VOICE EXPEDITION INTERVIEW TRANSCRIPT
The Oral History of Nephrology
Donald J. Sherrard, MD
Interviewed by Dugan W. Maddux, MD
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DWM: It is Wednesday, January 30, 2008. Today I am talking to Dr. Donald Sherrard and we are conducting this interview at the VA in Seattle, Washington. Dr. Sherrard attended undergraduate school at Yale University and then medical school at the University of Washington in 1960. After serving in the Army Medical Corps from 1962 to 1964, he completed his medical residency at the University of Washington, between 1964 and 1966. Dr. Sherrard has been studying bone disease and end-stage renal disease for the past four decades. He reported the use of tetracycline labeling to study bone disease in uremic patients in 1972. In 1990 he received the National Kidney Foundation Distinguished Service Award. Today Dr. Sherrard continues his clinical research in metabolic bone disease in uremic patients at the VA Puget Sound Healthcare System. Dr. Sherrard, thank you for letting me come to Seattle to talk to you today about the history of nephrology.

DS: Well, you're welcome.

DWM: And, I want to start with asking you where you were born and raised.

DS: Okay. I was born in Yakima, Washington, which is a funny little town in Eastern Washington, and my parents promptly moved to Seattle and I grew up in Seattle, went to high school in South Seattle, and my dad went to a Chamber of Commerce meeting and some guy from Yale was talking there, and he said, "You ought to go there." So I applied and I went there.

DWM: Across the country. Had you been to the east coast before, and...

DS: Not since I was a kid actually, a little kid. Um, so I hadn't been, basically hadn't been out of Seattle my entire life, until I went to Yale.

DWM: To Yale. And while you were at Yale, what did you study?

DS: Ah, mostly, well I did pre-med, but I mostly, I majored in English. I wrote my senior thesis on ah, well pre-Victorian poets, English poets from Alexander Pope to William Blake.

DWM: Amazing. And you said you were pre-med. What, how did you know that you were interested in medicine that early?

DS: How did I know? I don't know, I cast about for what I might do with myself and I didn't, initially thought of myself as a science-type person when I went there, but I got more liberally

educated, so I did other things. But decided basically that medicine was an interesting profession, career, and that I'd give it a shot. I didn't think about being an academic. I thought I'd go practice medicine somewhere.

DWM: So you finished on the east coast at Yale and came back to medical school here...

DS: Right.

DWM: ... the University of Washington, and how, when did you decide that you wanted to stay to do internal medicine, and then nephrology?

DS: Well internal medicine was sort of a natural. You know, I enjoyed intellectual puzzles and when I finished my internal medicine residency, I decided that I knew so little about kidneys that I had to study kidneys before I could go into practice.

DWM: Why kidneys? Why was that important do you think?

DS: Well it seemed to me it was such a key organ and I knew so little about it, that I didn't see how I could go out into practice and know so little about such a key organ, so I took a nephrology fellowship to figure out how to take care of the patients.

DWM: So about what year would that have been that you would have been starting or thinking about a nephrology fellowship?

DS: Ah, oh, 1965-66.

DWM: Okay. And who did you, when you thought about nephrology here at the University of Washington at that time, who were the teachers?

DS: Scribner. Scribner came here in '59, I think, and started, well he must have come before that actually because he really started dialysis about '59, doing dialysis - mostly acute renal failure - and it was kind of fun to listen to him talk. He said, "You know, I put this huge machine in the back of my pickup and drive around the city and treat people with acute renal failure. Most of them died and people got to know me and talk about my death machine, which was the dialysis machine I used, and occasional patients survived."

DWM: So you would have been aware of that at the time you decided to do a nephrology fellowship...

DS: Yeh.

DWM: ...that Scribner was out driving his dialysis machine around.

DS: Around Seattle dialyzing acute patients and saving occasional ones he said, but not many.

DWM: So was Scribner just a dynamic person? Is that part of...

DS: He was a pretty dynamic person and of course, dialysis was cutting edge medicine in those days...

DWM: Sure.

DS: ...and lots of people were fascinated with finally being able to take care of a fatal disease. I know ah, Clyde Shields - do you know that name?

DWM: I do know that name.

DS: First patient on dialysis. He went to Atlantic City when the scientific meetings were still held there. You know this story?

DWM: No!

DS: And was presented ah, by Dr. Scribner. Ah, I wasn't there but I heard that ah, well Paul Beeson, who I know pretty well, said that it was the only scientific session that he had been at, at Atlantic City, in which they applauded the speaker.

DWM: So did Clyde Shields speak?

DS: Well Scribner spoke and introduced Clyde, who answered some questions. People said very strange things like, 'he doesn't have kidney failure - he couldn't be alive,' or 'he'll be dead in two months.' Those were comments that were made at the meeting, and of course Clyde lived 10 years.

DWM: Clyde was one of the early patients.

DS: He was the first patient on dialysis.

DWM: As a chronic dialysis...

DS: Chronic dialysis.

DWM: ...first patient on chronic dialysis.

DS: Chronic dialysis in the world.

DWM: Yeh.

DS: And he lived a little over 10 years. He died of an MI, which foreshadowed the current situation in kidney disease and we wrote a paper about that, Scribner and I, and Dr. Lindner.

DWM: What was the paper about?

DS: It was called accelerated atherosclerosis in chronic dialysis patients. It was in the New England Journal.

DWM: What year?

DS: Oh gosh, I don't know; '72 or '73, somewhere around there.

DWM: So Clyde Shields' death prompted that paper?

DS: Pretty much.

DWM: Prompted your thoughts about...

DS: Yeh, and we said well, we basically reviewed, I think, the first 40 patients that were on chronic dialysis here and just showed this incredible cardiovascular disease epidemic in that group. I mean, half of them died of cardiovascular disease, which is no big deal because half of the population, half of the people in the country die of cardiovascular disease, but they were dying in their 40s. So that was unusual. Dr. Scribner called it the 'gloom and doom paper.'

DWM: Well let's back up just a little bit. So there you are in 1965-1966, and you see Scribner who is a pretty dynamic man, and you see that he has this kidney machine he is carrying around and beginning to treat people and save lives, and you don't know anything about the kidney, feel like you need to know something about the kidney.

DS: Right, right.

DWM: So...

DS: But by that time, when I started, he had already started his chronic dialysis program.

DWM: Oh, okay.

DS: And in fact, in '63, the University of Washington asked him and the guy who invented marrow transplant, Don Thomas, to leave the University Hospital because they were not interested in proven technology, they wanted to do experimental stuff, and in '63, they asked Don Thomas to leave and take his marrow transplant out in the community and they asked Scrib to take his kidney dialysis treatment program out into the community, and that was the start of Northwest Kidney Center, which was the first free-standing dialysis unit, and was the start of the Fred Hutchinson Cancer Research Program, which clearly a lot of research has been done in both those areas since then. It was a real, in my opinion, stupid thing for the university to do, but they've done lots of stupid things. Probably all the schools have. But the University of Washington, has ah, led the way in some boondoggles.

DWM: So when Scribner was sort of told to leave in 1963, how did he feel about that?

DS: Oh he thought, you know, he didn't think the technique was proven. He thought, you know, and obviously it wasn't, he thought it was premature to try and set it up somewhere else, but he was a friend of, I've forgotten the name - where are you going this afternoon?

DWM: Blagg, Chris Blagg's?

DS: No, well Chris came later actually. No the, he was a friend of the president of the King County Medical Society.

DWM: Okay.

DS: Ah, what's his name. Begins with H.

DWM: The Haviland?

DS: The Haviland.

DWM: The Haviland Kidney Center is where I'm going.

DS: Jim Haviland.

DWM: Yes.

DS: And, Jim Haviland basically helped him get funding, and obtain support in the community, and that eventually became the Northwest Kidney Center.

DWM: Do you remember where they got funding? How they...

DS: A variety of places. They got some, one large insurance company ah, gave them a bunch of money. Ah, they got some grants. Ah, they got donations from the community. Ah, it was just a hodge-podge of funding sources. There was no ah, ah, in general insurance companies at that time excluded dialysis from coverage. It was an experimental technique, right? So, and, and, the insurance companies were instrumental probably in the Medicare passage, passage of the Medicare funding program for dialysis. Because they didn't want to pay for it.

DWM: Right, right.

DS: So they got multiple sources of fundings and started a huge kidney foundation, Northwest Kidney Foundation before the National Kidney Foundation ever got started and it was largely a fundraising program initially.

DWM: Yeh, and well what a dilemma, to be in a position where your university has said, you know, this is not proven technology, you need to take it out of the university...

DS: No, they said it was proven technology.

DWM: Oh, it was proven!

DS: And so we don't need it here anymore.

DWM: Anymore. I see.

DS: You've demonstrated its feasibility, now get it out and into the world.

DWM: Okay, get it out. I see, okay.

DS: And that's what they said to Don Thomas, who got the Nobel prize for his marrow transplant work eventually.

DWM: But Scribner wasn't quite so sure that he was, that it was proven technology. He had...

DS: No he was sure it wasn't, that it was just an evolving technology and you know, in those days we were dialyzing people 16 hours twice a week.

DWM: Right.

DS: We don't do that anymore.

DWM: No. Well let's back up a little bit and talk about then when you decided to do your fellowship in 1965-1966, what patients, what kinds of patients were you seeing?

DS: Mostly chronic dialysis patients. I took care of Clyde Shields for a long time. He taught me a lot of stuff. Ah, and other, you know, of the early dialysis patients because it was, in 1966, the total, although the plan had been to kick out the kidney program from the university, there were still patients at the university and the kidney center was just getting off the ground and patients were moving from the university to the kidney center. Clyde Shields was still at the University at that time.

DWM: What were the diseases most of them had?

DS: Well most of them had glomerulonephritis. Um, ah, did anybody - I think a couple had polycystic kidney disease. Ah, for many years we didn't accept diabetics on dialysis and I remember the first patient who came, um, it was a terrible situation. The patient, a woman, showed up with her husband. Joe Eschbach was their physician at the time and they were getting all set to start her on dialysis and her husband had a private conference with Joe and he says, "You know I'm a young man. I guess I don't want to do this. You're going to have to tell my wife that you don't do diabetics." And so Joe went to the wife and said, well we've decided we can't do diabetics yet. We're not up to it. It is too complicated. So she died. It was, I couldn't believe the husband myself, but I don't...

DWM: Well does, these stories do come up, I mean because all of a sudden you have a disease that within the prior decade was fatal.

DS: Fatal, yeh. Right.

DWM: And it does seem like it was a difficult treatment at that time.

DS: It was, and you know, and the reason for this 'life and death committee,' which we will look at later, was you know lots of people who had money, were moving to Washington state because it was the only place the treatment was available for years. And eventually the state also provided funding for dialysis in the mid-'60s, and they decided that they couldn't fund nonresidents, so you know, it was ah, in fact, there were, even residents there were far more people applying for dialysis treatment than situations were available to treat them. Ah, ah, lots of people died because dialysis wasn't readily available.

DWM: Yes. What a dilemma to be in, having this technology and not quite being able to use it.

DS: Yeh, and the program at NBC did, kind of gets into that in great depths and I know most of the people that are shown on the video, some of whom didn't make it to dialysis it turned out.

DWM: We'll definitely look at that shortly. Yeh.

DS: We'll see that.

DWM: So these are patients with glomerular disease and polycystic kidney disease, and you were dialyzing them, I guess, as a fellow in the hospital as they would present, new patients?

DS: Yeh, yeh.

DWM: Well tell me about dialysis in the university setting, for the new patient who came in. What machines were you using? What dialyzers were you using?

DS: Well we, we were, I can't remember the name of the machine. We were using a Kiil dialyzer though, which the technicians built in the basement of the hospital every, for every treatment.

DWM: Did you go, did you see them build it?

DS: Oh yeh.

DWM: What was it like?

DS: And we started, we may have an old Kiil here, if you haven't seen one.

DWM: I'd love to see one.

DS: I think we do anyway. We built them here, too. When I started here in '68, technicians would build, put the dialysis machine, the dialyzer together, which was about this big.

DWM: You know, we're recording, so you used your hands, but tell me how big.

DS: About 2' by 4', I would say.

DWM: So what did it take to put it together?

DS: Well, there were several layers of plastic, first of all, ah, and these plastic sandwiches were encased in plastic boards. I think there were three layers and a double sandwich in between the plastic boards. And then blood tubing, the blood would run between the two layers of plastic and the dialysate on the outside, between the plastic layers and the hard boards, which held the thing together. So these boards were machine tooled and um, they would have to set one board down, then two layers of plastic, second board, two layers of plastic. I think there were

three layers, and it had, it had to be sterilized before you used it, usually with formaldehyde, which was then flushed out, occasionally not adequately. IV formaldehyde isn't fun.

DWM: No.

DS: And uh, and here we actually were filling the dialyzers, flushing them out after treatment, filling them with formaldehyde and using them two or three times. We didn't have permission to do that.

DWM: So instead of restacking them every time, you...

DS: We just, instead of rebuilding them every time, and they were doing that to some extent at the kidney center, but less than we were.

DWM: So essentially re-use.

DS: Yeh, we were doing re-use in '68, I know.

DWM: Amazing. How long would it take to put a Kiil board, a Kiil dialyzer together?

DS: About an hour.

DWM: Yeh?

DS: So a lot of technician time involved in building a machine.

DWM: Right. So if you had a dialysis coming up, they would build it, get the Kiil board ready, get the dialysis machine. Where would the fellow be involved? What would be the role of the fellow? The physician?

DS: Ah, several things. Ah, the fellows initially put in the Scribner shunts. I put in quite a few Scribner shunts. Wouldn't want to do them now.

DWM: Tell me what it was like to put in a Scribner shunt.

DS: Scary. I mean we'd cut down on the radial artery, put a small plastic tube in the radial artery, clamp it off, identify the nearest vein, put another plastic tube in that, then a connector between the two plastic tubes. It was wild.

DWM: And these were pretty sick patients, I imagine, some of them.

DS: These were pretty sick patients, yeh.

DWM: So you're at the bedside doing your cut-down?

DS: Doing, usually in a treatment room, not at the bedside, but doing cut-down in the treatment room just down the hall from the dialysis unit.

DWM: And what was the shunt made out of? What were the tubes?

DS: Ah, it was made out of polyvinyl and Teflon, and Teflon was the key. I don't know if you've heard the story of Scribner's dream. Heard that story?

DWM: No.

DS: He had an acute patient referred from Spokane to Seattle. Seemed like a fairly typical acute patient. They dialyzed him for three weeks - not uncommon for an acute - but he didn't recover. And they got, you know, this was long ago, they then got plain films of his abdomen and his kidneys were like 6 cm each. So he was not an acute. He was a chronic. But, he had come over in uremic coma. He now was perfectly rational and functional, and Scribner had to go in and tell him, "Sorry, you don't have acute renal failure, you have chronic renal failure, and we don't have a program for that." So the guy went back to Spokane and died. And Scribner had nightmares about that for months and finally said, you know, lots of people have tried to make shunts before but Teflon had just been invented, and he said, "You know, if we could coat the polyvinyl plastic with Teflon so it wasn't sticky, maybe a shunt would work." So he and who was the technician then, Quinton, Wayne Quinton, and he got together and Quinton figured out how to coat the plastic with Teflon and the surgeon, Dave Dillard, figured out how to put it in the vessels, and that was the start.

DWM: So the Teflon really made all the difference.

DS: The Teflon apparently made all the difference and ah, allowed the shunt to stay open and you know, we had some phenomenal lifetimes for shunts. Jim, Jim Albers - I don't know if you've heard that name - he was probably the fifth or sixth patient on dialysis. He was a PhD candidate at Stanford in physics when he got kidney failure and he came to Seattle because it was the only place he could get dialysis and he was a Washington state resident, so he got started on dialysis and he actually had the same shunt for over 20 years.

DWM: Wow.

DS: And it was replaced. He had two revisions in 37 years of dialysis. So it could last a long time, but Jim dedicated his left arm to the shunt. He didn't use it for anything else.

DWM: Anything else.

DS: It was in a sling and never used. And he later became a full-fledged physicist and was head of the physics department at Western Washington University for many years.

DWM: So he started dialysis in the 1960s and lived for 37 years.

DS: Thirty-seven years, yeh. There were a few longer survivors but not very many. And he was, you know, we didn't know anything when he started, but he was a guy that if you said jump, he'd ask how high, you know. Whatever you wanted him to do, and so when we learned something, we talked to him and he said "Okay, I can do that."

DWM: So when a patient came in, the Kiil board is made up. The dialysate, you must have person-by-person had to fix dialysate.

DS: Yeh, they made individual containers in a large vat. It wasn't, they didn't have concentrates. They had huge 500 liter vats. I remember one of the technicians dropped one down the elevator shaft at the university once, spilled it over because the elevator lurched just as he was exiting, and the whole thing tipped over. Burned out the elevator system at University Hospital, and they had, that was, I think that may have been a factor in...

DWM: Telling them they needed to go somewhere else.

DS: ...telling them to get out. Probably, yeh. So they'd have these huge vats and they'd pump the dialysate from these Sweden Freeze actually made the vats, and I don't know what they were used for before, but they tailor-made the concentrate. Didn't have to worry about bicarb in those days because it was a physiologic solution, so we didn't have to worry about acetate and bicarb and all that stuff, and so we could make it anyway we wanted.

DWM: Right. So you make up your machine and get your access going, and what was the treatment? I'm going to get you to slide towards me just a little bit, so I can make sure I'm getting all of this information.

DS: Okay.

DWM: Tell me what a treatment was like. Were the physicians present during the treatment?

DS: Well they were at the beginning. In the first treatments physicians were present throughout the whole run. But later on, physicians were present at the beginning and end of dialysis and they would disconnect the shunt. The shunt had a little connector. You'd clamp off both limbs

of the Scribner shunt and then separate them, and hook them up to the blood lines and the physician would do that initially and so would the, and, and would also disconnect at the end of the dialysis, and in the first year or so, there was a physician present for every run throughout the run. Gradually it became more of a nursing technique and the nurses gradually assumed the responsibility for connecting and disconnecting. Physician was usually present for the beginning of the dialysis and usually visited at least once during the treatment to monitor blood pressure and see, you know, symptoms and so on.

DWM: When you were dialyzing then in the mid-1960s, would you, did you have a set routine? If someone came in as an acute, or a chronic, would you set up, I guess certainly with a chronic, would you set up 'yes we're going to dialyze you twice a week' or with acutes would you wait and see when they needed dialysis again? How did you decide how often to do dialysis?

DS: Ah, I'm not sure exactly because I think this was a decision made out of my league at that time, but ah, we soon came, Scribner kind of dictated that we shouldn't let BUNs go over 100, and if a patient was heading that direction, we should do dialysis. This is for acutes, and actually some data came out not long after his dictum, somebody else - Scribner often invented things and never, told lots of people about it, but never wrote it up, so at any rate, somebody actually in England, compared patients with acute renal failure who were dialyzed when they became symptomatic, or when their BUN reached 100, and there was a remarkable survival difference in the patients who were kind of prophylactically dialyzed, not waiting until they got sick.

DWM: So you all were using that already by then.

DS: We were using that already by then, and um, and for the chronics, it was sort of a, you know, let's try this and see how it works and the reason we now do three times a week dialysis as standard care, is because that's what Scribner was doing in '65 when Medicare decided to fund dialysis. This is the standard approach and they haven't allowed deviation from that.

DWM: Why did he decide three times a week?

DS: Well he started out with once a week, then he went to twice a week, and by '65, most people were getting three times a week, often while they slept, eight hours three times a week, and that's just where it got to. I would bet that if, if Medicare hadn't bought that and said okay this is standard care, and refused to fund anymore frequent dialysis, I would guess, I would bet he would have gone on to kind of more frequent dialysis. Of course at that time we were using Scribner shunts. Fistulas came into existance probably, I think the first fistula we had was in '69, and gradually evolved and by '75, probably half the patients were on fistulas and everybody thought it would be awful to force people to have fistula punctures daily, so that sort of was a hold-up, too, until we realized a lot later than that obviously, that that wasn't too bad.

DWM: Right. Well you know, as we're talking about this, I'm reminded that there weren't any randomized control trials happening out here, I don't think, about whether you should dialyze, you know, twice a week or three times a week.

DS: No. No trials at all. These were all just guestimates.

DWM: Observation.

DS: Observation. Patients did better on twice a week than once a week, and ah, if twice a week was good, there was clearly, people were feeling terrible when they were dialyzing and they would feel better on nondialysis days, and so we came to recognize that that was because of all the biochemical shifts that were occurring so rapidly with once or twice a week dialysis, that we ought to increase the frequency, so that these big shifts, you know, the urea didn't, BUN didn't drop from 120 to 40 in 8 hours, ah, so we would you know, we tried to get people's BUNs, ongoing BUNs to be in the 70s. And you couldn't really do that with once a week or twice a week dialysis, so it sort of grew like topsy.

DWM: That is the observation that you want to find that balance between...

DS: Too rapid a dialysis..

DWM: Yes.

DS: ...and reasonable comfort for the patient.

DWM: Right.

DS: And you know patients, many patients dreaded dialysis. They knew they had to have it to stay alive but when they were just getting it once a week or twice a week, it was a pretty uncomfortable situation. Urea shift was not just an intellectual argument, it was, we were watching it happen.

DWM: Right.

DS: And ah, and the theory all developed after the fact, but I know in '65, Scribner arbitrarily decided that if a patient had a BUN greater than 100, you needed to add urea to the dialysate to prevent too much urea shift, and he settled on a gradient of 100. If patient's BUN was 200, the first dialysis had a urea BUN equivalent of 100 in it, and it helped a lot with dialysis seizures, headaches, etc.

DWM: It does seem like Scribner took a little bit of criticism from the east coast renal community, which was more scientific.

DS: Yeh.

DWM: Were you aware of that?

DS: Oh definitely. I still remember John Merrill was one of the early pioneers that followed closely in Scribner's footsteps but he and Scribner were on a program at the ASN, and the question from one of the participants had asked them both, you know if a patient has advancing chronic kidney disease, what's the most important thing to tell them. And Dr. Scribner said restrict salt because if you don't, you won't control their blood pressure. And Dr. Merrill said give them lots of salt because you need to maintain their blood volume so they perfuse their kidneys.

DWM: Hmm.

DS: Yeh. We know what the truth is now, but back then it was kind of strange, and there was this kind of conflict ah, who was the guy, chief of medicine at Rochester eventually? At any rate, the east coast group appointed an emissary to come out here and ah, kind of mediate the conflict.

DWM: What do you think the conflict mostly was?

DS: Oh, what was the conflict? I don't really know. There was, you know, if you had kidney failure, the best place for you to be was in Seattle. If you wanted to learn about kidney failure, the best place to be was in Boston probably. And ah, so ah, there was just competition. Hard to know.

DWM: Right. Well that, I think it's a good way to put it, that if you wanted to know about kidney disease, there; if you wanted to actually take care of patients, here. I mean that certainly seems like the true thing.

DS: Yeh. That's sort of what we felt in 1968.

DWM: Let's talk, I know we're going to look at the film later, but let's just talk about the selection committee and what you remember about the selection committee, how it functioned and...

DS: Well it was a complicated process. The selection committee was anonymous and so far only two people that were on that selection committee have identified themselves. Several are said

to have committed suicide and not surprising when you see this film. But the process involved first of all, patients had to have money. And the kidney foundation, Northwest Kidney Foundation spent a lot of time trying to find funding for people, from local communities, charitable groups, insurance companies and so on, but they basically wanted 10,000 bucks a year guaranteed funding. The second was the patient had to have no other chronic illnesses besides kidney failure, like diabetes was clearly not in the game then. And thirdly, they had to be, ah had to pass a complicated psychological/psychiatric evaluation - are they going to be able to handle this and that comes out, too. Once they pass those three, then they went to the God committee, the life and death committee. Not personally. They were not seen by the committee.

DWM: They were never interviewed or seen.

DS: They weren't interviewed.

DWM: Yeh.

DS: They just reviewed their records and I think it would have been even tougher, if they had actually had to meet them, but they then after they passed all those other hurdles, then they got presented and usually their decisions were unanimous. Although several times they had one slot and three patients and in general, they chose relatively young, employed white males. It was rare to choose anybody else, although some of the early patients, a few were minorities and a few were women, but most of them were ah, young white males.

DWM: How long would that process take? I mean gathering information about funding, psychological, I mean...

DS: Several months actually. So nephrologists in the community had to identify the patient early enough, that this was an appropriate candidate and get the process rolling because raising 10,000 bucks in 1960, for the average working stiff, wasn't easy.

DWM: Um hm.

DS: So they had to, you know, ah, go through the medical hoops, the psychiatric hoops and the financial hoops, and it took a few months.

DWM: So these are people who have been identified as having chronic kidney disease, they're approaching end-stage renal disease, have not reached it yet, and they apply for the opportunity that when they need it, they can start dialysis.

DS: Well it was a mixed bag. Some people were knowledgeable enough to recognize, read the newspapers. They knew this was going on but it was mostly the kidney doctors, the nephrologists in the community that identified people that they thought were good candidates.

DWM: So here is some pre-selection going on. I mean there are doctors in the community making decisions about...

DS: Yeh.

DWM: ...whether even to try to get some...

DS: Whether even to try, yeh, and the medical ah, you know initially, most I think all the patients that, all the first 40 or so, were all under 40, maybe under 45. Ah, they were all ah, generally employed. Um, they were ah, usually family men. Um, so those were sort of built in biases that were identified after the fact, that maybe we were selecting young white males that were employed that were like the people on the selection committee.

DWM: Were the outcomes pretty good, because this was such a selected group of people?

DS: Yeh, the average survival ah, in Seattle, has actually gotten worse over the years. The first 40 patients, the average survival, I think it is in that New England Journal paper, I think it was eight and a half years, and what did we know then?

DWM: Right, amazing.

DS: Well we didn't know anything more than we know now, we knew less, but we selected young healthy people, and other than kidney failure, and we don't do that anymore obviously.

DWM: Right, yes. No certainly we are dialyzing an aging, more complex...

DS: Yeh, the average dialysis patient now is 66, I think, entering dialysis and half of them are diabetics, which none of them were diabetics in those early years.

DWM: Yeh. So your fellowship, how long did it last?

DS: Two years.

DWM: Two years. And were you also doing some fluid and electrolyte, and other nephrology besides dialysis and...

DS: Oh yeh, yeh. We were the nephrology consultants for the University Hospital, managed fluid and electrolytes. I did research with a guy named David Simpson, who was later chairman of nephrology at Wisconsin, and we did research in glutamine metabolism in renal tubules.

DWM: Hm.

DS: Which was kind of interesting.

DWM: Sure. Did you um, I've heard a little bit about the Scrib kit. Were you using that?

DS: Oh yeh. Yeh we were. In fact, I ah sent it to a friend of mine in Africa, who was a doctor in the Sudan, because she was doing surgery and she had no way to monitor electrolytes. Scribner kit measured sodium, chloride and bicarb. They never had a good potassium measurement but you could measure all those things in about 5-10 minutes.

DWM: What did the kit look like? What was in it.

DS: It was about ah, 8 inches wide, 8 inches high and maybe a foot long, and had little vials and syringes in it. You measured things out, and yeh, we used, I used it as an intern at Harborview because we had a point system when I was an intern at Harborview, for the laboratory and once you got your, you'd used up all your points, you couldn't get anymore labs done. And so I usually did my electrolytes, sodium chloride and bicarb, on the Scribner kit.

DWM: And it was portable, very portable and you could carry it around.

DS: Yeh, it was, weighed about 10 pounds. I knew, Bob Erikson, who was a medical student here and then went to Columbia, they had the same point system at Columbia. So he did, he used the Scribner kit and did electrolyte measurements for everybody in Columbia Presbyterian. I don't know about everybody. He said he spent many nights measuring electrolytes for people.

DWM: And you're measuring urine chloride.

DS: Well we were measuring urine chloride, but blood, too.

DWM: Oh.

DS: Blood sodium bicarb and chloride. The urine chloride ah, we usually didn't do urine sodium. It was, I mean if we are going to do this, we're going to do it on serum.

DWM: Well good, fascinating.

DS: And so the Scribner kit, many people thought was Scribner's greatest invention and it was used in third-world countries, mostly by missionaries that had trained here for many years. Actually I mentioned it to a nurse who goes to the Sudan every so often, just a couple weeks ago, and she said, "Where can I get one of those?" I said well...

DWM: Can you still get them?

DS: I don't know. I gave her some hints about where she might find them and as of yesterday, she hadn't been able to track one down, so I don't know if they're still made.

DWM: Yeh.

DS: It's pretty simple, that they could easily be made again.

DWM: Amazing. So in the middle of all of this dialysis and, and taking care of kidney patients, how did you get interested in bone disease?

DS: Well two, two things. First of all, um, a major problem in early dialysis patients was metastatic calcification, and it wasn't what we see today, like calciphylaxis. These were huge, tumorous lumps. One we removed weighed 10 kg. It was in the guy's shoulder. They got them in their buttocks often around the joints. Some patients got them in their feet. They couldn't wear shoes, and it became, after a while it became clear that this was related to the calcium/phosphorus product and that's when Scribner invented the calcium/phosphorus product of 70, as a bad one, and figured out um, um, that they needed to, they couldn't alter the calcium very well and they didn't know why at the time and do now, ah, but they figured, the GI doc at the University Hospital, told Scribner that you know, there were some antacids that lowered the phosphate a lot and you know, these patients had calcium phosphate in these crystals and they had high levels in their blood. The 70, by the way, came out of experiments with aqueous solutions in which they raised the value to 70 and found that the calcium phosphate precipitated out. So this GI guy told Scrib that, you know, if he used aluminum hydroxide, he often had gotten phosphates too low in some of his GI patients that were using it for an antacid, so they thought about other things. Calcium didn't make much sense because they were calcium phosphate deposits so it didn't seem sensible to give them calcium. Uh, magnesium didn't make much sense because it is so well absorbed from the gut. To give enough to bind the phosphate, you'd probably put the people in coma from hypermagnesemia, so they didn't try that. So they settled on aluminum because it was nonabsorbed and nontoxic. Everybody knew that at the time, and, and you know, Scribner wrote an editorial in the mid-'60s again, New England Journal, saying that his patients were turning to stone and when he gave them these "phosphate binders," which everybody else called antacids, ah, the deposits went away. They disappeared. I have some slides somewhere, that show a patient who had a

huge deposit that vanished with phosphate control. So that was one thing. And then I, after finishing my fellowship, I came over here to help start the dialysis program here...

DWM: At the VA?

DS: ...at the VA, and the guy who actually preceded me, had a grant to study calcium metabolism and he had no idea what to do with it, and how he got it, I don't know but he was tired of it and he said "Here. You take it." So it was serendipity. So I walked into a situation, had funding and started figuring out ways to study calcium/phosphorus in bone and there was another guy here that was a bone guy, who was interested in doing bone biopsies in patients, in osteoporosis patients. So he actually was doing biopsies and I had this grant to look at the kidney patients so we signed up some of them, and I watched him do a biopsy and he used so little Lidocaine, that the patient was writhing on the bed, and I said, I'm going to do this after this, if we are going to do any more. So, you know, I started doing bone biopsies then, in '69, I think. In relation to this calcium grant that the other guy had to study calcium metabolism in renal patients, they mainly were focusing on the fact that dialysis patients were recognized as being "calcium deficient." Many had osteopenia on routine x-rays. So they were looking at how high the dialysate calcium should be to correct the osteopenia the dialysis patients were recognized to have and I remember going into a contractor's meeting and ah, let's see, who is the chairman guy from Heidelberg, um, Eberhard Ritz and I were at the same contractor's meeting and discussing how high the dialysate calcium and there was some concern among the contractors that we were going to overload these patients with calcium and we both said no, that's what we are trying to find out - how much do we need to give them. But once we have it corrected, we can stop. We can lower the dialysate calcium, but we've got to figure out how to monitor it, and ah, and ah, Eberhard went one direction and I went another, obviously, he is more famous than most anybody ah, and has gotten off into other things besides bone disease. But um, but I focused on the bones and my friend and I, the osteoporosis guy, David Baylink was his name. He's still working in Loma Linda, I think. At any rate, developed this bone biopsy technique and with the electric drills, it turned out to be much less noxious than routine bone marrow biopsies, which all of us recognize is probably slightly worse than waterboarding.

DWM: Not good.

DS: Not good. But I, but ah, but ah, you know, we did a lot and I used a lot of Lidocaine, and it's a large drill, large bit. I probably have one here somewhere, which ah, somewhere. I don't see it, but at any rate, the ah, the bit is about 4 mm in diameter and it turns out drilling a large hole into the iliac crest is much less painful than putting a needle in and sucking, aspirating the marrow.

DWM: Ah.

DS: It's the aspiration that really hurts, and so drilling into the, here, I've got to find, I've got to show you that.

DWM: Okay.

DS: That biopsy drill. I've got it here somewhere. Probably down here.

DWM: That looks promising.

DS: That's it.

DWM: Okay, I want to take a picture of that when we finish talking so...

DS: There's the bit.

DWM: Okay. That is big.

DS: It's big and you'd think it would be terrible but I've had three, so...

DWM: Yourself?

DS: Myself and so we drill in...

DWM: Can you leave this out so I can take a picture of it before we go?

DS: Sure. And ah, after a while, I learned if we gave a whole lot of Lidocaine, we didn't need any pre-med or anything else. Initially I was giving people Morphine and all that stuff.

DWM: Right.

DS: But...

DWM: If you could just get them numbed up enough...

DS: I used, you know, 20-30 cc of 1% Lidocaine and people felt the Lidocaine going in and they had about 5 seconds of pain when I pulled a sample out, but nothing like the pain that you get with a bone marrow aspiration.

DWM: So when you first started doing these bone biopsies, late 1960s, sort of all into that grant, what were you thinking you were looking for and what did you think you were going to...

DS: I had no idea what we were looking for an nobody else did either. Nobody had done that before and I didn't know that nobody had done that before, I thought, you know my, don't say this, now this is illegitimate.

DWM: All right.

DS: David Baylink pretended that he knew everything about bones and he didn't know any more than I did.

DWM: Spell his last name.

DS: B-A-Y-L-I-N-K.

DWM: Baylink. Okay.

DS: And so we went along and did all kinds of stuff that we never wrote up because I thought, well this must, people must know this, and they didn't.

DWM: No. So these early years were really just trying to figure out what was going on.

DS: Yeh, looking at the bone. I mean, well let's see, I think I gave a report at the Western Society Meetings in '71 or so, and we had done something called cluster analysis of the 100 biopsies I had done in renal patients up to that point. We had done maybe four normals. Later on we did 100 normals, one of which was me and another was my wife actually. Ah, I paid her 200 bucks for her school.

DWM: Good!

DS: Yeh, at any rate. Ah, but...

DWM: So you started out just looking at abnormal bone, just trying to have a clue what was going on.

DS: Just looking at what was going on and I looked at pictures to see what was normal. We didn't know what, have any normals of our own right away, and ah, eventually did this thing called cluster analysis to look at what we were seeing and we were doing, we started doing tetracycline labeling, which was actually invented by an orthopedist named Robert Frost. Not Robert Frost, Harold Frost, at Cleveland Clinic.

DWM: What was the purpose of the tetracycline label, as you first used it?

DS: Well it's purpose was to measure bone formation and Frost had defined it in, in ah, animals and shown that normal rats deposit bone, tetracycline deposits in the bone, if you give a short label, single fluorescent label, if you give two labels spaced in time, you can see two labels, and I don't know if he actually did much work in humans. I don't think so. He was an orthopedist, so he did surgery, but he noticed when he was doing surgery that for some reason he had a Woods lamp in his OR and he found that some people had fluorescent bones, and when he looked at their medication list, they were getting tetracycline. So the early days of tetracycline use in humans, there was a lot of controversy, because it was such a common antibiotic being used in the '60s and early '70s that everybody said 'well you won't be able to see anything because everybody has been exposed to this drug,' but it turned out not to be true. As the bone forms, the tetracycline moves into the bone and new bone was laid down on top of it, so within a month or two, it is deep in the bone and you can re-label a patient, if you want.

DWM: So when you first started doing bone biopsies, you wanted to see what the bone looks like, I mean just period.

DS: Yeh, just what does it look like.

DWM: And then you got to the point where you wanted to see what it looked like over time, which brought on the tetracycline labeling.

DS: Which brought on the tetracycline. Frost has developed the ah, um, concepts and theories, and so we knew what it was supposed to do and another guy, Michael Parfitt. Do you know Michael Parfitt at all?

DWM: No. No, tell me about him.

DS: Michael was also at Cleveland Clinic and he was the scientist in Frost's group, and he is still working part-time, I think in Little Rock, and he, I'm pretty sure he is a nephrologist, too, by the way. But he mostly did this bone work and is famous for his bone work and a lot of other interesting calcium stuff. But at any rate, he kind of developed principles with Frost and then a few people began to apply them, and we didn't, you know, all we saw in the first hundred biopsies was it looked like just this mish-mash. You know, we had low turnover, high turnover, osteitis fibrosa, hyperparathyroidism, osteomalacia, it was just, and some people had combinations of hyperpara and osteomalacia. We call that mixed uremic osteodystrophy now, ah, and ah, it was really the tetracycline labeling that allowed us to kind of characterize the fact that we had low turnover and high turnover bone disease and a range in between, and fairly early on, we identified PTH as an important factor in, in the bone formation and in the bone lesions we saw.

DWM: So you were looking at PTH levels in combination with your biopsies and...

DS: Yeh, PTH assays didn't come out until early '70s, and they were pretty poor assays. They were carboxyterminal assays, which have been pretty much abandoned now, but basically they still reflected what was going on, although there were many times what was found in normal people and renal patients but we could see that high values were associated with the high turnover bone lesions with marrow fibrosis. Lower values were often associated with osteomalacia and no fibrosis. Of course we didn't know why some people had low values and other people had high values, but we made that connection fairly early, like, I think in a paper in the mid-'70s probably.

DWM: Were people willing to be biopsied? Were you having any trouble with people saying, 'No I don't want to have that bone biopsy?'

DS: Oh, well, you know, dialysis was, and kidney failure was pretty primitive in those days and it was tough initially but after I'd done a few and I'd learned how to do it without too much discomfort, people would say 'Hey, it ain't so bad.' So we recruited fairly easily. I remember when we did the normals, we did 100 normals and um, we did two biopsies in everyone who would let us do a second one. Only one normal person turned us down for the second biopsy. I said, "Look, we agreed to pay you 200 bucks - I've done one, if you don't want to have the second one, we'll quit now." And he said, that's okay, not a big deal, and most of the dialysis patients, like at the kidney center, I probably biopsied 400 patients over there.

DWM: Were they volunteers? You asked them?

DS: Well we asked them, yeh, they were, they weren't, every now and then we did a biopsy for cause. Some doctor wanted to know what was going on but, and of course they signed a consent anyway.

DWM: Sure.

DS: But, yeh, most of them were volunteers in research projects. I remember one patient says, I saw her at a social meeting, she said "Remember me Dr. Sherrard, I'm Esther Kilcup." And I said "Yeh, I remember you well." And she said "I've been in four of your studies." And I think four of mine and five of Joe's. She was in the EPO trial, the first EPO trial, too, at any rate.

DWM: Well those patients were so critical to what we've learned over the years.

DS: Oh yeh. It is very hard to get bone biopsies now. Patients are really, even though I don't think, I think I know how to do it pretty well and it's not anymore painful, people don't volunteer as much as they used to.

DWM: Yeh. So we were talking about PTH assays, when did all the vitamin D interest come and what were you thinking?

DS: Well that's an interesting story. Jack Coburn and I were at this, again at this, I think it was the same meeting at which I presented the first analysis of renal osteodystrophy.

DWM: So maybe 1971?

DS: Somewhere around there. In Carmel. That's a great place to have a spring meeting, and we were on the beach at Point Lobos, which is a state park, and Tony Norman is a biochemist from Davis, and we were sitting down there having lunch and he said, this was probably like, maybe a little later than that because actually in '72, Kodicek first identified that the kidney made 1,25-D and so then we knew what was part of the problem anyway, but in '74 or '75, Tony Norman was having lunch with us at the park on the beach and he said, "You guys, I've just synthesized 125 dihydroxyvitamin D. Do you have any use for it?" Yeh we said we have lots of uses for it and so he arranged, I don't remember what, I don't think FDA was involved with anything in those days but he would send us 1000 mcg of 1,25-D in 100% alcohol solution in little vials. I remember handing a vial to Esther Kilcup and I said "Here's your medicine. Here's a syringe. This is $10,000 worth of vitamin D so be careful. She's reminded me of that several times. "Ten-thousand bucks. I was shivering in my boots."

DWM: So when you handed Esther that vial, what did she have to do with it?

DS: Oh, it was oral. She took it orally. We required a bone biopsy for people who wanted to get this new drug. We knew what to use it for, of course. We knew that it would, we had no idea what was causing the osteomalacia in our dialysis patients. We knew that hyperparathyroidism caused the osteitis fibrosa, so we were totally out to lunch in terms of osteomalacia. But you know, it looked just like osteomalacia in vitamin D deficiency, identical from all we could tell, so we selected people with osteomalacia to treat with 125-D. Guess what? They all got worse. Fortunately in the early cases, Jack and I threw in a couple of patients with hyperparathyroidism, just by chance and guess what? They just responded dramatically. So we wrote a paper on that first and then we wrote a paper on vitamin-D-resistant osteomalacia, which turned out to be aluminum toxicity. So that, that we described later but we didn't know what it was when we gave these patients, we thought, you know, they are D-deficient, they aren't making active vitamin D, they must be deficient, so we'll give it to them.

DWM: Right.

DS: And what it did was probably increase bone resorption, which didn't help and it didn't mineralize, didn't help them mineralize anything at all. At that point people thought vitamin D

was involved in bone mineralization. It isn't. We know that now, but we assumed that the active form was deficient and that's why they had osteomalacia.

DWM: I think I read somewhere in your talking about these patients where you did the first vitamin D trials and you had mostly osteomalacia patients but you had a few osteitis fibrosa patients, that the osteitis fibrosa patients *really* got better.

DS: Oh yeh. It was dramatic! I wrote a, do you do the nephrology bulletin board with the Canadian guy? There was a big controversy in the United States. Well this is how it all got started.

DWM: Yeh, so tell me that. Tell me how it, that all got...

DS: Well we thought it was for osteomalacia and we selected a bunch of patients with osteomalacia and just threw in a few and it was really dramatic and we then did a controlled trial, which ah, the fellow lost the data. I'm not sure he really did because some funny things happened with that data, but it was very effective in people with hyperparathyroid bone disease. Gosh, fibrosis, the normal bone volume is about 20%. Bone takes about 20% of the space in the bone marrow and the normal fibrosis volume is 0. Some of our patients in this study had more fibrosis than bone and I still remember Robert Smith who had really severe bone pain and if you've done a parathyroidectomy in a patient like that, you know that the bone pain goes away in a few days. And his bone pain went away within a week. And his repeat biopsy, he had 14% marrow fibrosis, was 0.5% in two months. It was just dramatic and the symptoms were dramatic, too. Though I said in the, there was an article in the Annals in vitamin D use in renal patients and how it doesn't seem to make much sense and doesn't seem to do much. Well that's because we're not using it in people like we originally did. We're using it in everybody and I'm not sure it's really needed in everybody. Abbott has convinced us that we need to use it in everybody but ah, but the um, patients who used it and were really 4+ hyperparathyroid bone disease and they just did as well as people with parathyroidectomy, even better in one sense because they didn't have hungry bone syndrome.

DWM: Right. Well I think this did come up because the controversy is, you know, how did we get to this point where we are using vitamin D?

DS: So liberally. Yeh, and I agreed with the article in the Annals that, you know that said, you know there is not enough data. We need a randomized controlled trial. Well we did one long ago, after the first pilot studies but the um, the frankly this is conspiracy stuff now, so I don't know what you think about conspiracy theories, but the fellow actually sold the data to some, to a drug company and I know that because the drug rep came in my office one day, started showing me this data and I said "Where did you get that?" And he said "Oh, I got it from Dr. Arnie Brickman, actually." And I said "How did you get it from him?" And he said "Well we hired

him to do these studies for us." And didn't at all. I said "That's my data." And Arnie had sold it to this company and he never could find it when we tried to, we had 100 patients randomized to vitamin D and placebo, so it was a good study. In fact, the results were pretty good but he lost the, he lost the key that identified placebos or controls, so...

DWM: But it, and it...

DS: At any rate, it was a dramatic study...

DWM: Yeh.

DS: ...but in fact we never published it because we couldn't get the data...

DWM: Data together.

DS: Jack Coburn and I basically organized it and Arnie was the key.

DWM: Right. As we were talking about vitamin D though, I guess I don't want to lose sight of the fact that these patients that truly were, um, had osteitis fibrosis early on, obviously significant bone disease, did respond quite well to vitamin D.

DS: Oh yeh! Dramatically!

DWM: I don't want to lose sight of those people who really...

DS: Yeh, so there is a role for it, and I think Abbott has really pushed well beyond any data.

DWM: That empirically that...

DS: That treatment, you know...

DWM: ...more is better.

DS: FDA bought their argument that PTH is a good surrogate marker for this disease and it's a, certainly helpful, but I think they've gone overboard. Treating people to get their PTH from 350 to 250. There ain't no data to show that we should do that. And we, why did we settle on 100 to 250 as a PTH for these patients? It was based on our original data but what we showed was that people had normal bone formations with that PTH level and is it bad for people to have slightly elevated bone formations? Probably not. Or slightly low bone formations? Probably not. It's probably bad if they have two- or threefold increases, which is what you see with PTH, intact PTHs of 1000 or so. But 300 to 500, you know if the main disease, problem with 300 to

500 PTH is? Usually progresses to worse levels. No problems at all with that number and if you keep them there, you'd probably be just fine.

DWM: Be okay? What do you think about surrogate markers like PTH, compared to bone biopsies?

DS: Well you know, the FDA is now backing away from surrogate markers for most, for many diseases. PTH is a good indicator of patient care and I think we should probably try and keep it in a reasonable range, but you know, how aggressive do you need to be to do that. If you start early, probably not as aggressive as we have to be when we ignore the patients for years and then start trying to fix them.

DWM: To catch up. Yeh.

DS: Yeh, ah, and if the patient ah, ah, in the early patients with secondary hyperpara, they have decent vitamin D receptors on their parathyroid tissue, whereas the tertiary patients, they have almost no D receptors, so no surprise they don't respond very well.

DWM: Yeh. Is there a role for bone biopsy today, you think?

DS: Ah, only in research. Not in clinical care. And um, it's still difficult even to get patients, to get patients to agree to bone biopsies. I've done several studies in the last few years in which we couldn't recruit, patients wouldn't agree to bone biopsy. We did a study using pamidronate to prevent bone loss in dialysis patients and the study was designed for pre- and post-treatment bone biopsies. We only got, I think 10 out of 100 patients to agree, which, so we just abandoned the bone biopsy. It wasn't worth doing it.

DWM: Hm.

DS: And I don't know what the climate is that's changed but invasive procedures are not liked by patients.

DWM: No! I think that's true. I did actually see. I don't see all the NephroL communication, but I did see the NephroL communication about this paper and the controversy about vitamin D and your response to the historical position...

DS: Oh, oh,yeh.

DWM: ... and so I'm glad to hear that, as we were recording today. But in your response to the vitamin D controversy, you noted that the relief of bone pain and the disappearance of marrow fibrosis was dramatic, and ah, you said that you likened it to what Paul Beeson said about the

first use of penicillin to treat pneumococcal pneumonia that "You didn't need a statistician to know that it worked."

DS: Yeh, right.

DWM: As you look back on some of your, I mean certainly the early days of dialysis and Scribner, and then your early days in treating bone disease, what do you think about those observations, the value of observations like that?

DS: Well you know, we didn't even know what a randomized trial was in 1965 but ah, most of what we do is based on observations and I think there's a place for it, but clearly randomized trial are the gold standard today. More things should be properly done but they are still not done very often or very well ah, as you probably know.

DWM: Well, it's true and sometimes they are biased from the outset.

DS: Start. Yeh, or the, or just crazy. There was a recent study of prostate cancer in which they randomized people to one treatment versus another. The problem was, everything was good except they studied people who were 30 to 45. Prostate cancer doesn't occur essentially in that age group and they had no blacks in their study. So, you know, what does this mean? It doens't mean anything.

DWM: Right. I do, I have read over the last few months that Scribner felt at some times that the things that you all learned early on about dialysis, that some of the innovations really came from observation, and that...

DS: Yeh, like the number of hours on dialysis. I mean we didn't do a controlled study.

DWM: Right.

DS: We just ah, recognized that patients were not doing well when they had wide chemical shifts during dialysis and ah, most of the early observations are what determined the course of dialysis for much of the first 20 years probably. There was some interest, interestingly ah, ah, the FDA made noises about doing a randomized trial of dialysis in the '60s, and ah, Scribner said to them, "Randomized trial? You mean you're going to not dialyze some people with kidney failure?" And they backed away from that.

DWM: Yeh, but also that the climate in the 1960s and 1970s, which allowed progress by observation, allowed people to very freely talk about what worked and what didn't work, what was a mistake and what seemed to work very well, allowed a very impressive time of innovation.

DS: Yeh true. And in addition, there was a lot of leverage favoring the physician investigator then, because if we didn't dialyze somebody, they died. So you know, whatever we decided, we weren't going to be held libel for, if something bad happened because the alternative were that the guy wouldn't be alive anyway.

DWM: Yes. Was there a pretty good climate of talking about mistakes made, and...

DS: Ah, I don't, certainly there was some. We didn't hesitate to question ah, what we did. I'll tell you a crazy experiment that Joe Eschbach and Jim Burnell - do you know that name?

DWM: I've heard the name, yes.

DS: At any rate he is still around but not doing anything. They cross circulated some people with halothane hepatitis with some patients who had been refused dialysis, so nobody died in that experiment. I think they did five patients and the halothane hepatitis patients had good kidneys. The renal failure patients had good livers, so they treated each other. All the patients who participated, all the renal failure patients who participated in that were subsequently put on dialysis, but they weren't offered anything initially, just you know, we can do this, keep you alive a little longer probably. Amazing.

DWM: Amazing, yes.

DS: You couldn't do that today. I mean there would be 23 investigators from FDA out there telling you, or from what's the other, what's that crazy organization that stopped the ah, the ah, spreadsheets in ICUs? You know about that?

DWM: Oh, like the Joint Commission?

DS: The Joint Commission, yeh.

DWM: Yes. Yeh. But you know it does seem like there was a time where like Scribner could think about Teflon and then use Teflon.

DS: Do it.

DWM: Do it.

DS: Yeh. Yeh. The alternative was if we didn't figure out a solution the patient died, so you know, nobody complained about all this crazy stuff we were doing here.

DWM: But look what it's led to. I mean it's a, it's an amazing thing. I do want to just, as we've talked about a lot of names here today and I wonder if you could just talk about who, as you were beginning at the time in the 1960s and early 1970s, and as you look back on it today, who in your mind were the heroes, and the pioneers, from a physician/scientific investigator?

DS: Well I think Scribner, obviously. Wayne Quinton was an extraordinary technician. He's still alive and he sold his company, Quinton Instruments a few years ago, for multi bucks. He was the head technician at the university medical school. He, I don't know, are you familiar with the Bruce treadmill?

DWM: Yes.

DS: He built that.

DWM: Ah.

DS: Bruce, ah...

DWM: Protocol, the Bruce protocol?

DS: Bruce protocol. Robert Bruce was a cardiologist here and Quinton built his first treadmill, which has staged, and he did the Scribner shunt and he also made all the Tenckhoff catheters originally and all the Hickman catheters. Ah, but he, he developed a way to bend the plastic. The plastic wasn't as flexible that they used for the shunts initially and they had to bend it under, over a Bunsen burner and then connect the tips and coat it with Teflon. It was, it was a miracle.

DWM: Yeh.

DS: So Wayne Quinton, um, ah, Jerry Pendras, who was the first head of the Northwest Kidney Center.

DWM: Spell the last name.

DS: P-E-N-D-R-A-S. Great guy. Ah, died of brain cancer about 20 years ago now. And of course, Joe Eschbach.

DWM: Tell me about Joe Eschbach.

DS: Joe. Joe was an incredibly industrious guy. I mean he, he, he had a full-time private practice and he did his research in a little corner of the animal lab at the university. And they, he, I don't

know what he started exactly with, but Scribner basically said, "You've got to figure out this anemia thing, Joe. That's your assignment." And he did!

DWM: Yeh.

DS: But he started out early in the game dialyzing sheep. He put sheep into renal failure and then he made other sheep anemic, collected their urine, isolated some factor from the urine, he didn't know what it was, and gave it to his sheep that were on dialysis, and it corrected their anemia. That was sort of the, I think, the key experiment in EPO. As an aside, in I think it was late '70s, early '80s, in an NIH grant that he wrote, a reviewer said, everybody knows that EPO has nothing to do with the anemia of renal failure and his grant was declined. He has shown that letter in a slide every now and then.

DWM: In the great wisdom of...

DS: In the great wisdom of the...

DWM: ...the NIH.

DS: ...NIH expert.

DWM: So, Joe Eschbach, who is taking care of patients, practicing nephrology...

DS: Yeh, full-time practice.

DWM: ...is writing grants for the...

DS: Is writing grants, is doing research at the university, ah, it was, I don't know, and he has three kids. I don't know how he had time. And he's got a beautiful, had a beautiful, has a beautiful, his wife is still a beautiful person. Ah, but ah, he was just, worked far harder than I ever did. The other unsung hero is Henry Tenckhoff.

DWM: Yeh. Tell me about Henry.

DS: Henry Tenckhoff, you know, invented peritoneal dialysis. Ah, he started doing peritoneal dialysis by using temporary catheters. Have you ever seen a temporary peritoneal catheter?

DWM: Just that when I was training, we had an acute catheter that you, you inserted the trocar and then the catheter through it. And of course...

DS: Right.

DWM: ...the catheter was soft. It was not hard...

DS: Yeh.

DWM: ...but it would, it did go through a metal trocar, you know.

DS: Right, and he, in his initial peritoneal dialysis program, he had 40-liter bottles of dialysate, two of them, that were delivered to the patient's home. They did this at the home, and he went around. He had half a dozen or a dozen patients on PD at home, and put in a temporary catheter for each dialysis.

DWM: So how often was he putting in a catheter for them?

DS: Three times a week.

DWM: Three times.

DS: And he had half a dozen or a dozen patients he was doing that with. And he soon realized that unless he figured out something different with the catheter, this wasn't going to be a viable technique. Just too labor intensive, not to mention potential complications, so he thought up the cuffed catheter, the Tenckhoff catheter, and he didn't put it in his peritoneum, but he put it in his abdominal wall and wore it for six months.

DWM: So he had someone implant a catheter?

DS: Yeh, a catheter of his design and it just hung out of his belly. He went swimming, did all kinds of stuff.

DWM: And he decided well it worked okay?

DS: It worked okay and I don't know when he decided to put it in the first patient, sometime in the late '60s.

DWM: But his idea was that if he could implant this catheter that was partially in a tunnel under the skin, that it could stay in.

DS: It could stay in, and so he did it to himself first. Henry was the ultimate nice guy. He was, he was unbelievably nice. You couldn't, as a fellow, when you first meet this guy and you think he's a phoney, he's too nice. Nobody is this nice, but he really was. He was just an incredibly nice

guy. Always there for you when you were a fellow. So he was another hero. I didn't know Fred Boen. He was before my time, but he worked with Henry in the development of initial stuff.

DWM: And their thinking, do you recall anything about their thinking about early PD. I mean, how did they decide that this was a good plan, to put fluid in somebody's peritoneal cavity?

DS: I don't know. Hopefully, maybe you can track down Henry and ask him, or Fred Boen is still alive and lives in Holland. That would be worth asking, but it was all there by the time I got there and Fred, Henry developed an automatic cycler, which used, initially used his, these 80-liter jugs of dialysate that he delivered to people - 40-liter jugs, two 40-liter jugs, and they would be elevated on a stand in the patient's room, so they'd drain from the jug into the patient's abdominal cavity and then, then ah, as I recall we used to have generally a five-minute inflow ah, depending on how much fluid we wanted to remove, 15- to 40-minute dwell time, and a 15-minute outflow so we did very frequent dialysis.

DWM: Well it's certainly an early solution to chronic outpatient dialysis, you know, to a certain extent.

DS: Yeh, yeh, and he only initially, he developed this - he thought it might be cheaper than hemodialysis and less technically a problem. So he started doing it, I never really asked him, but as I understand it, for patients that had been turned down for hemo, for the hemo program. They had no options.

DWM: Oh.

DS: So he said okay, well let's try this. And it worked.

DWM: Absolutely.

DS: And the cyclers since, are basically just modifications of his original cycler. CAPD was done somewhere else. It wasn't invented here. I think what's his name, in Missouri, I think it was...

DWM: Ah, Karl Nolph.

DS: Karl Nolph is the inventor of CAPD, but the cycler, Henry put together. But ultimately he was just the nicest guy you can imagine. I don't know, who's the nicest guy you ever met? Henry was that guy.

DWM: That guy. Great.

DS: As I say, I thought he was a phoney at first. He was so nice. Nobody can be this nice. But he was sincere and you know, you'd call him up, even if he wasn't on call and he'd come in and help you out with something. He'd let you know that if you had a problem, he was there and if you couldn't reach anybody else, he'd be glad to come in and show you what to do. He was a great guy.

DWM: Was he, in addition to his peritoneal dialysis, helping with other kidney patients as well.

DS: Yeh, he attended on the nephrology service and took care of hemo patients, and you know, knew as much as anybody about hemodialysis, but he developed this technique for basically initially for the patient who were turned down for hemo and adapted it to acute, too of course, fairly quickly.

DWM: Sure. Absolutely. Anybody else you can think of that...

DS: I think those are the, Joe, and Scrib, and Jerry Pendras, and Henry Tenckhoff. I think those are the...

DWM: We mentioned Clyde Shields before and this has come up as I've talked to people, that they had these patients that taught them a lot, or that they really remember. Clyde Shields sounds like somebody for you that you learned a lot from.

DS: Yeh I, well, in those days the access was the Scribner shunt and I learned all about the Scribner shunt from Clyde Shields. He said, "Okay, Dr. Sherrard, you've got to get some heparin and flush that out." And you know what we did, we flushed out clots from the Scribner shunt and you know, you'd squirt some heparin in and what would happen next, the patient would cough. So you knew damn well you were giving them a pulmonary embolus every time you did that. He says, "Well if I cough, I know you got it out of there." He was a real laid-back guy.

DWM: Yeh, and it certainly sounds like he was willing teacher for people and for...

DS: Yeh, yeh, and he was you know...

DWM: ...the politicians.

DS: ...he, he, I'm pretty sure - don't know this for a fact - Chris will probably know, that Clyde invented the bubble technique. Do you know about the bubble technique?

DWM: No!

DS: To know if your shunt was working okay, the nurse would inject, or the patient when he was at home, would inject a bubble into the blood line and see how quick it made a 1 meter distance, and you know, if it was above a certain number you knew you had a good, your shunt was working well.

DWM: It was a crude measure of flow.

DS: It was a flow measure, yeh.

DWM: Yeh.

DS: And I think Clyde Shields invented the bubble technique for measuring flow, and there is a patient in here that talks about his bubble time, on this tape. Because the home patients did it to monitor their shunts.

DWM: Sure. Sure. Were there any other patients that you can remember that were...

DS: Well Jim Albers was really neat.

DWM: Spell his last name.

DS: A-L-B-E-R-S. He was the physicist.

DWM: Right.

DS: And um, Nancy Spate is a really impressive - have you met Nancy?

DWM: No.

DS: She goes around all over the country and gives talks. She actually started dialysis in '64 and she's, I don't know what the absolute mix is, she's had four transplants.

DWM: Oh.

DS: She still works full time and her story is remarkable. It was in the D&T, ah, actually ah, Eberhard Ritz asked her to write up a short bio sketch, which she did. But she talks to the fellows here all the time. She's an impressive patient. My first patient here was John Street, a big black guy who couldn't read. He was illiterate but just a, you know, he was the guy who said, he was another one of those guys like Jim Albers who said, "Doc, if you told me to stand on my head for 30 minutes, I'll do it." And you know he started dialysis when he was 54 and he died after 21 years, so he did very well.

DWM: Pretty good. Yes.

DS: Yeh, but like, he did, you know, he bent over backwards to, he figured you know, this is was his second chance at life and he wasn't going to blow it.

DWM: Yeh. You know um, a little bit of difference also between the west coast and the east coast, I think, is that the, certainly early on it seems to me that the east coast folks, the Boston folks, really saw dialysis as a bridge to transplantation. The whole goal was to transplant patients and that they thought that was a better outcome. I mean..

DS: Well it is! No doubt! But a lot...

DWM: Some, sometimes it is. Sometimes it's complicated.

DS: Well I think for most patients it's a better outcome, and I don't disagree with that. Although we had great fights in the early days of ASN, about you know, a transplant surgeon would get up and talk about the evils of dialysis and people would applaud. It was just craziness. Ah, but you know, in the early days, transplant was a disaster and it is sometimes now still, but everybody lives longer with a transplant.

DWM: So what was the thinking in Seattle about, you know in those mid-1960s, the role of dialysis in relationship to transplantation?

DS: Well transplant really wasn't available in Seattle. The first transplant was done. The first two failed, so that didn't impress anybody.

DWM: Oh.

DS: Ah, I can't remember when the first success was, probably not until the early '70s, though and that guy, what was his name, Murray, in Boston?

DWM: Yes.

DS: He did those twins in '66, didn't he?

DWM: Yeh, either the last '50s or early '60s. Yes, they were doing the twin transplant.

DS: But you know, there was no possibility of transplanting unrelated donors or even related donors, except for twins. It's still a problem obviously, but ah, but there wasn't much of a controversy until I would say transplant really got up and running in the late '70s, in terms of

becoming a competitive potential, but even then, immunosuppressive treatment was pretty grim and we, you know, for ideal cases, we clearly ah, recommended people for transplant, but we were very restrictive. Of course, we didn't do diabetics on dialysis either, so...

DWM: Right.

DS: ...they didn't get transplanted, but it was, we recognized it as a bridge to transplant in the patients who were appropriate but we were pretty conservative about who was appropriate for transplant.

DWM: Right, well and it sounds like it was just so much later coming here and being successful here, so...

DS: Yeh.

DWM: Do you, did you participate early on in a lot of the organizations, like the American Society of Artificial Internal Organs?

DS: I wasn't very involved with that. I mostly went to ASN. Scribner was very involved in ASAIO, and I went to some meetings, and presented one of my first bone papers at an ASAIO meeting. I can't remember when that was, 1969 or '70. It was here in Seattle, so...

DWM: Oh, okay.

DS: ...whenever that was, I presented a bone paper there.

DWM: And how was the ASN. I mean do you remember the early ASN meetings and what were they like?

DS: Well, it was a very small intimate group. I mean 1000 people was a big meeting and you know, I remember, I still say this to renal fellows, when I first started going, I could walk in any room in an ASN meeting and know what was going on. Today, you know, probably 10% of the presentations, I can have some inkling about what's going on, and it's grown obviously. You know, 1000 people was a big meeting in the early days and it was interesting. The early meetings were smoke-filled rooms. All the senior docs were heavy smokers. Ah, I've noticed that over the years. Hardly anybody admits that they smoke now, much less...

DWM: Right, well and they're not smoking in the buildings.

DS: They're not smoking in the building for sure. I need to go and drain my coffee again.

DWM: Oh sure. Finish with my questions...

DS: Okay.

DWM: ...and when we're finished, we will stop recording and turn on our video here, so just a few more things. We were talking about the ASN.

DS: Oh yeh.

DWM: And the ASN being very early on small, and very scientific, I gathered.

DS: Pretty scientific, yeh. But it had both clinical and basic science research programs reported. All the early focus was pretty bench-oriented science. Not a lot of clinical studies, um, and there was kind of a, I think, less enthusiasm about clinical studies, clinical trials and that sort of thing. We needed to know more about the function of the tubule.

DWM: Right, than dialysis.

DS: I think, as you know, ASN grew out of the childhood nephrotic syndrome. At first, it was the nephrotic syndrome society first, I don't remember the exact name of it, but it evolved into ASN, and then the people like Gerhardt Gebeisch and ah, who is the guy in Boston, the physiologist, Jordan Cohen worked with him, I know.

DWM: Ah, yes.

DS: You know, they, after, they sort of focused on the tubule for many years and I guess that's why my research, my mentor here was, and I were doing studies of glutamine. tubular metabolism of glutamine. So that was the 'in' thing to do in those days.

DWM: Yeh. And it does sound like, you know, again the clinical people took a little bit of a back seat, Scribner and some of the clinical people who were...

DS: Yeh, yeh. Definitely.

DWM: Yeh. And how about the National Kidney Foundation?

DS: Ah, I, ah, that came along sort of subsequently, as a basically patient-focused group. Ah, ah, but a lot of important people worked with the National Kidney Foundation, ah Glassock and Schreier, and Massari, were all presidents of both NKF and ASN. I think, was Michelle president of ASN? I'm not sure. President of NKF anyway. But that was a clinically focused group and that, I think grew out of patient organizations, renal physicians' association to a great extent,

because the patients felt kind of ignored at ASN, and they were. Ah, so they, I think they really pushed the National Kidney Foundation into existence.

DWM: Okay.

DS: But Bob Schreier would know better than I do.

DWM: Okay, I can talk with him maybe, too. Um, also, you know talking about things that really impacted the practice of dialysis, it certainly sounds like when the Gottschalk committee did their work and the government finally weighed in, in 1972, and passed the public law that funded the end-stage renal disease program, that it did ripple down to influence the practice of nephrology and the practice of dialysis. Do you remember that time and did it change what you were seeing?

DS: Well we were, I was at the VA so that we never had a funding problem.

DWM: Well let's talk about that because the VA early on had demonstration projects and...

DS: Yeh, and our project was kind of, was started without permission here at the Seattle VA. The guy that preceded me, King Curtis, Kingsbury Curtis, was a fellow and then he came over to the VA, and the VA director here wanted to have a dialysis program, so I think it was one of the first five or six in the VA and was unauthorized when they started it. But once it got going, they couldn't stop it.

DWM: So funding was pretty easy for you. Why was that? How was that, that it happened that you got good funding through the VA?

DS: Well, the VA decided - they have various funding mechanisms, but the original dialysis program was a "special program" and what that meant is that you send in the bill and they pay it, no questions asked. That resulted in huge boondoggles in the VA. For instance, we had 15 lab techs, two chaplans and four surgeons on our dialysis budget. We sent in the bill, they paid it. Richmond, Virginia was the worst. I was the head of the VA dialysis committee for a while and they were going to close the VA dialysis program because it was so costly and it was costly. Richmond, Virginia had 31 lab techs on their dialysis program and reams of other people, too. So, you know, dialysis was costing maybe in the mid-'90s, when we did the survey when they wanted to close the VA dialysis program, dialysis was costing you know, like $60,000 a year per patient in the community, including everything, hospitalizations, etc., was costing $140,000 in the VA per patient per year. But, it was this boondoggle. My hospital director, when I said "Look, you know we have all these people on our dialysis program that have nothing to do with dialysis and we're paying for them." He accused me of micromanaging his budget. I said "Well I don't want to micromanage your budget. I just don't want to pay for all these people that aren't

doing anything in dialysis." And I wrote, there was a big commission in the VA to close the dialysis program and I wrote them a letter, Saul Klar and I went and talked to the VA director and said, "Look. This is what's going on. These people are ripping you off." So then they changed the program to ah, not an automatic pay the bill program but you had to justify what you were doing.

DWM: Why do you think the VA was so agreeable early on, to dialysis?

DS: I don't know. It was a sexy program in those days, ah, and they were interested in being an innovative group and so they saw dialysis as a way to kind of up, increase their image. As you may recall, in the '70s and '80s, there was lots of talk about closing the VA system because it was duplicative and inefficient, and medical care wasn't very good and, none of which were really true, but it was duplicative, that was for sure. I mean, we had a dialysis program here of 100 patients and down the road, the kidney center had 1200 patients. That doesn't make sense really. Ah, ah, so you know, a lot of people thought we ought to close our program and put them all over at the kidney center, but not, none of the veterans thought that. At any rate, there was significant duplication and it looked like it was terribly ah, inefficient in terms of cost, but it was all paperwork.

DWM: People just bundling together.

DS: Bundling together. Today the VA marrow transplant program is a special program and they've learned, since we were there, so they don't get to do all that stuff. They're still well funded and they pay the bills that come in, but the bills are screened by our fiscal officer before they go in, for accuracy, so it's not quite as liberal as it was long ago.

DWM: Was. But it is an interesting phenomenon and I've heard it before, that at times when there is the Northwest Kidney Center, and it was not called that I don't think at the time, but you know trying to find funding for individual patients, and yet the VA was forward-thinking enough that they were already supporting dialysis.

DS: Yeh. I don't know. They have had other special programs at the VA, which generally have worked out pretty well. Um, the spinal cord injury program is another special program in the VA, which probably is, I think, probably ah, leading the country in terms of spinal cord injury research. They do a lot of innovative and new things.

DWM: Yeh, it certainly is quite a compliment to the VA, because well before this was really accepted and funded by the rest of the government, I mean, they recognized the value of it.

DS: Yeh, and you know, the VA is run like any sensible first-world country, in terms of healthcare. Ah, people come in, they get taken care of and it costs less than the private

community. I'm reviewing, a sideline, drug costs in the community and the VA. You know what hydrochlorothiazide costs in our community?

DWM: No.

DS: A 25 mg tablet costs 30 cents a pill. The VA - half a cent a pill.

DWM: I was going to say, it should be really cheap.

DS: It is. It's half a cent. That's what the VA pays for it and that's what they charge patients, although most patients don't pay anything except their co-fee. They get ah, I think it's $20 a month per prescription, up to a maximum of $200 a month. I mean, $20 for three months. They get a three-month prescription. That's their pay. They get billed for it. If they can't afford it, they don't get billed. But if they can afford it...

DWM: That does make great sense.

DS: But if they can afford it, they get billed up to $200 for three months.

DWM: $200. Yeh.

DS: But it's just remarkable. Ah, ah, Fosamax is ah, 5 cents a pill at the VA; it's $1.10 outside.

DWM: Well this does bring us to talk a minute about...

DS: But that's a different issue. We're not here to talk about VA funding and healthcare funding.

DWM: ...well but it's true. It does bring us to talk a minute about pharma, an industry, and drug companies and clinical research.

DS: Yeh.

DWM: Sort of, maybe thinking back even to the late 1960s and 1970s, were you getting support from the drug companies and what was industry doing to support research?

DS: I mostly got support from VA and NIH. Did I get, I got some money from um, some companies. Not a large amount. It was interesting. A couple times I submitted grants to the NIH, which were turned down because industry should pay for this because they are the ones that are going to benefit.

DWM: Yeh, I've heard that before. What was, what was the...

DS: And, and industry generally wouldn't pay for it. I wanted to study bisphosphonates in dialysis patients and the NIH said "Industry should pay for that. We aren't going to learn anything from it and it's not going to help patients." And I said, "Okay, all our patients are breaking their hips." And we've reported that several times now. And so then I went to industry and they said, "Well you know, dialysis patients are too complicated and we're making tons of money on osteoporosis. We don't need that million dialysis patients and worry about all the liability that we'll get from that." So they wouldn't fund it either. I eventually got the VA to fund it but it was kind of weird to get this runaround.

DWM: Yes. Um, the NIH early on, did they have interest in some of the bone disease research you were doing?

DS: Not a lot. They did, Jack Coburn that I, who I collaborated with a lot, got a fair amount of NIH funding, but I didn't get much for the bone work. They didn't, it was, you know, it was too, it looked like it was too clinical. You know, we weren't answering any basic science questions, and NIH, well there are two problems with NIH and clinical research. One is there was a bias against it for a long time, I think. My personal conspiracy theory again. But secondly, ah, clinical research wasn't done very well and for a variety of reasons. I mean you can easily define a biochemical or in vitro project where you grow some cells, and do things to them, and get interesting results. Whether they mean anything or not, who knows. But it is very tough to get, to design a very tight clinical trial and, and it costs a fortune to do any decent clinical trial and NIH has done badly with a couple of large renal clinical trials, so they're not real hot about doing clinical trials.

DWM: Yeh.

DS: Actually, and there is some reason for pharma to support it obviously but of course, then they want to tell you what to find.

DWM: Right.

DS: I had that happen at least once. We found, we did a study by a company who interestingly had Slatapolsky do the PTH assay, I did the bone biopsies and somebody else did the vitamin D assays, and they coded all the substudies differently. We knew where the study centers were, but the patient codes were not the same as the lab codes, so the company didn't like the results and they wouldn't give them to us. So I had bones, but I didn't know who were controls and who were treatment. Same with Slatapolsky and he didn't know, his coding was different from my coding, so I couldn't even put his PTH together with my bone histology, and the same guy, the guy who measured the vitamin D metabolites had the same issue, and the company would never send us the key.

DWM: So therefore never published it.

DS: So we never published it. Because they didn't like the results, so they just buried it.

DWM: Great.

DS: I got funding from them for a while, but no paper out of it.

DWM: No. No progress.

DS: No progress.

DWM: Yeh.

DS: That's a study that still needs to be done but it never will be.

DWM: Well we've talked quite a while now. Is there anything else you can think of that we have not talked about that, that you think 'gosh, this is something I really think in this setting I want to mention,' or...

DS: Ah, well you know, I just can recall Scribner saying and some other people in other subspecialties, "You know, if we had the FDA and IRBs around when we first did this, we wouldn't have ever done it. They wouldn't have allowed us to do this." I think he's right.

DWM: Right. And I also can only imaging the courage it took to take care of these patients who...

DS: Well, not courage but in a way, you know, Scribner was bent out of shape by that patient who died, that I first told you about, and he often referred to that patient in lectures he gave. This is how it got started, and I don't think he had a lot of questions about where to go from here, whether we should do this or not. I think he thought, you know, if your disease is 100% fatal, and you reduce that to 50% or 30%, I mean, who can argue with that.

DWM: True.

DS: But HIPPA would and NIH would.

DWM: But also Henry Tenckhoff who says, you know, I believe I can implant this catheter and I'm going to implant it in myself and watch for six months and that looks pretty good, you've

still got to take that first step and saying, you know, I'm going to try, I'm going to offer this treatment and I'm not certain about the outcome of it. This may be a bad thing.

DS: Yeh, but you know, his big concern was infection. He said, that's you know, when we see infection with temporary catheters and it's just a short time, so we need to know, so he invented the idea of the cuffed catheter. I mean, lots of cuffed catheters out there now, but they all started with Henry.

DWM: Henry. But taking that step to say, I have an idea, I can do some things to try to make as much of an effort to say I think this is going to be successful, but you have to make...

DS: I wouldn't be surprised if Henry put it in himself. I don't know that he did. But I wouldn't be at all surprised.

DWM: But making that step and then saying, you know, I've done the best I can and we're going to try it on a real live patient, you know, not really knowing or being certain about the outcome. It's easy from our standpoint today to look back and say, 'Yeh, that was a good idea.' But it must have taken courage to try that.

DS: Yeh, yeh. Right. And the Hickman catheter. Bob Hickman. I don't know. He's around. You want to talk to Bob? He's still putting in Hickman catheters.

DWM: Yeh, I probably do need to talk to Bob at some point.

DS: He's on contract to the oncology group here who uses Hickman catheters for all their marrow transplant patients and he mostly puts them in.

DWM: Wow!

DS: I don't know. My, I'm not sure I can do bone biopsy anymore. You know, I, my eyesight is bad and I get a little shaky with too much coffee, but Bob is still putting in Hickman catheters.

DWM: Amazing. Well I just want to thank you for spending your time today talking to me about nephrology.

DS: Oh it was fun. But Nancy Spaeth, you ought to find that article by her in the NDT.

DWM: I'll do that. All right, well I'm going to...

END OF DICTATION

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DWM/jhl
T: 03/2008