those kidneys, if it was possible you had to reuse them; how to decide whether they were reusable. Because there would always be some bits of clot so we finally realized that this was a rigid blood compartment so at the end of dialysis after it was rinsed and cleaned you could blow out all the water in it and measure it and if the fiber bundle volume was what it should be for that kidney you knew it was reusable and so that became the standard for reuse; to measure the fiber bundle volume. We also did a lot of studies showing how clearance falls as you lose area in a kidney. You could lose a fair amount of area with little change in clearance so we developed a criteria that a 10% loss of fiber bundle volume would be a reusable kidney and that was very, very useful. I'm not sure they ever told us much; we had little 20-fiber kidneys that Marsha and I would have in a little tiny parallel line with the blood line and we could examine them under a dissecting microscope in the ward, right at the patient's bedside, and we could see the clot form and the main thing we learned from that is that it really wasn't in the fiber it was at the entrance and particularly at the exit. They would get the big mounds of platelets accumulate and you'd get all this red clot then on the entrance it was all white clot that we saw because it was just pure platelets. Another thing that was so satisfying; we'd have kidneys that were going along, no changes of any sort and all of a sudden the pressure would go up and they'd have to come off and we'd often not know what had happened because we couldn't see that there was that clotted. We finally saw some of those that had this little flat membrane forming on the inlet header and that one did it, we saw it. It had dissected off and just plopped onto the tube sheet and obstructed the kidney. So we learned a lot about the geometry of clotting and what they looked like and things like that. But I think the thing that really paid off was the idea that there were pyrogens and clotting factors activating clotting from storing those kidneys.

DWM: From the manufacturing process?

FG: Yeah.

DWM: Yeah. Amazing. In hearing you talk about this time of working with Ben Lipps and Dow and this hollow fiber dialyzer, all of a sudden I'm hearing your math background and your quantification.

FG: Yeah.

DWM: Having gone through that period of time in the 60s where you're setting up dialysis units, dialyzing patients, doing a lot of clinical care, to moving to a time where you're thinking more about research and calculations and quantifications; is that a time of transition for you?

FG: That's exactly right. It was the Artificial Kidney Chronic Uremia Program that made all that possible. I now was working with two very bright guys, Ben and John, and they were very gifted engineers and I learned a lot about dialyzer transport and so it all happened right about then. Then later in the kidney program John left Dow and went back to school to get his doctorate and it was during that time that we began studies with urea and formulating that model and studying acetate and acetate metabolism and acid base. But all of that started during that period of time. And, you know, it was essential that there was the collaboration between engineering and medicine and Marsha was a big contributor. She was the bedside nurse who participated in this every step of the way. But it was this group that just, you know, supported each other in these things. It was very very _____.

DWM: So let's talk about urea modeling and moving towards the Kt/V. So take me through those years of how that progressed.

FG: Well I think the insight was recognizing that the two factors that determine the BUN are urea generation and protein intake and breakdown and how much is cleared by the human kidney or the dialyzer and those two things were firmly in our minds at the time the hemo study was undertaken. The Kt/V became a real idea when we, in San Francisco, I was a consultant to John who was doing the control of the study with the modeling with the Kt/V. He was writing all the prescriptions from California and sending them out. He was a coordinating center in a way, not a coordinating center; it was only with regard to therapeutic modeling. But I remember, once we had this grid, we could look at BUN as a function of PCR and get a family of lines that were Kt/V and I remember thinking that, seeing when we looked at that, in general it was true. The patients who had low therapy and were eating well had high BUNs and they were judged failures for clinical reasons, a variety of clinical reasons. It wasn't chemistry; it had to be a clinical decision. The ones that were in the low BUN treatment group did much better; there were much fewer failures. But there was this block of patients where the protein intake was low and they were failing because they had a high failure rate with BUNs of 30 and BUNs of 100 and it was this group of patients that made me realize that there were at least three categories. Well there were already four because the ones with high and low therapy had long and short time but they basically on the map were plotted with regard to BUN and PCR. And it wasn't until we realized that when we looked at the Kt/V line of one it cut through this low PCR data and it put them all above the line so that was the insight that the dose of dialysis ... Because we wondered at first are these failures down there pure nutrition but then when we could put the family of curves on that we could see that 95% of the patients now were defined

as above Kt/V of 1.0 so that's really where it all developed. And, God, it took two years to get that paper published. That was a long ordeal because the hemo people didn't want it published. They didn't want this interpretation out there so there was just endless correspondence between the biostatistician, whose name I can't even remember right at the moment, and me. Who was the first editor of International Journal at Vanderbilt, no at Duke?

DWM: Ike.

FG: Ike, yeah. He really saved that paper because he followed this thing through and there was a change in the editorship, it went to _____ after his tenure was over, but he kept that paper and I'm so glad he did because he just kept... We'd write one, rebuttal, write again, rebuttal and he finally decided that we had answered the questions and it was accepted for publication. But if he hadn't followed that through and been as careful as he was, I don't think the paper would have ever gotten published.

DWM: Why were the hemo study folks against its publication?

FG: Because they had their map. The hemo group wanted to make this as simple as possible, just predialysis BUN, so they said TAC because they were getting problems with separation because of the shapes of those curves. So they changed the definition to time average concentration, which really was essentially the same thing. So they said that the study showed that if you have a TAC below 45 or 50 that that's adequate therapy but one should also look at the PCR. I mean you had to look at the PCR. You should look at it. They just absolutely refused to look at it as a dose of therapy. They just wouldn't do it. They felt physicians would never go beyond looking at a predialysis BUN and so that's what the final conclusion was and we strongly disagreed with that.

DWM: So after you finally do get it published, what happens in the time after that? How was it accepted and what kind of comments did you get?

FG: I think it made a very big impact right away because it was a new insight. I mean nobody had really put together urea in any meaningful way in terms of a uremic toxin before. But still nobody wanted to have a program and to do that initially, even now to some extent. So the simplest approach then was to do the urea reduction ratio so the URR became the standard, which begged the issue. It at least gave you an idea of the dose of treatment.

DWM: Right.

FG: But you can't prescribe a URR. I mean you can only prescribe a Kt/V. You know, you're a doctor – I think you're probably a very good doctor though. But they don't want to be

controlled by any mathematical relationships generally, clinicians. They want to eyeball it and, well I think you need to increase the time here.

DWM: Yes, we do like to use our skills of clinical medicine.

FG: Pattern recognition.

DWM: Pattern recognition. We're all comfortable with that. I'm sure that's true. Did you get to the point where you were not doing as much clinical medicine?

FG: Oh yeah.

DWM: When did that happen?

FG: Probably soon after I built the unit at Davies Medical Center which would've been in '73 or '74. I was just more and more involved with ... I'd go see patients but I wanted to get back to the things I was having fun with and interested in but it steadily went in that direction. I stopped wanting to go see acute renal failure patients in hospitals around the city and I got more and more involved in the clinical research that we were doing.

DWM: Yeah.

FG: Just went that way.

DWM: Yeah. Because, you know, I think a lot of research nephrologists I have known always knew they wanted to be... they do a little clinical medicine but they almost always are just research people. But your life as a nephrologist really started out when you were in the trenches dialyzing patients.

FG: Oh yeah.

DWM: I mean you are really using all of your clinical skills.

FG: That's right.

DWM: So that's a pretty big change in your career to sort of give that up.

FG: That's right. It's interesting; my life is split into two. The first 45 years were essentially clinical medicine and teaching and beginning use of the kidney and the last 45 years were just totally different, almost all research. And then finally it got that I couldn't practice any more because I have macular degeneration and sensory neural hearing loss. I couldn't hear a

murmur to save my soul then and I couldn't look at a fundus. You know it's getting harder and harder to read.

DWM: Yeah. Let's go back a little bit again still to when you were doing some clinical medicine in 1972 when the law passed that amended the law such that the end-stage renal disease program was covered by Medicare.

FG: Right.

DWM: What difference did that make in your practice? Could you tell that it mattered?

FG: Oh yes it did. It's hard to get a real vivid picture in my mind of what it was like then but it was a huge difference. First of all it made it far more feasible to run a dialysis unit because you weren't always scrambling for every penny to get the necessary money and of course the selection of patients was greatly broadened. At that time, of course, then they started proliferating like local stores, grocery stores, all over.

DWM: How did you feel about, you know, at that time Merrill and some of the folks from the Peter Bent Brigham were beginning these for-profit dialysis centers? Did you have a feeling one way or another about for-profit?

FG: I did. _____ I didn't know too much about ... Gus Hampers was the big person there. John Merrill, I don't think was ever involved in that. I think that was entirely Hampers and then Lowrie. But I do remember very vividly a physician, I won't name, who was a fellow on the Renal Service at UC and finished his fellowship and then there was a big private practice building across the street from UC-SF and he rented a suite and got twin-coil dialyzers and started doing chronic dialysis and made an absolute fortune. Within a couple of years he was a multimillionaire. Then he went out and developed other units up in Marin County and I really felt strongly about that. First of all it wasn't good therapy. He was using this primitive kidney. He wasn't setting up with the best dialyzers and the reimbursement was so lavish, you know, \$600 a month for the physician fees then a big reimbursement for delivering the therapy. He was vertically integrated and I felt very strongly about that.

DWM: There would have been some income, I guess, for your units but were they affiliated with a hospital? I mean those were not... They were all...

FG: I've never worked in a freestanding unit.

DWM: Freestanding unit. Right. Well it certainly created a lot of controversy this for-profit.

FG: Yeah, it did.

DWM: You know some people say that it did allow a lot of people to receive...

FG: Absolutely, and I think that's true. And what I've seen now, I just sort of followed along with Ben and he now is FMC. So I've worked a fair amount with Mike Lazarus and the clinical group there and I think they really try very hard to define good therapy and to do it so I don't have ... And it's the only way it's going to get done. I mean it's got to be organized and you need big units and big operations to do that. But I think they do a very, very good job. And it's very different from the entrepreneurship that I saw at UC in the early years.

DWM: Yeah. How about peritoneal dialysis? Were you, in the early 1970s, seeing, doing some peritoneal dialysis?

FG: Very little. I thought that was such an inefficient way to approach this and there were so many problems with infections in those days. My total experience, really, with peritoneal is we did a few acute renal failures particularly at UC before we had a kidney up there. We had one patient that, I guess it was Joe _____, now I can't remember, I guess we weren't doing chronic dialysis yet, but he presented with an acute renal failure, I think, and I treated him at UC and then he just didn't recover. We put catheters, not a Tenckhoff catheter, a Doolan Murphy catheter in him and we treated him for a few months. The thing kept plugging up and it was a terrible time and he finally died. But then I never really got involved in it. Everyone has biases about something and one of my biases was about PD.

DWM: So dialysis today, if you had a pick a therapy, a way to dialyze today, what would you think would be the best thing to do?

FG: Well I'm not sure I would dialyze to be honest with you. I did have an opportunity to think a little bit about that. I have hypertension and I had a three-vessel bypass 12 years ago and the reason I had to have it is that they found this 5cm distal aortic aneurysm and so my closet angina had to get treated before they would do anything with that. So I was worked up at UC and got the bypass and everything was fixed. Then I had to have the aneurysm three months or four months later I think. But anyway what the problem there was and why this became somewhat urgent was that I had learned when I was a resident, I had already decided salt and water is what I wanted to do, but I had a urinary tract infection and so the chief resident insisted on getting an IV pyelogram and I remember being in there getting the pyelogram and the radiology resident came in and said, somebody ever take out one of your kidneys? I said, no. Well it turned out I have a solitary pelvic kidney with a very short ureter right at the bifurcation of the aorta so this was really a problem with this resection of the aneurysm. So what they did is that they cut off the blood supply and they put the kidney in a bucket of iced saline and the procedure took longer because they had trouble with exposure and they ruptured the spleen with the retractor so they had to then do a splenectomy, which they charged me for. But that was billing and that's what happened. _____ But any way it never turned a hair. The creatinine never budged, nothing happened at all. But I had to think

about it. But when I really thought about it, it must have been about three or four years ago now, my cardiologist had started me on an ACE inhibitor and I was on thiazide. I came in for routine chemistries and follow up, an annual sort of thing, and my BUN was 60 and my creatinine was 3.5 or 4. He looked at it and he said, well you're the nephrologist, go get the tests you need. So I did and I was immediately reassured because I got a urine collection and there was tons of urea and creatinine in the urine so this couldn't be an end-stage process. I was having hypotension and postural hypotension. It turns out that getting rid of the thiazide and reducing the... things just came back like that. But at the time I wondered would I be willing to go on dialysis, if I did what would I want to do. I knew I'd want to do it as fast as possible. I didn't want to spend any more time. I decided well I'll probably try it but if it really becomes an impediment to walk on Mount Sutro... One of my big pleasures in life is we have a nice mountain right in the center of the city which is woodland and isolated and I just love getting out there. But if I was 80 and had a hemoglobin of 9 or 10 and couldn't do those things and was very.. You know it's very difficult to live a life when you get dialyzed three times a week.

DWM: Yeah.

FG: I mean the constraints are so huge. I really don't think I'd do it. Now I think more frequent dialysis would be an option that I'd consider, the short daily time, but I'd have to look and see how well I did on it and whether it'd be worth it.

DWM: Yeah. I think that maybe Scribner said or somewhere I read, you know, when they first started offering chronic dialysis that one of the things that was clear from Scribner and maybe some of you all that were in there early, is that dialysis, although it kept people from dying, was a tough therapy to deal with. That maybe it was not for everybody.

FG: That's right.

DWM: Because it's difficult.

FG: Very difficult.

DWM: And still today, I mean ...

FG: It's still a lousy therapy that you spend lots of money and time in and you don't get that much out of it.

DWM: Yeah.

FG: But on the other hand, many people simply, you know, do not want to die and that's a way not to die.

DWM: What do you think... What can you see that we might see maybe in the next 20 or 30 years that would make this a better therapy?

FG: Okay, what I think there is a feasibility of making a continuously functioning wearable dialyzer but the number one thing that has to be solved is a non-thrombogenic circuit. If you could develop a device... You don't need much function but the sorbent technology has improved a great deal. You wouldn't need large quantities of fluid but if you get a device that could have a continuous flow of 30, 40, 50mL of blood a minute and not clot I think it would be entirely possible to have just a step function change in the quality of life and therapy.

DWM: I would definitely think a wearable kidney that takes away that burden of coming to the dialysis unit and still provide slow steady therapy such that you don't have these big shifts in...

FG: Absolutely, there's no doubt about it, and it would not be that big a burden to have a little pack that you carry around.

DWM: Yeah.

FG: But, you know, I can remember how naïve I was at the start of this. I remember giving a talk and everybody was interested in the artificial kidney, so you gave a lot of talks. I can remember giving a talk that we'll have a wearable kidney in 10 years. Oh God, what a thing to remember.

DWM: Well and I can remember telling patients for these many years that coming into this dialysis center, sitting next to this big machine, it seems very, you know, archaic like we should be able to do better.

FG: That's right.

DWM: But good gracious what is happening in dialysis is pretty technically complicated stuff even though it takes us this big machine to do it.

FG: Yeah.

DWM: It is pretty complex.

FG: If you could get the non-thrombogenic circuit I think that would really open the way to it. And I don't know when that can possibly come. I think a wearable kidney that you have to be continuously, rather heavily, anticoagulated, is simply not an option.

DWM: Right.

FG: I don't think.

DWM: Right. I wanted to just talk a minute about organizations. It sounds like the American Society for Artificial Internal Organs was a pretty important forum for innovation and ...

FG: It really truly was. It was the only place that you could go and talk about these things that you really are interested in. It was the program; it wasn't sandwiched in some place. Now ASN has a big representation in that field, not device development, but the clinical use and certainly there are a lot of very good papers now at ASN. ASAIO is still a forum. What it is now it's the forum for the people that are trying to make a functional heart.

DWM: Yes.

FG: That's by far the biggest ... and dialyzer kidneys have shrunk considerably as far as being in the programs. Of course I don't go to many. You know it's almost pointless for me to go because I can't see the slides and I can't hear what people are saying so I don't go to much any more.

DWM: Other than the ASN and ASAIO, were there any other meetings early on that you were attending or involved with? Any other organizations?

FG: The CDTF while it was there and the contractor's conference.

DWM: Right.

FG: Those are the only two societies that I regularly... Well years ago I used to go to European meetings, EDTA, and to various conferences. I was big in Europe for a while in those early years; I got a lot of invitations. But the ASN and ASAIO by far are the most important meetings that I usually went to.

DWM: You know I want to talk maybe a minute about people.

FG: Yeah.

DWM: Let's go back to John Merrill. Can you tell me about John Merrill? What kind of fellow was he?

FG: He was a very nice man. He was considerate. He was a gentleman, unlike me, and he was a very polite fellow. Just a very nice human being. He learned a lot about dialysis and he was one of the leaders in the field and he contributed to conferences; he was always up to the microphone, but polite and often with very cogent comments. He didn't really get into the

gadgeteering part of it much. He went with what was available on the floor with twin-coil dialyzers. He spent a lot of productive time looking at what happened in these patients. His strong suit was clinical.

DWM: Yeah. Do you ever meet Dr. Kolff?

FG: Yes, and he's such a blunt man. I knew him reasonably well. I was at one of his in-house research meetings in Salt Lake City once when he was still head of the department there and he was a general. He just commanded you to do this and yet somehow it was not like in the spirit of a German commander, he was a nice, very authoritative person, but somebody they wanted to do it. Well it's interesting that the rotating drum kidney was the first real dialyzer and they did understand, in an intuitive way, that they had to have a lot of area and they had to spread that blood as thinly as possible over that because if you go back and look at the early reports of the _____, I can't remember his name now who did it, but a number of people tried dialyzers and they had no idea of the magnitude of the amount of solute they needed to remove. Kolff and Burke, that was the engineer that worked with him, they really understood what they had to do with it. Of course they had more than the blood volume out in the device but it was the first time that anybody really, really had the right understanding of a dialyzer.

DWM: Yeah.

FG: That was a big, big step.

DWM: Yeah, to think through those early basic principals.

FG: Right. And then not much happened until the Artificial Kidney Chronic Uremia Program then they pumped money into design ideas and all the stuff that we have now really started from there including delivery systems, proportioning systems, all that stuff.

DWM: Yeah, proportioning systems. Yeah I think some of that came out of Seattle, Les Babb worked on a proportioning system.

FG: And a lot of that was supported by the Artificial Kidney Program.

DWM: So we talked a bit about John Sargent and Ben Lipps. Are there other people that you have worked with and collaborated with that you think had really big ideas?

FG: Oh, Nathan of course. We've been working together much more so now than really in the past. We both served as consultants to Dow, Cordis Dow and FMC but it's only in the last, probably eight years, well since RRI developed, and it's been a very good relationship between what we can do in Walnut Creek in terms of technology and modeling and the clinical things we can do here. I really have no place in San Francisco any more that I can do any clinical research

because I'm not on the active staff of any hospitals. And there aren't any places; you've got to have an environment like RRI or like we had at the unit in county and in Davies, an environment where research is part of this therapy to be able to do it.

DWM: Right.

FG: So I have to come all the way across the country to do clinical research.

DWM: How did you become a consultant for FMC and what role have you played as a consultant?

FG: Purely Ben. I just followed him along. He went to FMC and so I came along. I serve as a consultant; actually I have an incredibly good job because I can do what I want to do. I don't really have anybody telling me to do this today or that tomorrow. Ben has been the stimulus for many of the things that we've worked on. I told you the clotting and he solved that problem and what I've been involved with, almost exclusively now for at least five years, at least five, is modeling calcium metabolism, calcium and phosphorus, and that comes from Ben. You know there's been no real effort put into that. I remember I was on the DOQI whatever the committee was and I remember talking when they were trying to formulate calcium and phosphorus metabolism guidelines, _____ was heading that group. I had some assignment to look about dialyzer clearances or something and I said, you know there's really no information out there. Nobody's really looked at this system. I said, if you're interested I'd be willing to take on some real hard look at what is available and what needs to be available and after the meeting he wrote me. He said, we've talked about it; we don't think there's any need to do that. That was really surprising to me. Ben has decided, what can we do about this and of course Fresenius has a calcium-containing binder and so, can you adjust the dialysate calcium to compensate for any extra load of calcium from the binder. So I started reading everything that was ever written and I've become an enormous fan of Mike _____ and, what's the other guy in Dallas at ... I can't think of the name. But they did a whole series of studies in the single meal absorption technique in the late 80s and early 90s. But any way, I think that they've written some stuff I could really model of how absorption occurs. So we developed a model that we can try to use to guide therapy and that whole idea began with prodding from Ben. At first when he said change dialysate calcium, you can't do that Ben, this is something we don't understand. You know it was sacrosanct everybody dialyzed with this free calcium for years and years but then we started looking at it and it became so fascinating, so interesting.

DWM: I can imagine. That's certainly an area that we have struggled with clinically.

FG: That's right.

DWM: So to be able to begin to look at it in a meaningful way is a good thing.

FG: And it's a big killer, at least cardiac.

DWM: Yes. Yes. I think that's definitely an issue for sure. So any other people we need to talk about that have come your way?

FG: We've covered ... Seattle was the center in those early years. Georgetown. George Schreiner, I have enormous respect for and he's a very funny man. But he gets started talking and you can't get ever George to stop talking. But I'm enormously fond of George Schreiner. He had a lot of very good ideas in those early years. And Jack Maher.

DWM: Yeah, and it sounds like George Schreiner was politically active too.

FG: Very.

DWM: Which was very important.

FG: And very effective politically.

DWM: Yes.

FG: He was very good. And Jack Maher did so much work in those early years and then Karl Nolph has been very productive and I know Carl quite well. But of course I never was involved in that area of research with PD.

DWM: Right. The PD. Well and he and Dimitri Oreopoulos.

FG: Yeah. That's a whole area you have to get an oral history of.

DWM: Yes.

FG: You've got a big job ____.

DWM: A wonderful big job, an exciting job for sure. If you have any thoughts, I know certainly in the research you've done government has played a role in helping this therapy move along. So do you think government has done good things for dialysis and the care of kidney patients?

FG: Oh yeah. Well, you know, it's a mixed bag. They are very stingy and they've gotten stingier but on the other hand the units are all still opening up and they are growing. But it's very troublesome to me. I haven't been involved in the fiscal aspects of dialysis, not since we started our first with state funding so many years ago. But I hear that the only profit is with leftover EPO and things like that. That just seems wrong to me.

DWM: Yeah.

FG: And of course Amgen has become so enormously wealthy controlling it.

DWM: Industry has had; I mean certainly industry has funded a lot of wonderful research and spurred a lot of great development but industry also is sort of a love-hate relationship.

FG: That's right. That's exactly right.

DWM: True. The Amgen's and... Which also, I mean, Joe Eschbach has died but certainly his work with erythropoietin

FG: Who?

DWM: Joe Eschbach.

FG: Oh yes.

DWM: And understanding erythropoietin has made, probably the time I've been practicing nephrology, being able to treat anemia with EPO has made a big difference for patients.

FG: It has.

DWM: For sure.

FG: There's no doubt about that. You know so many memories ... I remember our first patient with the hollow fiber kidney was a high school girl. She was Chinese-American and they'd moved from Mississippi I think. She's the only Chinese-American I ever knew with a southern accent. We used to tease Marianne about it. But I remember one night, she was an evening patient, and I drove her home and I walked a block with her where her apartment was where her family lived and I realized how difficult it is. I mean you had to walk very slowly with this 18-year-old girl because she had a hemoglobin of around 9 or 10 and that made a big impression on me.

DWM: So you were driving patients home?

FG: Well I did; I didn't do that regularly but I did sometimes for Marianne , yeah.

DWM: I do think, in listening to people talk early on about patients, I mean they got very familiar with these patients.

FG: Very involved with them.

DWM: What they were studying, what work they did, what their family was like.

FG: Actually, Marianne got a transplant then from her mother, fairly soon, and went to college, got a degree in chemistry, I think, and then came to work in the dialysis unit in the research lab and she worked there for, oh, 10 years. Then she got married, had two children, done very well and got rich being a real estate agent in the East Bay. She recently died. She had a second transplant. But she had at least 30 years of very productive good life and we were all so involved with her and followed her through all these things.

DWM: Yeah. Speaking of transplant, one of the things that has come up as sort of one of the differences between what was happening in Seattle and the west coast to a certain extent where, you know, there was a big understanding that chronic dialysis was a therapy in itself versus the east coast, perhaps even Peter Bent Brigham, were they were very committed to transplantation.

FG: That's right. That's right.

DWM: And sort of viewed dialysis as a bridge therapy.

FG: Precisely.

DWM: Yeah. What was your thinking about transplant and the role of transplantation?

FG: Well John Najarian was two years behind me in medical school, I knew him quite well, and he was at UC in the early years of transplantation and I can remember when he set up finally to do the first transplant and it was magic. I mean if they got a transplant that worked, I mean it was a whole new life. It was just – there was no comparison between being dialyzed and have a well-functioning transplant, it really was remarkable. So I never had to be sold on that.

DWM: Is there anything else you can think of that we should talk about while we're recording these thoughts.

FG: I think I've said most all the good and bad things I can think of.

DWM: Well, it's wonderful to talk to you today. I really appreciate it.

FG: I enjoyed it.

DWM: Great.

END OF DICTATION



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DWM/dlb
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