

OREGON HEALTH & SCIENCE UNIVERSITY

ORAL HISTORY PROGRAM

INTERVIEW

WITH

Grover C. Bagby, M.D.

Interview conducted August 28, 2014

by

Joseph D. Bloom, M.D.

Narrator: Grover Bagby
Interviewer: Joseph Bloom
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Transcribed by: Teresa Bergen

[Begin Track One.]

Bloom: My name is Joseph Bloom. I'm interviewing Grover Bagby for the OHSU Oral History Program. It's August 28, 2014. We're in the BICC building at OHSU. Grover, good morning.

Bagby: Morning, Joe.

Bloom: It's a pleasure to interview you, to have this opportunity to learn a little bit about your life and about your career. Why don't we start with some of your early life? Where you were born and a little about your family and where you went up and where you went to school.

Bagby: I was born in New Jersey. Summit, New Jersey. And that was because my dad was at theological seminary in Madison, at Drew University. And became a Methodist preacher during my early life. We lived in a little town on the Hudson River and little towns near Paramus until I was around seven years old. Then we moved to Southern California where I spent all my, the rest of my young life, including college. The move to L.A. preceded the move of the Dodgers by two years. And everyone assumed that I was going to be happy because the Dodgers were my team. You know, the Ebbets Field Dodgers.

But when my grandfather called and told me that the Dodgers were moving, I was in L.A. And he said, "They're moving to L.A. Aren't you happy?"

I said, "No, I'm not. I don't want them to leave Ebbets Field." And they just never were the same to me, anyway.

I was raised in East Los Angeles, mostly. Long Beach and Huntington Park. And I went to a public school and then went on to Pomona College, where I majored in zoology. And did other things at school and enjoyed it very much. I liked Pomona.

After that, I went to Baylor College of Medicine in Houston. And where I spent six years. One of them was difficult, because it was a pretty serious culture change. There was a lot of overt racism. The whole society was very, very different from Southern California. And I went to a school that was very heterogeneously mixed racially. And that obviously wasn't happening in the neighborhood I was in in Houston.

So I wasn't really that happy the first year socially. I think from the standpoint of the school, I loved it. It was great. But it helped me study.

Then the second year was different because one of the new freshmen in that second year was Susan Pound. She was one of three women admitted in the class of eighty-five people. And from the University of Texas, born and raised in Texas. And it was destiny, because we got together and got married in 1967. And Texas became much more enjoyable at that point. I learned about the good stuff that way.

We wanted to get out, though. She was interested in leaving and trying Yankee territory. She was a couple of years behind me, temporarily. And we left Houston to come to Portland after I had done a couple of years of medical residency in Houston, waiting for her to graduate. And we applied to a number of places in the north. And I was going to be a cardiologist. And she was going to be either a hematologist or a nephrologist. And we signed up to come to Portland, because it was the days of Dave Bristow and Frank Kloster and the team, and Albert was here during his heyday. And it was a big time cardiology place. So we decided to come to Portland. For other reasons, too. Friends of ours had lived here and said, "We know that you'll love it in Portland. It's just your kind of place."

So we came here in 1970. And apart from being in the Navy a couple of years, '72 to '74, we've been in Portland ever since.

Bloom: Where were you stationed in the Navy?

Bagby: In San Diego. Sorry. We lived in San Diego and I was assigned to the First Marine Division at Camp Pendleton Naval Hospital. And I thought it was going to be a wasted two years. I didn't like the idea very much, but I knew that it was just what I had to do, because I'd had a Barry Plan deferment.

And I got there and we had a group of nine internists. Five of them ended up on faculties of medicine after their reserve career in the Navy. It was during Vietnam.

And the captain, my boss, was really, really great. I mean, he protected us. He was academically inclined. He'd worked at these units called the Naval Medical Research Units, NMRUs, they have them in Egypt and Taiwan. They do a lot of tropical disease research. And he was a tropical disease expert.

So we had this family of people that were working together. And it was just great to be on that team. It was really a terrific time, because Susan was doing training at UCSD at the time. And she was really working hard. She was working nights and didn't have enough time to be Mrs. Mom. And I was Mr. Mom for a couple of years with our son Matthew, who was three months old when we went down there. And that was great, too. So I was totally wrong about what this was going to be. I loved it.

Bloom: So you were at Camp Pendleton. What did you do there? Were you a general medical officer?

Bagby: No. We had an internal medicine department. So I ran a medical ward with medics that were staffing it, and nurses. We had, it was an ICU. We had an ER. So there were general medical officers stationed around, and in the satellite clinics. And we had some there as well. And we had a department of pediatrics. We had a vascular surgery department. I mean, it was a big place.

Bloom: But you had two years of medicine training at—

Bagby: I had had two years of internal medicine. Well, no. Sorry. I had had two years at Baylor, and then a year, my third year here, and then one year of hematology. I forgot to add that part. When we were coming, when we had already signed up for Portland, to

come to do cardiology for me, or to prep to do cardiology, I had a rotation at Methodist Hospital, which was one of the teaching hospitals at Baylor, and a young, twenty-six year-old woman came into our service and had acute myelogenous leukemia. Three kids. Blue-collar husband. And in ten days, she was dead. And that really, really affected me. I really became interested in, “What the hell? What is this disease? What is it about biologically? What’s wrong?”

And I asked the person who was running the service this, the professor, whose name, Ed Lynch, one night. I was very depressed about this. And I was looking at the bone marrow of this patient after she died one night. And I turned to him and said, “Why are these cells that are obviously abnormal, why are they so abnormal? And why are they not turning into neutrophils like they should be?”

And he said, “Nobody knows. Why don’t you figure it out?”

So I made a call and checked around. And called Bob Koler and others here, and said, “What’s hematology like in Oregon?” at the University of Oregon, which is what it was, University of Oregon Medical School. So that’s when I “became a hematologist” interested in leukemia research. So I’d finished one year of fellowship and then went into the Navy. So I’d completed all my internal medicine training, so I was an internist.

Bloom: So you were an internist.

Bagby: Yeah.

Bloom: So you did come to Portland before the military.

Bagby: I did. And that was fortunate, because I became an Oregon vet.

Bloom: And how did that—

Bagby: It helped when we came back, because I was able to get a really pretty good loan to get our first home. Which we still have. We’ve only had one home.

Bloom: That’s good. So then you were in the military for two years?

Bagby: Yup.

Bloom: And then came back here—

Bagby: Yes, I came back at the time James Linman came from the Mayo Clinic to run the hematology department and was building a research laboratory in Baird Hall on the third floor. It was an old, rundown lab that had to be retrofitted. And that’s where I started doing bench research, sort of when I could. Because the second year was pretty clinically heavy.

Bloom: What year was that that you started the fellowship?

Bagby: This was in 1975, ’76.

Bloom: And at that time, what was Susan doing?

Bagby: Susan had done one year of a nephrology fellowship at UCSD. And then she came here to finish second and third year. So we were both second and third-year fellows on track now because of the frame shift that I had from the Navy.

Bloom: She didn't have any military obligation during that time?

Bagby: No. No. Although her duty was worse than mine.

Bloom: I imagine.

Bagby: Well the department was very cardiology-heavy. In fact, at the time, David Bristow was the department head.

Bloom: In '75?

Bagby: I think so. I'm pretty sure about that. In fact, I am certain about that. I remember, an important question I asked him about staying on the faculty. But, so it was a pretty small department. Lean and mean. He was a very admirable guy. He just was a truth teller and straight shooter and would tell you what you needed to know. And he was very productive academically. And he knew what it would take to be the faculty member for anybody who was interested in academics. Which we both were. Both of us.

George Porter was the division head in nephrology. And just at the time I came back, James Linman took over the division of hematology and oncology. Medical oncology was missing. I mean, we just didn't have it. Any chemotherapy that was going on investigatively, was going on in the department of surgery under Bill Fletcher. And it was terrific that he was able to keep that alive. Ultimately that became a choking point when we tried to evolve to "medical oncology; does chemotherapy and surgical oncology to surgery," but he, you know, Bill's commitment to clinical studies was the only reason we even showed up on the radar screen in those days in medical oncology.

But in hematology, we were stronger. Scott Goodnight was here at the time. And he ultimately took over from Jim Linman when Jim Linman retired to Hawaii. I can't remember the date on that.

Bloom: Was Scott already on the faculty then?

Bagby: He was. Scott had come up from the University of Southern California, where he had spent five years on their faculty and then joined us. He was born and raised in Portland and his dad was a physician here. He was, obviously, he was a friend, a good friend. And he did a terrific job holding together a small division where we had a couple of medical oncologists. But he was the one that did all the hemostasis and thrombosis. And he was an outstanding teacher. He would get the teaching awards every year from the students. He was a great leader when he took over for Jim Linman.

Linman's interest was in pre-leukemia, the myelodysplasia. It's a condition that leads to leukemia very often. And that's how I got started, focusing on myelodysplasia. And got very interested in that investigatively. There wasn't anyone around since Edwin Osgood that had done in vitro studies of bone marrow. And I knew that that had to be done. You had to know what's going on in the bone marrow of these patients. So I started using some techniques that had just been developed. And if Edwin Osgood who's here a sort of reigning king of leukemia in Oregon in the '40s and '50s, if he had had these techniques that I had available, he would have just slayed the field. He would have gone crazy.

In fact, Marion Krippaehne, who I know you know, did the in vitro studies that Osgood tried to do. And they did these little culture things that didn't actually work. But they were, once people figured out the medium that was required that would make the cells happy, pretty much exactly what he was doing with Marion with those bone marrow cells is what we use now. So he was just like five years off, ten years off. He didn't have the tools. But we did, and we were lucky to get some good results.

Bloom: Were there other faculty members? You've mentioned two.

Bagby: Yes. A lot of them, let's see. Scott Goodnight was here. Bob Bigley was here in genetics. And he was sort of a neutrophil physiology guy. His lab was at Mac Hall on the second floor. Right next to Bob Koler's lab, who was also a hematologist, but worked mostly with Dick Jones and did hemoglobinopathy studies. In fact, Dick Jones and Bob Koler, before the genomic era, before you could clone DNA, described more hemoglobin variants than anyone in the world. That is, Dick Jones and Bob Koler were the leaders in that field.

Y.W. Kan, at UCSF, started employing genomics and then sort of took over. But they were the leaders. And of course Dick trained with Linus Pauling. I don't know whether you knew that. You probably knew that.

Bloom: Now you're still a fellow. And Susan, you're both fellows. Did you finish at the same time?

Bagby: We did.

Bloom: So you had a decision point there what to do next after the fellowship. And you're describing a productive but small group here at that time.

Bagby: Right. Right. We had a group that included Bob Mass, Sr., Michele Mass's dad. And he was at the VA and was the hematologist/oncologist at the VA. The one. So I didn't mean to leave him out, because he was very, very clinically busy. We wanted to stay in Portland, number one. We loved Portland. And number two, we both were really driven to do basic, what's now known as translational, research. It's like, it was like it was invented in the '90s. But the fact is, that's what George Porter was doing when he was a fellow, when he was working with toad bladders in San Francisco. That was translational research, too. So frankly, that's an overblown thing. It's a tiresome word. But that's what it was; translational research.

And Susan was also, as a fellow, picked out by Bill Bennett as a terrific teacher. And she's a really awesome teacher. And she wanted to do that. She had an opportunity to do that. And I talked to, because Bill would have supported her doing research for another year or two as an instructor or something. And I asked Dave Bristow, are there opportunities for me to join the faculty as a hematologist, da, da, da, da, da. Hod Lewis at the time was supportive. He helped me apply for an American College of Physicians Career Development Award. And Dave Bristow's answer, and that happened after Dave and I met. Dave said, "We'd love to have you. Get a grant." Which meant we'd love to have you, we don't want to pay. And show us that you're worthy of being retained by getting a grant.

So he referred me to Hod. And Hod was helpful in getting an ACP award. And then he referred me to John Kendall, who had just sort of taken over as the associate chief of staff of research at the VA. And John was totally helpful. And said, "Well, there are these career development awards at the VA. And we could double our population of hematologist by your getting a hematology award. And then there would be Bob Mass and you."

And I said, "Great." So I applied and I was lucky to get one. So I joined the faculty that way. I got a grant in 1976.

Bloom: '76.

Bagby: Yeah, a career development award. That also made the VA build a lab for me. And it was in a Quonset hut. I shared it with Steve Jones, who's at Legacy now and, I think, retired. But he still hangs out. And I've had my lab at the VA ever since. Through maybe four iterations of locations and new buildings and, thanks to Mark Hatfield. And I really have liked it a lot.

Bloom: Do you still have a lab now?

Bagby: I do. And, but it's going to close in April of 2015. I've decided I'm done writing grants. Part of it is because we've got a lot of junior faculty that are really smart and really interested in continuing on the work that we sort of have done with Fanconi anemia and leukemia and aplastic anemia. So I'm going to keep meeting with them and editing their applications and editing their drafts, manuscripts and drafts and things. So I'm going to keep my office over there and close my lab. No more grants.

Bloom: So in '76, you both joined the faculty? Susan—

Bagby: We did. We did. Assistant professors.

Bloom: What was your family like then? You had a child you had mentioned earlier. Did you have other children?

Bagby: We had Matthew. And he was four when we joined the faculty. And a year later, we had Sarah. And that's our family. Sarah and Matthew and Susan and me.

Bloom: Well, why don't we go forward from '76 and describe your career here at OHSU as a faculty member?

Bagby: As I said, John Kendall was really very, very helpful to me in developing the skill set you need to get grants. Bob Koler and Scott Goodnight were helpful in learning how to present papers. The first paper I presented to the division as a fellow, I had published a paper in the *Annals of Internal Medicine*. So I figured okay, it won't take me any time to prepare for this talk, because I'll just basically go down in the order in which it's in the manuscript, this published manuscript. Who's going to argue with that? And it was boring. I said introduction, methods, blah, blah, blah, blah, blah. And I wasn't telling a story. I could see them sort of canoodling in the back. And I think Bob [Koler] said, "Talk to him about how to present a scientific paper." So that was helpful

John Kendall I recruited to help me figure out why I got a "disapproval" on my first NIH investigator-initiated award. Just in case you don't know, a disapproval means, and there is no such score anymore, a disapproval means that if we had all the money on the planet, we wouldn't give you any, because this is such a bad application. And so he referred me to Dennis Burger. And John and Dennis sort of redlined my application and said, "Well, you should have done this, you should have done that." That helped a lot, because the next application, I got. So that was still a training period. There's junior faculty, time was an important training period for me.

Bloom: What did you do wrong to get a disapproval? Can you remember?

Bagby: Yeah. You know, there's a format. You really have to be very declarative. You have to. You can't fold in a nuanced concept into an introductory paragraph. You have to start the paragraph by making a declaration. "X causes Y" and then you can be a little more nuanced about that. I didn't do that. I didn't call out the bullet points. I figured oh, the person who's reading this is going to be able to get that from my nicely crafted paragraph. So the writing was clear by the format wasn't great.

The other thing I did wrong was to not keep reminding the reader what our hypothesis was. And I just said, okay, here's the area that they said, there's where you state your hypothesis. So I state my hypothesis. And then I never mention it again. That's a big mistake.

Then I packed it too densely. I didn't have any spaces. I didn't have any figures breaking up the narrative. So I changed all those things. And there was a bunch of other stuff, you know, rookie mistakes. In fact, I give annually the lecture to the hematology fellows on how to write an NIH grant. And I have examples from my first grant about don't ever do this. Don't ever do this.

The other part was sort of gamesmanship. Don't leave out a reference written by a member of the study section. That's just, and that's kind of cynical, but you've got to do it. You have to do it.

Bloom: So the next year you succeeded in getting your grant.

Bagby: Well, it wasn't the next year. But ultimately I was able to get an investigator-initiated award on a related subject.

Bloom: What was the topic of that successful grant?

Bagby: It had to do with the role that mononuclear phagocytes, monocytes, had to do with controlling the growth of bone marrow progenitor cells. And how that happened in a way that involved one cytokine stimulating another cell to make another cytokine. It's sort of a chain reaction sort of molecular biology thing. And I spent a good deal of time on it and had published a couple of papers in the *Journal of Clinical Investigation* to sort of bolster the idea. That was another mistake that I had made, i.e., I hadn't done enough preliminary work that I'd published to convince the review group that I could actually pull it off. The first one. And the second one, it was a slam-dunk. That, they couldn't say.

So that was my first NIH grant. And then it was renewed. And I got invited to join a study section at NIH pretty early on in my career. And I was, I said, of course I'm going to do this. Why would I not do this? That's where I really learned how to write a grant. Because when you review someone else's grant and you say, oh my God, this is terrible, you know, oh my God, I made that mistake myself. You sort of know what you need when you read grants that sort of make you mad because you have to read it and read it again and read it again to find the real meaning of what they were trying to say.

So that was very helpful. And ultimately I chaired that study section. And then had a hiatus and joined another. So I always tell the junior faculty here, if someone invites you to be on an NIH study section, to review investigator-initiated awards, you have to say yes. No matter how busy you are. No matter how many kids you've got. No matter how many dogs you've got. How big your farm is. You just have to say yes, because you learn so much about the right way to write applications.

Bloom: So now you're starting to get grants and you're at the VA. Are you at the VA fulltime?

Bagby: I'm at the VA fulltime because I'm a career development awardee. I had two stages of that.

Bloom: Okay. And you're basically doing hematology at the VA?

Bagby: Mm hmm.

Bloom: And you were the only one doing it?

Bagby: No, Bob Mass was there.

Bloom: I thought he was doing oncology. But he's doing—

Bagby: Oh, no, no. It was both.

Bloom: You were doing both?

Bagby: Yes. Yes. So I would have a patient with colorectal cancer in my clinic. And lung cancer in my clinic. And leukemia or myelodysplasia in my clinic. I was a general hematologist/oncologist. And in practice, that generally means oncology and hematology. At the VA, it was a little bit different because Bob was great about sending me most of the hematologic malignancies, and he would take all the rest of the stuff. But that's the way it was.

Bloom: Okay. And what did you do on the other side of the canyon, as we used to call it?

Bagby: I did teaching. So when the sophomores had their hematology course, I participated in that and gave lectures and was involved in small group sessions there. I didn't do any practice on this side of the hill except for weekends. And then, so I didn't really, and I liked the fact that I was hanging out, focusing on my research. Had a containable clinical burden. And when Scott told me that he was going to retire at like age ten – that made me mad—but he was, he said, “I'm going to retire.”

And they started looking, George Porter was chairman of medicine then, called me in and said, “Scott's going to retire. You should think about applying for being division head here.”

I said, “My God. Why would I do that? I'm having so much luck in my lab.”

Bloom: What year are we talking about now? About?

Bagby: I was afraid you were going to ask that. I think '84.

Bloom: Okay.

Bagby: I think it was '84 or '85. It might have been '85. So I said, “No. I'm not going to do that.”

And George said, he said, “Scott's going to retire. I think you might want to consider applying for division head job.”

I said, “To add to my burden nauseating administrative responsibilities? No thanks.”

And he said, “Well, just in case you change your mind, we'll convene a search committee. But I don't want you to be on the search committee.”

I said, “Great. Fine. That's also good.”

And I was grateful to him for that. Because what he meant was, he thought I was going to change my mind. And I was, of course, meeting with the candidates as they came through. And it was one after another medical oncologist who did clinical trials in the context of multi-institutional study groups and had no laboratory experience at all. No commitment to understanding the basic, fundamental causes at a molecular level of cancer. And this was right at the time molecular biology was taking off.

And in fact, I did a sabbatical in 1985. Susan and I and the kids went to Zurich. And I don't mean Zurich, Kansas. It was Zurich, Switzerland. To work at ETH with Charles Weissmann and to learn basic molecular methods. He was the guy that cloned the first human gene. Alpha interferon.

And that was what I wanted to do. That was the answer to everything. I mean, if you could figure out what genes turn on and off at the right time, and make a cell behave this way and that way, and distinguish a liver cell from a blood cell, I mean, how good could it get?

So when I came back from that, we were interviewing these candidates and they were all empirical, clinical trialists. And there's a place for that. Hodgkin's disease is largely curable now because people did clinical trials of an empirical nature. But for me personally, and for academics, my view of what an academic enterprise needed to encompass, wasn't right. It wasn't enough. Because it would be the pharmaceutical companies [dictating all clinical trials]. And those guys that were saying, "Oh, here's a new poison that you can try to kill these cancer cells." It was sort of a one-trick pony of kill it, kill it, kill it. And forget about what causes it; just kill it.

So my feeling was, after I'd seen the fourth candidate come through and they were all of the same phenotype. One was in one study group, one was in another study group. They had these, they had no laboratory and no apparent interest in using the lab to learn about how to advance the field. So I wrote a two-and-a-half page late letter saying I'd like to throw my hat in the ring. And here's what I believe. And what I believe is what I just told you. And they said, okay, we're going to interview you. And I interviewed with Bud Bardana and Bernie Pirofsky and George and they said okay, we like your vision, and that's what's going to happen.

And a couple years after that, Len Laster brought me in and said, "You know what? We're happy that you recruited Jeff Lawrence and these new guys." And Bruce Dana was there on the faculty as a medical oncologist. "And we're happy with the way things are going. You've sort of changed the culture of the division because there's more than one NIH grant in it now. So we want a cancer center." That's what Len said.

Bloom: So this was around '87?

Bagby: This was around '86 or '87. And I said, "Okay. Let me look into that and I'll get back to you."

So I learned what the National Cancer Institute did to provide cancer centers with bases of support. Because there's something called a Cancer Center Support Grant, CCSG, that the NCI puts out. And there were, I think, around thirty-five of them or forty of them at the time. And Len said, "We've got to have one. We've got to have one."

So I looked into the rules and regulations. What it would take, how they judge, what the instructions to the reviewers—oh, that's another trick. When you're applying to the grant, you should always go find the instructions that the funding agency provides to the reviewers of the grants on how they're going to score it. What they want. So I did that, as far as the Cancer Center Support Grant goes. And then went back to Len and said, "Here's what they want. We don't have these things. Here's what it's going to cost."

He said, "Well, I don't have that. So, see you." And that was the end of that discussion. Which was fine. Because that was yet another administrative thing that was going to get in the way of my happiness in the lab and seeing patients and teaching students. And those are the things that really made me happy.

Bloom: But you're now division chief. So you're doing things—did you move over here?

Bagby: Yes. Yes, I did. Yes.

Bloom: And you kept your lab at the VA?

Bagby: Kept my lab at the VA, and I kept my position at the VA.

Bloom: Your clinical work at the VA?

Bagby: Yes. But I also, I was administratively here and then got into the clinical rotation here as well. So we also fused the divisions. So we started crossing over. Scott would see hemostasis thrombosis patients on both sides of the hill, and that sort of thing. So we sort of, I didn't have an established clinic here every week. But I was seeing patients on the in-patient service during that time. And my out-patient clinic exposure was largely at the VA. And it stayed there. So I was based here now.

Bloom: When you said that Scott retired, you meant as division chief. Because didn't he stay on as a clinician?

Bagby: I think not. I honestly can't remember. Did he?

Bloom: Well you just said that—

Bagby: Yeah, I guess he must have stayed on for another year or two. But honestly, he retired in his fifties. That's just what he did. And he went off to Mosier and lived in Mosier for a while. And now he lives in Hood River. Growing blueberries.

Bloom: Well I hired him in the dean's office in '94.

Bagby: Oh, you did?

Bloom: I did.

Bagby: So then he came back, then.

Bloom: He must have come back.

Bagby: He had retired.

Bloom: Yeah.

Bagby: So I guess you're right. I guess he retired as division head. Hung out, did his hemostasis. And teaching, which is really important. He was a great teacher. But I think he came back after retirement.

Bloom: Yeah. Okay. I just wanted to clarify that. So you're now division chief. You have responsibilities for the whole division. Dr. Laster asked you to start something but didn't have the resources at that point. So how do we move on from there?

Bagby: Well at that point, and just a few years later, Pete Kohler came to town. And after about a year in harness here as president, I had the same conversation with him. And he said, "You know, we really do need a cancer center." And part of the motivation for bringing it up at the time was because he had heard that Susan and I were looking at positions at Baylor at the time. And the, what was on the table at Baylor was "we want a Baylor cancer center." And that's how they were going to spring for endowed chairs and blah, blah, blah, blah. Texans have a lot of money to spend.

And the, I remember John Kendall came to me and said, "I hear you're looking at Houston. And that you've gone more than one time."

I said, "Yeah."

He said, "What's on the table?" And I really did want to pursue it, because it might help show the world of oncology that there is another way. "And that not a lot of places are doing that right now." And within weeks, he called me in and chatted. And it helped a lot that he knew what Baylor was like. He'd spent time at Baylor. He knew a lot of people that I knew and said, "We really need one of these as well. And we may not be able to spring for a gazillion resources right away, but there are certain things we can do right now."

And so I said, "Okay, that's great." It's a great way to be able to stay in Portland. And things in Houston had started looking not as rosy after the third visit. The realities. I was able to dig up some of the truths of what was on the table.

That was where I moved to a different space. Because I told them in order to plan the cancer center and to invest all the administrative time that I'd need to invest, they'd have to get a division head in hematology that wasn't me.

And it may be that Scott came back for a period of time. I think maybe Scott stepped in for that. Gosh, I'm sorry. I can't remember that part. But I remember moving over to the BIC in the basement. And there was a windowless office and an outer office. And I had an administrative assistant and myself to plan the cancer center. And we had, and I was fortunate to recruit a terrific woman by the name of Candi Adams who was just perfect. She was exactly the kind of person that I needed to take care of all the things that I didn't really want to be bothered with.

And Pete was totally supportive. He was very, very supportive and got Mark Hatfield involved. The National Cancer Institute came out with a call for planning grants for cancer centers. And we duly applied. And we were one of twenty-four institutions that didn't have cancer centers to get a planning grant. And it was a three-year planning grant to create a plan to apply for a real one.

So we did our thing. And I think we got the planning grant in 1994. And applied in '96 for a real one. And missed. And the score, it was a reasonable score, but we weren't funded that round.

This was just around the time that Brian Druker's, who we recruited earlier in the '90s, Brian Druker's studies were starting to evolve in a way that were very, very optimistic. And let me back up to say that one of the things that I made clear with Pete

was that I had to be able to recruit somebody who had the potential of proving to the world that the molecular approach was going to work. And from a good friend at Harvard, I'd learned that Brian Druker was such a guy. And he was being actively recruited from Harvard to other institutions. And my friend Jim Griffin said, "You really need to go after this guy."

So we did. He came to visit. And ultimately decided to come here. Built a lab in Jones Hall. And it took him six weeks from the start of the building the lab to doing his first experiment. It was the fastest lab build I'd ever seen. This was in the early '90s. And the first site visit that we had was in the old library, one of the side rooms. And we, I was able to point to this and the Hatfield Building, which was sort of a little steel superstructure, it wasn't a building yet, and say, "That's where our administrative offices are going to be."

But when I looked around the room, I was supposed to start out by saying what is the overarching, operating principle of our cancer center here in Oregon. And I started out with saying, "You can't fix it until you know what causes it. And to do that, you have to understand the molecular nature of how cells become mutated to become neoplastic, how they fail to die, how they expand, how they spread. And you have to know that from within. You have to know that in a molecular way. And that everything we do in our cancer center will evolve from that. Yes, we will do empirical, clinical trials, because they're important. But what drives us is the other."

And I saw around the table, which was full of external reviewers from other cancer centers who did empirical clinical trials. There was so much eye rolling, I couldn't believe it. And I was a little worried. Because they were saying oh, God, here we go. Another one of these science geeks talking about the importance of science. And we all know, of course, that empirical clinical trials are the only way we're going to make any headway. Which no one ever said, but that's what the eye rolling meant.

So we didn't get funded that time. And we applied a year and a half later and were funded in 1997 for our first real center grant. And at that time, we were only one of two out of the twenty-five planning grants that had actually been converted to a real center. Does it give us a lot of money to operate with? Not really. It provides some support for core labs. So it pays the salary of people who run core facilities that any investigator can use. And that it saves money for the institution. So it's a good thing. Not every biochemist has to build their own unit. They can go to this core unit. It provided some support for the administrative operation. But it really didn't provide much else. So it wasn't like a windfall.

But the halo effect of having an NCI-designated cancer center in our institution, that was the windfall. That's what made a difference. Because no one can compete with that, not in our state. So it was helpful to get that award.

But interestingly, during the second site visit, where these people were surrounding the table, we held it in the Hatfield. Up on the top floor. So we could say, okay, this is where we live now. This is our headquarters. That was important. But more importantly, Brian had his data. He had the data that compellingly showed that he could cure, that the likelihood was, based on his studies in mice and in vitro studies, that he was going to be able to give this drug to human beings and to cure them. And had data to support that.

And so when I had this closing session with this group, there wasn't any eye rolling anymore. There couldn't be any eye rolling. Because there was this clear evidence that this was going to work, and that this mission was right. And the eye rolling went away. And in fact, some of these guys were saying, "Oh, yeah. We knew that. We knew that." I was like, "you need to be slapped."

But that was our first round of being funded. And we've been funded as a cancer center ever since.

Brian changed everything, Brian's discovery changed everything. It changed our fundability. It changed the belief of pharmaceutical companies that they should be looking for small molecules that fixed molecules that were wrong in cancer. It convinced the investors for those companies that that's a safe investment. It convinced institutions that they should be doing that same kind of thing. And if you look now at the final statement of any publication in the field of hematology oncology, you're going to find, fifty percent of the time, a statement, or even in *Journal of Biological Chemistry*, a statement to the effect, "Now that we've discovered this, this molecule may be an important therapeutic target."

And I think one of the things I might do in my real retirement is to go through PubMed and search for claims like that and find out when the break point was, when did this start to happen. And my bet is that it's post-Gleevec. Gleevec is Brian's drug.

Bloom: Now am I correct in thinking that this discovery and this disease, that it works and went back to the patient that you described from Camp Pendleton?

Bagby: Oh, you mean Baylor? The Baylor—

Bloom: The woman, yes—

Bagby: The woman who died with AML? In my mind, the Gleevec discovery was based on a process of twenty years of learning what was wrong with the chronic myelogenous leukemia cell. The genetics had been done. The translocation had been discovered. First, the chromosomal abnormality was discovered in Philadelphia. It's called the Philadelphia chromosome. Be happy about that. The translocation was worked out in Chicago. There were investigators around the country, including L.A., who had figured out how that translocation resulted in an abnormal protein, and how the abnormal protein worked to drive the growth of these leukemic cells. The uncontrolled growth of these leukemic cells. So it was a disease that had a discrete molecular cause and an abnormal enzyme that was a target. And Brian took off to try to identify the way to shut that out, shut that down.

Bloom: Let's go forward from there. So—

Bagby: So in a way, I'm sorry, I didn't get to your question. In a way, what I had hoped to do with AML is what he actually did with CML.

Bloom: Ah, that's what I was—

Bagby: Yeah.

Bloom: So it must have been an enormously satisfying—

Bagby: He was more successful than I was.

Bloom: Well, but very satisfying for you.

Bagby: Oh, yeah. Absolutely. Absolutely.

Bloom: Yeah. Well let's go forward from that site visit and presenting those data to today. How did the cancer center develop from that point on?

Bagby: Each year we got stronger. One of the things that is helpful when you have a Cancer Center Support Grant from the NCI is that donors, or potential donors, don't have to ask the question of is this smoke and mirrors or is it real. Because it is real. The one thing that the NCI does know is that once they give that kind of award, the fundraising activities of the institution around the cancer problem are much more successful. So we, the cancer center established a good working relationship with the foundation. We had a cancer council, which was made up of community leaders and donors that were interested in our success. One of them, there were many members of importance at that time. Jim Rudd, for example, was there with Pete and Mark Hatfield when they said, "We are very, very committed to getting this cancer thing done. And what we need is bricks and mortar. What we need is new faculty. What we need is to develop a funding, a pathway through the foundation that is going to be able to sustain the cancer center and its expansion beyond what the National Cancer Institute provides because all that covers is the administrative operations and core labs.

So the cancer council and its evolution to what it is now, which is unbelievably effective, was an important second step. The bricks and mortar thing was important as well. And in fact, the Hatfield Building was important because we were able to be assigned the fourteenth floor for our administrative operations. So we had some core labs there, biostatistics and bioinformatics were there. We had a tissue processing lab and some things up there, too. But a lot of clinical trials people were there.

We then, fortunately, because Mark Hatfield was the chair of the appropriations committee at the time, were able to get a new VA building. And the second floor of the VA building was ceded to the cancer center. That is, as cancer center director, I was able to control the second floor and the appointments of people to the second floor. Some of which were VA cancer researchers, and others of which were investigators from OHSU. So that was a quid pro quo kind of thing for Hatfield. That was a building that was called now buildings 102 and 103, half of which is research, half of which is clinical. The VA needed the space. They needed the research space and they needed the clinical space. So there's a plaque on that building. Bill Clinton, da, da, da, da. I'm sort of down on the plaque. I think I've reached the pinnacle of my success. I'm on a VA building plaque with Bill Clinton.

And it was a really great experience, because the cancer center's third review, the second review of our real center, was able to go and see this place, and saw it as a good

sign that the VA and the university were cooperating and it was a unique arrangement, from their standpoint, one that they'd hoped to convince other institutions to do.

Bloom: I remember that when the center transformed from a center to an institute, that was an important thing for you as I remember it.

Bagby: Yeah.

Bloom: Could you describe that a little?

Bagby: Yeah. This was, this question of whether it needs to be a center or an institute, I remember that part of it had to do with centers arising around our, in our environment. Eugene, Providence. That you can, everyone was sort of saying, "Oh, well, they've got a center, we can have a center, too. We'll put up a sign." That's overstating it in some cases where they made bigger investments in that, including Providence. But part of it was I think the majority of it was a perception kind of thing.

The other part had to do, I think, with who I was supposed to report to. And there was, the hardest part of my job in developing the cancer center was to overcome 400 years of the same model. So the first medical school was in Italy in the seventeenth century. And it had departments. So the department model is a 400 year-old model. And each department had its own department head. And maybe it made sense then, because the surgeons did surgery, and blah, blah, blah. But in today's practice and research environment, the departmental model has its limits.

And so for example, we have a bone marrow transplant unit. And that transplant unit has dedicated infectious disease people – and I'm not talking about departments now, I'm talking about divisions, and the differences in divisions – that are paid by the revenue streams that are from the transplant unit. And that's what they do. They do transplant ID fulltime. That makes it better for patients and families. And it makes it better for physicians caring for them to have a coherent interdisciplinary group focusing on one population of very, very sick patients.

The same is true for science. Can you do [big science] without having a bioinformaticist these days? No. Can you do without having somebody who's interested in chips? And I mean Intel chips? No, you can't. So it's pretty important to be able to be, to have a unit that is more than just a department. And each department will have its own needs. And the revenues that pass to the departments are something that they will fight over. And if there is an individual in a department that wants to raise money through the departments to enrich their research activities without paying attention to the people that they actually work with in other departments, that's a problem. So it seemed, the big challenge was, you've got to trust us to be able to raise money for cancer. And that you're going to win with this. So please join us. Help us raise the kinds of support that are required for endowed chairs and professorships for seed money grants for start-ups, for core labs.

And that you're going to be able to get your work done better when you do that. And by doing it through the cancer institute, which was a peer-reviewed, administrative entity, from the standpoint of the NIH, made it reasonable to me. Moreover, if we couldn't do that, if we couldn't show the NCI that we had the authority for overseeing the

operation of the foundation in raising money for cancer, they wouldn't have believed that we were a cancer center. They would have said okay, this is a collection of balkanized departments, each of which have their own agendas. And they're all focusing on their own agendas and not talking to each other.

So that balancing act was pretty hard. That was the hardest part of my job. Thankfully I had some smart deans. Of which I've had eleven, by the way, through my career.

Bloom: You've survived, though.

Bagby: Yeah. So the other element of the institute versus center thing was that the centers that pop up around the country and around even in our region are very commonly clinical centers. That they are centers where people go and have multidisciplinary care. And that's all fine. The part that made me want to rethink the center idea was the fact that research was part of our portfolio. And it's not a part of the portfolio of the other places. And to distinguish ourselves as some entity that did both care and fundamental research was important to me at the time. And I know that we went round and around about what does it mean. And in fact, I think you wrote the final draft on what an institute was and what a center was in the context of OHSU. I don't think that's changed.

Bloom: Well let's move on with the development of the institute. It's now named "institute," so how did we progress with the development over the last, I guess it would be the last ten years, maybe a little less, that you've been an institute.

Bagby: Part of the success has, well, a lot of the success has to do with Brian's success as a, not only as a scientist, but as a physician. Part of it also has to do with a change in the rules of fundraising. And that when Pete Kohler was here, there were places that were off limits. There were individuals that were potential donors, major donors, that needed to be cleared. And that the approach needed to be made, by edict, by either the president or someone that he designated. But largely by Pete himself. And that's an understandable thing. There were a lot of bricks and mortar projects going on at the time, and it wasn't anything that I would ever be critical of. But it was a fact of life that the most likely major donors were not approachable at that time.

When Pete retired and Joe Robertson was selected from a large group of applicants to run the center, it was clear that his major goal was to retain Brian. And to make sure that Brian was going to be happy here. And at that time, he and I were talking a lot about trying to convince Brian that he needs to be the director of the cancer center. I was nearly sixty-five at the time. And that was a major objective of Joe's and a major objective of mine. So I kind of worked with Joe and Brian to work out the conditions of that kind of thing. Because I was worried, too, because Brian had started looking at some pretty big-time places.

And I have to thank, I think all of us should thank Alex Druker, Brian's wife, for taking the following position. When he came back from X center and said, "Wow, I could have an operating budget of da, da, da, da, da," she would say, "You can do that here. You can do that here." And that was her standard thing. So I have to say I bless Alex for taking that position. Because Brian did do that. He took this position. Part of what he

knew that he needed was he needed to make approaches to people that weren't approachable in the past. And Joe said, "Great. Go ahead." So Joe was extraordinarily supportive. And we had important people on the advisory board, on the community council, at that time, that were able to get him entrees to important donors. And for a variety of reasons that had to do with, that were personal, and others that were belief in his status as an international rock star at the time, that ultimately led to the first major gift from Phil and Penny Knight. And that was all on Brian. That was all his doing. And he picked up this ball and really has obviously has gone to places that were unimaginable on my watch. That's for sure.

Bloom: So what is the cancer institute today? I mean, what do they actually do there? And what's your role in it at the present time? What year did Brian take over as director?

Bagby: I'll have to do some math. But I think seven years ago. Because when I turned sixty-five I said okay, I'm done. And now, well, first of all, my role is nothing. I mean, I meet with Brian now and then to chat about things. But I am very, very happy to have given up all these arguments about centers versus institutes, about policies for this and that. And focus on my research and mentoring stuff. And I've tried hard to stay out of the way. Because it's, you know, early on people would call and say, "Well, what do you think about this?"

And my stock answer was, "It doesn't matter. That's Brian's business. That's the business of the cancer center director, and I don't know enough about it to weigh in on an answer. And I wouldn't give it to you if I had." So I really am not participating very much in the cancer center operation at all, except that I have a lab and I use the core facilities. And I deal with the people that I help recruit to our center many years ago to help get our research done. So honestly I really, the answer to what my role is, I don't have a role. And I really, really like that.

Bloom: Are you seeing patients?

Bagby: No. I closed my clinic as well when I "retired" from the VA. So I've had a laboratory. I continue to involve myself with teaching and mentoring and running my lab.

Bloom: So my question, I would like you to, for our record here, to give us the theme of your research over the years, and what you've tried to accomplish in your own laboratory.

Bagby: My goal on the night that my faculty member at Baylor said, "Why don't you figure it out," was to figure out why leukemia cells behave in the way that they do. And the form of leukemia that I'm particularly interested is called acute myelogenous leukemia. The path from 1976 to now has been, has taken me all over the place. I knew that we had to learn how the bone marrow functioned. And early on in my career I saw patients who had strange bone marrow abnormalities that, some of which led to leukemia. Others of which were life-threatening and a failure of the bone marrow to produce the normal amount of blood cells per day. And there are billions. I mean, it's a huge factory,

the bone marrow. And there has to be a balanced production of the kind you need in your blood, and the parents of those cells in the bone marrow.

And in leukemia and acute leukemia, the parents, it's full of the parent cells. But they're not making any kids. They're not making any well-differentiated, functioning blood cells that protect you from infection and help you breathe and things. Help you avoid dying of a massive hemorrhage when you cut yourself.

So in seeing those patients and having a lab, I was able to ask questions that had to do with what caused this defect? And pretty soon it was, we were involved in studies not only on leukemia, but on a syndrome called bone marrow failure, aplastic anemia. And the failure of the bone marrow to produce blood cells. The bone marrow should be full of cells. And sometimes the patients I was seeing had bone marrows that were empty. The factories didn't have anything.

And so we actually ended up focusing on how the bone marrow fails and how cells communicate with each other. And determined that a lot of the things we were seeing were autoimmune. That is, the body of this, the lymphocytes and the immune system of this patient were actually attacking the bone marrow cells and destroying them. So it's, and now it's pretty standard therapy to treat patients with these autoimmune conditions with immune-suppressive therapy. And a lot of patients go into good, permanent remission if you treat aplastic anemia, for example, with anti-thymocyte globulin. And that's not because of studies we've done, it's because of studies that have gone on around the world, including in the intramural program at the National Heart, Lung and Blood Institute, which is particularly strong in that field.

But along the way, I've focused on bone marrow failure and leukemia. And in the late '80s, I got a call from a woman that I had spent a year in college with before she transferred to Stanford. And I know that it would be okay with her for me to tell this story, because the story is widely known. She said, "What do you know about," her name is Lynn Frohnmayer. And she said, "What do you know about Fanconi anemia?"

And I said, "Not very much. I know that it's a bone marrow failure condition and sometimes it leads to leukemia."

She said, "Well, that's what my two daughters have."

And so I started learning more. Soon thereafter we had Dave and Lynn Frohnmayer let it be widely known through, they were in *Parade* magazine with this, that they were looking for [stem cell] donors for their kids, and they'd learned about this, and they made a big push for public recognition. And we held, twenty-five years ago, a meeting in downtown Portland that they convened and asked people from around the world to attend who had done work on Fanconi anemia. In addition, they had others attend, including Marcus Grompe, Robb Moses, myself. And so there were people from the Netherlands, New York City, Toronto, Minnesota and Italy to talk about what needed to be done. Because Dave and Lynn were dead set on figuring out an answer to the problem. They've created the Fanconi Anemia Research Fund, which is probably the best disease-focused research fund on the planet. They've raised sixteen and a half million dollars to date, have funded more than 150 investigators in fifty countries, including investigators here. They've created a nationwide network of parents that raise money for the fund. They have an annual meeting of parents and families and kids afflicted with the disease in Maine every year, which is all about families, and not so much science. Although the parents, especially the newly-diagnosed parents, are interested in hearing

about the science, so there are some scientists who go. But there's also a scientific meeting that is very, very well attended. In fact, it's going to be held, the next one is in three and a half weeks. The twenty-fifth meeting of the Fanconi Anemia Research Fund.

Since that phone call, I've been focused on that disease. Because it was, number one, she's a friend, and David and Lynn are friends, number one. But number two, it's a huge challenge and an opportunity as well that put together my interest in bone marrow failure and leukemia. Because now it's very clear that one leads to the other. And now what we're doing is trying to convince the world that if you can identify patients at risk of developing leukemia and fix the bone marrow failure part, then this won't happen. What that means is that if you understand this part, the bone marrow failure part, and you can develop a therapeutic for that, a la a Gleevec for CML, that you are preventing leukemia. So to me, that's my mission right now is to convince people that if Brian can do it for somebody with established disease, is there anything better than curing somebody? Yes, there is. Preventing the disease in the first place. So that's my mission now. And that's one of the things that we're focusing on now, and some of the projects that we're transferring, that I'm making certain that are going to go on post-Bagby are those projects. Those projects that seek to prevent leukemia by fixing this problem in the background.

And in fact, from what we've learned so far, using really great systems biology methods in the core labs that we built, were our telling us that it may be that every single person with acute myelogenous leukemia had bone marrow failure of some kind, albeit subclinical, even, at some point in their past. And if there are markers of that, you could prevent leukemia in everybody. So that's a stretch. That's a moonshot kind of thing. But why not?

Bloom: So how are you planning for it to continue?

Bagby: One thing I'm convinced of is, I don't know if you know Good Morning America at all, but Robin Roberts is on that. And she has publicized her experience of having breast cancer. Getting chemotherapy for that. And then developing later, years later, myelodysplasia, which is a life-threatening condition that required a bone marrow transplant. Which she got successfully and now she's well.

Guess what happens when you get chemotherapy? You get bone marrow damage. And we've done a good deal of studies now to convince ourselves that there are some patients that had chemotherapy in the past that have bad bone marrows that you don't know about because their blood counts are just okay. They're all right. But you can prove that their bone marrows are abnormal if you take them out and look at them in the lab. And the way that they're abnormal is that they're vulnerable to environmental cues that we can throw into the culture dish.

When these people get leukemia – they aren't the same people we studied, because they don't have leukemia now – but when you have somebody who's had chemotherapy and then does get leukemia, those environmental cues, the vulnerability part to the environmental cues not only has gone away, but the leukemic cells have learned to use that bad environmental cue that was hurting them, their progenitors and their precursors before, to use that as a growth factor. So they've adapted in a way that a colony of rabbits would evolve in the Galapagos or something. And I know there's no

rabbits in the Galapagos. But it's a Darwinian story that is unarguable. So if you could fix the problem in the first place, then maybe you won't see it.

And I can imagine that someday we're going to say to people like Robin Roberts, okay, you have breast cancer, we're going to give you your adjuvant chemotherapy, no problem. But then you're going to take this pill for a year. And this pill for a year is going to protect your bone marrow and allow these epigenetic events that happen to recover. And then you can stop taking your pill and now you won't get leukemia. Or you won't get myelodysplasia. That's not irrational. It's a moonshot, but it's not irrational.

Bloom: You're going to close the lab.

Bagby: Oh, sorry. Getting to the point, there are investigators now in hematology oncology. Young investigators, assistant professors in medicine, that are all over this model. And really like this clonal selection model. And there are some who are working in Brian's lab now that have come to realize that that's probably the right model in a different way. By using Brian's screening tools and finding out that "oh, look, interleukin one is actually inducing the growth of these leukemic cells. And when we turn it off, these leukemic cells die." That's not the way interleukin one works for normal cells. It suppresses normal cells. But in these leukemic cells, in some patients, they grow better.

So there, I think, will be a team of at least three assistant professors applying for grants to the Komen Foundation to try to convince them. We tried unsuccessfully in the past, that studying leukemia induced by breast cancer treatment is breast cancer research. Komen came back and said, "yeah, it's an interesting model, but it's not really breast cancer research. It isn't about how breast cancer works."

Well excuse me. I'm sorry if three to four percent of women are at risk of developing this after they get chemotherapy, and hundreds of thousands of women get treated, you're going to see, I mean, that, to me, is a breast cancer problem. It needs to be solved.

So there will be Komen applications. There will be NIH career development awards from these three people, maybe four, maybe, hopefully more. And I'm going to be right there, telling them what to do.

Bloom: What do you see as your future here after you close the lab?

Bagby: I'm going to spend time with these guys and keep up, I'm going to keep reading the literature. I mean, it's really cool stuff. And read all the drafts of their manuscripts and the drafts of their grant applications. And give them editorial input.

Bloom: Good. We'll take a little time in a minute to talk about your view of OHSU over the years and the educational functions at OHSU. But just fill us in on where Susan is in her career at this point.

Bagby: Yeah. I'll start from the beginning. Susan graduated number one in her class at Baylor. Remember, she was one of three women. And her mom, at graduation, when she was called up repeatedly for, the OB, or the physiology prize, Susan Pound. Or actually,

she was Susan Bagby then. But her mom said, “This is embarrassing. Don’t the other guys get anything?”

So she was absolutely, she was just an absolutely brilliant student, and she is always brilliant. She worked in nephrology, she ran the nephrology unit. She has always been interested in hypertension. And she was one of the people that was most excited when David Barker came. Because David Barker’s hypothesis, which isn’t really a hypothesis anymore, it’s proven to be truthful, was that if mothers had abnormal nutrition during pregnancy, their children were not necessarily sick at birth or during childhood. But later on in adulthood, the incidence of diabetes, hypertension, congestive heart failure and coronary artery disease and obesity were significantly higher. And he did this based on epidemiological data from the Netherlands and from Hertfordshire in England, from Finland and India.

And Susan loved that model. Loves it. I do, too. It’s really an interesting story. But realized that the world and a lot of people were pushing back. It was during the genomic era. And a lot of people at Oxford, for example, were putting him down and being really pretty disrespectful of his results. Saying that it’s genetics that leads to those things. Forget about this environmental cause.

Susan wanted to approach that investigatively, so used pigs as a model and used microswine research to prove that in fact if you nutritionally deprive the moms, the babies were born, had fewer glomeruli, had vessels that were hypersensitive to norepinephrine and contracted more. So that was a perfect correlate of, a surrogate marker of hypertension in the future. And a whole bunch of other stuff that she’s done that I’m not up to date on.

During this time when David Barker and his wife Jan were coming in from Southampton, actually Hampshire, to stay in their condo down in Southwest Portland, we became good friends with them and spent a lot of time with them. And Susan’s a devotee of this in utero model now, and played a role in garnering the Bob Moore donation. In fact, Bob treats her like his daughter now. And Bob Moore is the guy who created Bob’s Red Mill.

So she’s very, very much involved with the Moore Institute. And in the part that has to do with outreach and education of adolescent girls and young women about this model. And it has a lot to do with nutrition. So she’s all over this thing with nutrition in the womb. She’s dealing with the March of Dimes. She’s giving talks around town. She’s way busier than I am. I’m spending more time gardening and biking.

Bloom: And she’s retired clinically?

Bagby: She has retired clinically. She has. Yeah.

Bloom: I wanted to catch up with her, too. So tell us some of your views. You’ve been here a long time. How do you see OHSU over the years? How do you see the educational programs now compared to the past? I mean, you brought up certain issues that relate to the growth of institutes and education. I appreciate your thoughts about that.

Bagby: I think that there are, first of all there are certain things that I can’t answer. I’ve never been on the admissions committee, nor have I wanted to be. I haven’t been

involved in teaching the sophomores for six years now. So there are certain things I just don't know.

One of the things that I have noticed over the years, though, and I think it's pretty much the same, is that the students get most energized and most engaged when you're being real about how you think as a clinician. And as an example, when I had the small group sessions with the students like four days a week, when we were talking about a case, the temptation was, "Okay, just read the history, the physical, and blah, blah, blah," and then start thinking according to what's on the page. But before we started, I usually, I'd say okay, I know this is what we could do. But picture yourself now—and I'd set a stage. Like this is about a rancher in Enterprise. And you're at the Enterprise Hospital. And you're all alone. All alone one night. And this guy comes in and he's hemorrhaging all over the place. So put yourself, think about it. Just take a deep breath and meditate yourself into the Enterprise Hospital. And now you're having to make these calls.

And people would wake up. They'd say, okay, I can do that. And that was fun, because the answer's about your hematocrit is low. Could it be just all about the blood. If I say okay, you have somebody who's having trouble breathing. Are you going to jump to [measuring] their blood count? No. you're going to listen to their heart. You're going to take an X-ray and make sure they don't have pneumonia. So if you really were paying attention to what it's like to be a doctor on the front line, then the students would respect that and really take it to heart. And that strategy works all the time.

It also works to identify the people who can't do that. You know? And I don't know how they turn out. But I'm not optimistic.

I'm not worried about the educational system for medical students here. It's always been good and I can't imagine that it's changed. Now I know that the curriculum has changed. I don't know very much about how the curriculum has changed. But it seems to me that what it has done is to sort of amplify what I've just described in a real way. I mean, you're going to see someone who's sick. But I don't know that. So I can't answer that.

One thing I am impressed by, though, is how many high school and college students we have on our campus, compared to the old days. There are programs for underserved that, and there are many programs for underserved now that we didn't have in the old days. I was happy that we were one of those that got an NIH grant to do that. They were called the CURE Awards. And the National Cancer Institute sent them out and said to cancer centers, "If you want to apply, tell us how you're going to bring up young people and get them into biomedical research in the cancer field." And so we applied and got one of those. And they sustained it for about five years. And they said, "Well, we don't have any more money. We're not going to do that." And we said okay, we're going to find a donor that will replace that.

And Rachel Hunsinger, bless her heart, who's our lead fundraiser for the cancer institute, did that. And we got a major donor. And we have that program still running on our own dime.

So that's a huge, that's a huge difference. It feels more like OHSU has reached into the community and sort of has got some blood flow into the community that we didn't have in the past. That's my perception.

Bloom: What about, you're an internist, I'm a psychiatrist. What about your view of your own specialty. Where do you see internal medicine? I know in terms of some of the things you brought up earlier, some of the department conflicts come up in your, might come up in your assessment of your own basic training in medicine.

Bagby: Yeah, I'm not, I think that the departments play an important role in training. I don't see how a multi-departmental entity can train people that go out and do stuff that involve a discipline that has 400 years behind it. That's the good part of internal medicine. I mean, it's a very well-defined field. I know that there's, so the funding pathway or the academic mission beyond training, I think, needs to be enriched by multi-departmental entities. But I don't think that multi-departmental entities can satisfactorily replace the departments in training. I don't see that happening.

I mean, the truth is that when I, I was lucky enough to have a job at USC Medical School washing dishes as a high school student. And after the first few weeks, they invited me in to do a few experiments in paper chromatography. And this was in the laboratory of a diabetologist at USC. And it was in my neighborhood. So it was an easy summer job to have, and I had it for a couple of years. And I was surrounded by endocrinology people.

And then I went to Baylor and before I started as a freshman, I had a chance to work in a lab in Houston in the summer of a pediatric endocrinologist. And I learned how they thought. I went to their grand rounds and things.

And a lot of the principles of endocrinology are applicable to all the other specialties in medicine. Especially hematology. If you think that someone is making enough of this stuff, then give them that stuff and see if they get better. Or if they're making too much, turn it off and see if they get better. And measure the hormone and then you—so there's principles of, simple principles of investigation that are, and clinical thinking that are relevant to all of the subspecialties of medicine. I mean, that feedback inhibition thing in endocrinology is applicable to almost everything. Including infectious disease. So, in cardiology. So I like the way the departments are. I like having the departments training people to do what they've always done. And I think they do that in a good way and I don't think that's broken. And I hope that no one tries to fix it with some multi-departmental thing.

Bloom: Now is there anything else that you'd like to say that I haven't covered? Or we haven't covered today? Other areas that—

Bagby: Yeah, only one. And I think they chose the right guy. In trying to get through this multi-departmental thing and convince people that it wasn't a threat, it was a good thing, I needed help from the Dean's office. And you were really terrific about that. And one of the things that made me feel best was that I knew in my heart that no one could come in and spin a yarn that wasn't true without your seeing through it. So I took some comfort in that, knowing that some department head was going to come in with his hair on fire and make claims that weren't true. And you were going to be able to receive that as a shrink would receive it; like "Well, how are you feeling about that?"

And the other part was that it also made me really happy that you were suited for the job because of your specialty in managing the criminally insane.

Bloom: Well, yes, there's something to that. Thank you very much. I figured out early that cancer was important. That's the whole deal. Thank you. You have some questions?

Young: I just have one, that is, what do you see is the future of the cancer institute, and how does that relate to the future of OHSU as an institution?

Bagby: I think the cancer institute has a lot to say about the health of the institution. And it would be easy to be fearful if you were someone in a field that wasn't, didn't have anything to do with cancer. And to those individuals, I would say, I have a lot of confidence in Brian Druker having the same view as I do, that your definition of cancer needs to be on that's so broad that people from the National Cancer Institute may not accept it as cancer-related or cancer research. You cannot have a—there are institutions, in Florida, for example, where the cancer institute or the cancer center is the dominant thing. And that the university itself is sort of a second-string player. That can't happen. And so the investment in the infrastructure, for example, in Joe Gray's unit, is something that has to do with bioengineering and computational excellence. That isn't limited to cancer. It's absolutely applicable to everything, including, for example, the David Barker model. And in fact we did a study on placentas from Southampton. Using the core facility of the cancer institute to get answers that had to do with development. In the Barker model. Not with cancer. And that's what needs to happen. When you have the kind of dollars, the kind of revenue or, the capacity, the financial capacity to be building up strength that's going to last, you need to build it in a way that everyone is going to be able to use it. You cannot turn to someone and say, "Oh, that's not cancer, so we're not going to be supportive."

That won't happen here. Brian is a smart person. He's a good person and he's dedicated to the university and the welfare of people who do not do "cancer research." And that's why we're seeing a lot of infrastructural development with these important donations from the Knights.

Young: Great. Thank you very much.

Bagby: Other people? We're done?

Young: We're done.

Bagby: Okay. Great. It was fun.

Bloom: Thank you.

Bagby: Yeah. It was fun for me. It's hard, though.

[End Interview.]