Peter Basser by David Zierler
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DAVID ZIERLER: This is David Zierler, oral historian for the American Institute of Physics. It's April 30th, 2020. It is my great pleasure to be here virtually with Dr. Peter Basser. Peter, thank you so much for being with me today finally.

PETER BASSER: David, it's a pleasure to be here, and I'm really looking forward to this interview.

ZIERLER: Excellent. Okay. So let's start with your title and your institutional affiliation.

BASSER: Well, I'm a Principal Investigator in the National Institutes of Health, and I'm a Section Chief of the Laboratory on Quantitative Imaging and Tissue Sciences within the National Institute of Child Health and Human Development. And I also have a title as an Associate Scientific Director for Translational Imaging and Genomic Integrity, which is a leadership position where I provide advice to our Scientific Director. So those are the various hats that I wear. I'm also on the Assembly of Scientists, which is an advocacy group for the scientists at the NIH who would like to make their opinions known to leadership, and we also communicate with scientists at the NIH based on what we learn from leadership. So it's a very nice organization that creates a bridge between Building 1, our administration, and the group of scientists that work there.

ZIERLER: Mm-hmm. OK.

BASSER: So that's pretty much it.

ZIERLER: So let's start right at the beginning. Tell me about your birthplace and your family background.

BASSER: Wow. OK. That's a long time ago.

ZIERLER: [laugh] I want you to know, the oldest person I'm interviewing is Richard Finkelstein at UCLA. He's 104.

BASSER: Wow! OK.

ZIERLER: So that's really a long time ago, so you're nowhere near that.

BASSER: Well, so I was born in Long Island on the South Shore where my family was living. I guess, to roll back a little bit, my parents are both immigrants from Austria. They came over around the time of the *Anschluss* to escape Hitler and the wave of Nazism that was passing through Europe. And they met in New York, and by the time I was born, they were comfortably ensconced on the South Shore. And I grew up in a working-class neighborhood. I was not a typical American kid. There was classical music blasting all the time at home, and we had all kinds of Austrian specialties at home that my mother would make. My grandparents and great aunts, who would come over every weekend, were speaking either Hochdeutsch or what's called Wienerisch, which is the local Viennese dialect. It really was like a German-Austrian Cafehaus that was transported to Long Island.

ZIERLER: Was German your first language?

BASSER: No. No, actually it wasn't. It was my sister's, but it wasn't mine. By the time I was born, my parents weren't really speaking much German to me. I had to kind of piece it together when I tried to figure out what they were saying when they didn't want me to understand.

ZIERLER: And, when they were escaping, they were Jewish refugees?

BASSER: Yeah. They were Jewish, and they happily left with their immediate family. But we lost many family members in Hungary and other parts of Europe who weren't able to come. And so, the reason I bring that up is because I think I was brought up with a sense of gratitude and feelings of how wonderful the United States is as a place that welcomed all these people who would've perished in Europe. And I guess it helps to explain why I'm working for the Federal Government and why I'm doing government service. My sister, who passed away, was also very public minded and did a lot of work in the public sector for the benefit of the public sector. And I think that that's an important part that sort of explains some of my later career choices.

ZIERLER: What professions did your parents find when they got to New York?

BASSER: Well, my dad was about 13 -- 12-1/2 -- when he came, my mother about the same age. My dad went to City College as a fairly young student, about 15-1/2, and he went into engineering. At that time, during the depression, it was hard to get jobs in almost any area.

ZIERLER: Yeah.

BASSER: And engineering seemed like a stable profession that you could get a job in if you had to leave on a moment's notice. And I think it was one of the reasons he went into that. My mother was a very good student. She loved sociology, psychology. She read Max Weber, and she worked in a number of jobs. She was going to get a master's degree, but I guess my father convinced her to stay at home and bring the kids up. But I think she was heading towards a city planning track and was very good in that area. It was very early on in that field. It hadn't really even fully formed yet.

ZIERLER: Now, growing up, was your family more secular? Were they Jewishly connected at all?

BASSER: Well, we went to services. We were Reform Jews. We went to services at least a few times a year for what they call the High Holy Days. And I went to Hebrew school twice a week. But I wouldn't say we were very religious. We were Reform Jews, more assimilated, I would say.

ZIERLER: And you went to public schools growing up?

BASSER: Yeah. I always went to public schools in Long Island, the South Shore initially, and then we moved to Port Washington on the North Shore and I went to public school there all the way through from 2nd grade 'til 12th grade.

ZIERLER: And when did you first start to develop an interest and express an aptitude in the sciences?

BASSER: I guess I'd always been curious about the natural world. I was always fixing my bicycle and trying to figure out how the gears worked. And I was mystified by bird flight, and I was amazed when my dad took me on my first jet airplane trip. It was just so awe inspiring that we were moving so fast, so quickly off the ground. Superballs and Slinkys and things like that I kind of always liked to play with and figure out, and erector sets. More like mechanical-engineering things. But, yeah, I think I always was—but I wasn't just interested in science. I mean, I loved sports and family time and music. I kind of thought more of myself as a jock when I was a kid as opposed to a student.

ZIERLER: Oh, wow. What'd you play?

BASSER: Well, I loved to play tackle football and baseball for a while. Then I picked up lacrosse when I was about 10, and I played that into college. And some people think that it helped me get into college.

ZIERLER: [laugh] Not just any college.

BASSER: No. I went to Harvard.

ZIERLER: Yeah.

BASSER: But I enjoyed playing that, and I made a lot of friends through that. I loved science, but I really enjoyed other things, too.

ZIERLER: What year did you get to Harvard? What year was that?

BASSER: I think it was 1976. I was the class of 1980, so that would have been the fall of 1976.

ZIERLER: So arriving in Cambridge, getting yourself settled in, did you have a major in mind when you got there?

BASSER: Well, if you look at The Face Book—this isn't the Mark Zuckerberg one. We actually had a physical face book.

ZIERLER: [laugh]

BASSER: And we called it "The Face Book."

ZIERLER: Really?

BASSER: I think so, yeah.

ZIERLER: Oh, wow! OK.

BASSER: Yeah. I think that's where he may have gotten the idea—

ZIERLER: Interesting. OK.

BASSER: —to digitalize what we had in paper form.

ZIERLER: OK.

BASSER: If you look there, you'll probably see that I had listed environmental engineering as a possible major.

ZIERLER: OK.

BASSER: I liked the math and physics, but I wanted to apply it to something that was for the betterment of the world. And I wasn’t really very interested in doing defense-related research at that time. It was after the Vietnam era, and I think that—

ZIERLER: Right.

BASSER: —I wanted to do something for society somehow, and I thought that that was a nice—that was the closest thing I could come up with at the time that blended public service with my interest in the physical sciences.

ZIERLER: Mm-hmm. So was that your major ultimately, environmental engineering?

BASSER: Well, environmental engineering didn't really exist at Harvard. They had a very small department, a very good one, but they didn't really have majors in that area *per se*. I ended up going into I guess what was called engineering sciences, but my concentration was in bioengineering. And I stumbled onto that quite late, I guess the end of my junior year. I had taken a lot of math and physics classes, but I wasn't really sure what direction I wanted to move in.

ZIERLER: Mm-hmm.

BASSER: Actually, a colleague of mine at the NIH, Cindy Dunbar, was a friend of mine as an undergraduate, and she was a TA or TF in a biology class at Harvard. And she said, "You know, Basser—" that's what she always used to call me— "you should take this class. There's a lot of physics in it." And I said, "Oh, but I'm gonna be with all these premeds, and why should I do that?" And she said, "No. You're gonna really like it." So I decided to take that class, and I really loved it. That was, I guess, junior year.

ZIERLER: What about the class spoke to you so much?

BASSER: Well, I didn't know that you could apply physics and engineering concepts to describe physiological processes. I didn't really even know what they were.

ZIERLER: Mm-hmm.

BASSER: And blood flow and pressure, flow, resistance, electrical signaling, all of these things were kind of new to me, that you could actually use physical models to describe performance of biological systems. And it got me interested enough that I approached a fellow named Tom McMahon, who was a well-known biomedical engineer, and I told him I was really interested in this area, and what should I do? And he said, "Well, why don't you take my class?"

ZIERLER: [laugh]

BASSER: So I think first semester senior year I took his class, and I really loved it. It seemed like the synthesis I had been looking for.

ZIERLER: Were you getting interested specifically in human health issues at that point, or that's still too early for that?

BASSER: You know, I wasn't thinking about human health so much, and it wasn't—if maybe somebody in the family had been ill for a long period of time with serious chronic problems, that might've had more of a personal impact on me—but no, it was not really my interest. And that class culminated in a what they called a "dog lab" where we would do experiments with a dog that was rescued from the pound, essentially, or so they explained to us that way. And we measured pressure-flow relationships, contractility, we did all kinds of things. And, at the end of the lab, they made an incision in the animal and they opened up the chest. And when I saw the lungs inflating and the heart beating and the glistening aspect of the lungs, I almost had like a religious experience. I thought this was so beautiful, it was so amazing, and I knew so little about it. And I wanted to kind of understand more about how the body worked and more about biology.

ZIERLER: Mm-hmm. Was there a senior thesis for your undergraduate program?

BASSER: No. No, there wasn't. Actually, I have an A.B. or—at Harvard they call it an A.B.. It's a B.A. in most other schools.

ZIERLER: Right.

BASSER: So I'm basically a liberal arts trained engineer.

ZIERLER: [laugh] And was the idea—did you want to pursue graduate school straightaway? Did you want to take some time off?

BASSER: I was, at that point, quite interested and motivated. And Tom McMahon said, "I happen to have a grant and a position in my lab." I said, "Well, that would be great. I'd love to work in this area." I didn't really know what I was getting myself into going to graduate school. Nobody in my family had really gotten a graduate degree, and nobody could really give me guidance about it, but it seemed like a great way to improve my knowledge about a whole range of things. And I loved the Cambridge-Harvard area, and I had a lot of friends there, and I thought that would be a great thing to do. So I applied and I got in, and I started that fall as a graduate student.

ZIERLER: Now, was the master's degree its own program or was that incidental on the way to the doctorate?

BASSER: It was incidental. If you had taken a certain—I think it was eight courses in the Division of Applied Sciences, I think they would give you a master's degree, something like that.

ZIERLER: Mm-hmm.

BASSER: I don't remember exactly what the requirements were. I don't want to misstate them.

ZIERLER: [laugh]

BASSER: And there was no Master's Thesis required for that. It was just based on coursework.

ZIERLER: And what was the basic divide between coursework and lab work for your program?

BASSER: Well, typically, students came in and they had a good idea which advisors they were gonna be working with. So a lot of people started working on research projects from the get-go, and they were taking the immigration classes: complex variables, and partial differential equations, and fluid mechanics, and continuum mechanics, and the usual engineering-physics curriculum. And then were very often spending a big chunk of their time either in the "bullpen" working with one of the theoretical scientists there like Bernie Budiansky or one of these very well-established continuum mechanicians, or one of the laboratories in the applied physics department or engineering sciences lab. That was just where I ended up.

ZIERLER: And was that your path? Did you come in with a research project in mind from the get-go, or you established that later on?

BASSER: Well, Dr. McMahon had a grant in fluid-mechanics-related projects, and I think he had targeted me for working on that. I started working initially on a project that had been started looking at the elastic behavior of tendon, and the strain energy—essentially trying to measure a strain energy function of tendon. And then I moved to this fluid mechanics project that he had a grant for, and I worked on that for a while.

ZIERLER: What did you learn about fluid mechanics and how it applied to your research?

BASSER: Well, let me just say I really didn't know any fluid mechanics when I went to graduate school. I picked up books right away, Li and Lam, and Lamb--the big Lamb--and I read through Li and Lam and did all the problem sets. And I learned fluid mechanics by being a teaching fellow in Dr. McMahon's biofluid mechanics class. So I went to the "school of hard knocks". I had a wonderful class with Fred Abernathy, who is a really wonderful, brilliant applied physicist, engineer at Harvard. And it was a more advanced class that used a significant amount of tensor algebra, and I think that grew more hair on my chest.

But one of the projects I was working on involved flow in collapsible tubes. And this was earlier on in my studies. I didn't have some of these upper-level classes under my belt. And I frankly read this article by Ascher Shapiro and I just didn't understand a word. I read the first couple of sentences and it was all Greek to me. And that was kind of a defining moment. I sort of saw the precipice there and stared down, and failure was not an option, so I basically taught myself a lot of gas dynamics and other things that were relevant. And eventually I kind of mastered that and some other papers, and that gave me confidence to keep working in that area.

ZIERLER: And how did you begin to put together your dissertation topic?

BASSER: Well, it was iterative process. I think part of it is finding an advisory team, and so a guy named Peter Griffith, he was a professor at MIT; Roger Kamm, whom I'm still friendly with, a professor in the mechanical engineering department at MIT; Fred Abernathy was on my committee. And I was just reading papers and thinking about things to do. My advisor had some notions about what I should be doing with my time. I didn't always see eye to eye with him about that.

ZIERLER: Where did you differ? What did he want you to be doing?

BASSER: Well, he wanted me to focus on a number of two-phase flow problems, which I wasn't as happy about doing 'cause they really didn't have as good analytical foundation—they were more empirical, experimentally driven. And I wanted to work on model development and using equations of conservation of mass, momentum, energy, things like that, to make predications about behavior of these systems. But it all worked out in the end.

ZIERLER: Mm-hmm. So what did your dissertation topic end up looking at?

BASSER: Well, it was actually looking at hydrodynamic instabilities, air-liquid interface instabilities, and actually dealt with, believe it or not, something that's very important in COVID-19, mucus transport in the respiratory system, and how waves form at interfacial mucus-air layers in flexible trachea and propel liquid forward, and under what conditions that happens. So now I was thinking that, after decades of stasis, all of the sudden there's a huge amount of interest in particulate matter that's shed when we speak and sneeze and cough.

ZIERLER: Yeah.

BASSER: And this has become a major public health issue.

ZIERLER: Right.

BASSER: Who would know?

ZIERLER: And yet, today, just to fast forward, you're not really involved in COVID-19 research?

BASSER: Well, I was aspiring to. I work really primarily now in medical imaging, and I was thinking of a couple of strategies that might remediate COVID-19. For instance, I had this hair-brained idea to—I can tell you this 'cause it really doesn't seem like it has any legs—but I had thought maybe you could create a light visor that projects UV-C light down to essentially create a screen, a curtain, like a mosquito zapper that would kill particles, not just COVID viruses, but flu viruses and other things. So it would essentially be a sterilizer, would create a shield around you that, so when you breathe in or cough out, you sneeze, or you're talking to someone and there's particles in the air, that it would go through this screen before it came to your face, and it would zap the viruses. But some of my smart friends at the NIH told me that you need really an excimer laser to produce the energy--the light flux--the fluence that would be required to kill the viruses. So that didn't seem like a portable and safe kind of hat that you could wear. But, unfortunately, a lot of the things that we do well in my lab don't dovetail entirely with the needs of this particular line of research.

ZIERLER: Uh-huh.

BASSER: And I'm very supportive and I've been reading grant proposals and things from other people and trying to push things along, but I haven't come up with anything myself, unfortunately, to try to remediate the situation.

ZIERLER: Well, we'll stay tuned for that. So getting back to the dissertation, what did you see as your primary contribution with your research, and in what fields did you feel like you were contributing mostly?

BASSER: Well, I think it was a way of extending notions of fluid mechanics flow in collapsible tubes into a domain of two-phase flow, and looking at different ways in which momentum is transferred from gases to liquids and instabilities. I think those are all areas that I had read about, worked in, written about. But it wasn't really something I ended up staying with, and certainly at the NIH I found my own voice and started to work on areas that were, I think, closer to my core interests. But this was certainly a good discipline that introduced me to many kinds of mathematical modeling approaches, to a whole range of physical science methods, and taught me how to think about problems and how to structure research, so I think that was the major benefit.

ZIERLER: And what year did you defend?

BASSER: I think it was 1986.

ZIERLER: OK. So 1986. And then what are you thinking afterwards? What are your next options?

BASSER: Well, it's an interesting question. I had actually done some consulting when I was in graduate school. I worked at a company called Medi-Tech for a while, and they make catheters for a variety of applications. They were a very small company and I think it was in Watertown. Now they're huge. But I worked with John Abele, who was, I guess, the President. He had a little lab in the back of his office, and I worked with some of the people there designing catheters, and that was interesting. Then I started working for Hewlett-Packard in Chelmsford, and I had met some really wonderful people there, Ed Merrick, who was kind of a corporate-type mentor.

ZIERLER: Did Hewlett-Packard have a basic research component to it?

BASSER: Well, they had a Medical Products Division and they were developing medical devices that they would sell, pressure transducers and things like that. It wasn't like Hewlett-Packard labs, but I think there was always research going on at that time in their various divisions. And so I had seen academia and I had seen industry, and I actually had been doing consulting. And I guess the third thing that I hadn't tried was government-type research. So I thought that that would be a good thing for me to start out doing when I left graduate school. And I had met John Rinzel, who's a wonderful biomathematics person, at a Gordon Conference, and he told me that the NIH had a great biomedical engineering program. And I got in touch with somebody named Seth Goldstein, who was the head of the mechanical engineering section down at NIH, and arranged for an interview. And he told me that he'd like me to be a Staff Fellow in his program. And I thought it was a—there were some very smart people down there, and I thought that that was a really good—potentially good environment for me.

ZIERLER: And that was not a postdoc that was a long-term opportunity?

BASSER: Well, it was a postdoctoral position, but it could have led to a long-term staff position down there. That didn't turn out to be my goal, but I was working down there doing biomedical engineering research in what was called the Biomedical Engineering and Instrumentation Program in the Intramural Program at NIH. And I guess I started there in '87. I think I may have been hired in '86, but I think I showed up there in March of '87.

ZIERLER: Mm-hmm. And so what were some of the projects you were working on initially?

BASSER: Well, I worked on a magnetic stimulation—I became interested in gels and gel swelling. And I thought that polymer gels were really an interesting kind of form of matter, I guess, that had a lot of tissue-like properties, and I was quite interested in that. And I was interested in mathematical models of nerve excitability, and I had met a man named Ichiji Tasaki, who was a very well-known neuroscientist. He was already in his late 80s when I met him. And I became fascinated with his nerve preps and the way that he was thinking about nerve excitability. I started working with Bob Dedrick and Paul Morrison, who were chemical engineers, and Bob and Paul had invented something called—what's now called—convection-enhanced delivery, in which you deliver drugs primarily to the brain, overcoming the blood-brain barrier, which would often prevent drugs from actually getting into the tissue that they were supposed to go to. They couldn't cross the blood vessels into the brain, and, as a result, they caused systemic toxicity and didn't kill any of the tumor cells. So they had the idea to drill bore holes through the skull and pump chemical agents, chemotherapeutic agents, directly into the tumors and obviate or avoid the blood-brain barrier and concentrate the drugs regionally where they needed to go and not cause this systemic toxicity. And they didn't really understand kind of the porous media pumping aspect of this very well, and they brought me in to try to maybe make some sense out of some of their results. I thought this was really interesting, and I developed a poroelastic model of infusion into brain tissue, which predicted the distribution of the drugs and the fluid-pressure drop and velocity distribution. And that was really a fun project that involved mechanics and fluid mechanics at the same time.

And with Brad Roth and Mark Hallett, I had stumbled on this new technology at the time called magnetic stimulation, which now is called TMS, transcranial magnetic stimulation, that's used all over. At that time, Mark Hallett, who is a neurophysiologist and neurologist at the NIH, had just gotten one of the first devices to try to make sense out of how it worked. Because, mysteriously, you'd put a coil near the motor strip and you'd pass a current through that coil, and it would somehow cause your arm to twitch or your leg to twitch, and the whole physical mechanism involved wasn't really understood. And my friend and colleague, Brad Roth, who was also in my department, and I developed a mathematical model to describe how the electric fields that were produced by these coils actually caused currents to flow across the nerve membranes, or the axonal membranes, and trigger an action potential. So it was really a nice biophysical model that explained something quite mysterious and put it into terms that were testable with parameters that could be measured. And I think it was very nice project to get my feet wet on. So the biomedical engineering department didn't really have its own independent funding, so we looked around the campus to find people to work with who were doing interesting things, and that was just great for me because I was very curious and wanted to find out what people were doing, and looking for interesting questions … problems.

ZIERLER: So, in those early years, what were your initial impressions of the culture of collaboration at NIH, in the way that creativity was valued?

BASSER: I think creativity had always been highly valued. I will say that some people with physical science backgrounds at that time were not always as valued as people with biological science backgrounds.

ZIERLER: Mm-hmm, mm-hmm.

BASSER: I would say sometimes we were considered second-class citizens, so I would meet somebody and then I would say, "Well, I'm in the biomedical engineering program." They'd say, "Oh, you're repairing the respirators or the infusion pumps?"

ZIERLER: [laugh]

BASSER: I'd say, "No. I'm actually a PhD and I'm doing research and thank you very much."

ZIERLER: [laugh]

BASSER: So there was this—

ZIERLER: They thought you were the repair tech or something like that?

BASSER: The repair guy, yeah.

ZIERLER: Yeah. [laugh]

BASSER: So biomedical engineering and physical science hadn't really taken hold—there were pockets of physical science there, and there was wonderful examples, but we weren't considered to be on par or equal, sometimes, to our colleagues.

ZIERLER: To fast-forward to the present time, to what extent has this trend remained the same and how much has it changed?

BASSER: I think things are oh so much better now.

ZIERLER: OK.

BASSER: I think that with the creation of the National Institute of Biomedical Imaging and Bioengineering, that Rod Pettigrew was the first director of, and now Bruce Tromberg is running, I think they did a magnificent job leveling the playing field. But a colleague of mine just retired, and he recounted a story along these lines of this sort of bias against physical science. He had been interested in muscle physiology and he went to a muscle physiology journal club, and his turn came to present a paper, and he read this paper and he thought it was very nicely written, he said, but it violated Newton's second law, F = ma. And a very famous muscle physiologist, whose name I won't repeat, sort of turned sideways to Richard and said, "Well, why do you think Newton's laws have any place in biology?"

ZIERLER: [laugh] That's amazing!

BASSER: Yeah. There was this sort of—

ZIERLER: And he was serious?

BASSER: He was serious, yeah, that somehow there was an exemption for the laws of nature.

ZIERLER: [laugh]

BASSER: And I think there were culture differences that were hard to bridge. But one thing, I came to learn how brilliant my colleagues were and how many good ideas they had, and I learned to speak their language. I think it was very important for me to kind of find out what other people thought important problems were, not just to stay in my own head and to think about what people who trained me thought were important problems. And I think it was really an important growing experience for me to circulate around. But we weren't really considered to be the elite science group on campus, even though we had some wonderfully talented people.

ZIERLER: Mm-hmm. And how long did you stay in that initial position at NIH?

BASSER: Well, I stayed in the—this was a group that had been run by a man named Murray Eden. He was the head of the biomedical engineering program. I guess I—in 1998—I moved to the National Institute of Child Health and Human Development. Art Levine had brought me in. He recruited me, essentially, to come into his Institute. So I guess I was in the biomedical engineering program for about 10 years.

ZIERLER: Mm-hmm. And then, what compelled you to move over? What was exciting about this new opportunity?

BASSER: Well, for one thing, it was a path to become a Principal Investigator, instead of working with almost no budget in more of a service position.

ZIERLER: So it was not tenure track? Your first ten years was not a tenure track?

BASSER: Well, there was a kind of a tenure. I mean, there was a job permanence there, but it wasn't a tenure track in the NIH or university sense.

ZIERLER: Mm-hmm.

BASSER: And that wasn't so important to me when I first got there, but I saw that there was a big difference in terms of the research trajectories and opportunities people had when they were PIs. And the work that I was doing already I think merited it. By that time, I had invented or largely invented diffusion tensor imaging, which is something I'm probably best known for. And it was quite widely used already. In 1997, there was a lot of interest in what we had been doing and developing with my colleagues, Denis Le Bihan and Jim Mattiello, already had significant interest, and I think that it seemed as though the things that I was working on, the resources that I would need, the kind of investment that would be required was sort of incommensurate with what could be provided in the biomedical engineering framework there.

ZIERLER: Mm-hmm.

BASSER: So it's not to say I outgrew it. I didn't. I mean, the people who were working there were wonderful and brilliant. But the level of resource support was really meager.

ZIERLER: So if you can explain with tensor imaging, what was the basic problem and what solutions did you offer?

BASSER: Well, at the time, I kind of stumbled into this field. People had already known how to measure the diffusion coefficient of water and tissue, and they knew how to combine that with MRI. Paul Lauterbur, actually the inventor of MRI, explained in his 1973 paper that you could combine the imaging, he called it zeugmatrophy, with NMR measurements that had previously been developed, spectroscopic measurements to measure chemical compositions and he mentioned specifically diffusion, but he didn't show how to do it. And then, about a little more than 10 years later, several groups combined and showed how to join MRI with the measurement of water diffusion so in each pixel or voxel within an imaging volume you could measure and map water diffusivity. The problem as that, in many biological tissues, nerves, skeletal muscle, in the brain white matter pathways, the diffusion of water was not isotropic, so a single scalar diffusivity just wasn't sufficient. And the people in the field frankly were stumbling around not really knowing how to describe that anisotropic diffusion process. And, for me, it was a problem that fell into my lap. I mean, I basically ate and breathed tensors in graduate school. I taught mathematics classes, and I was very comfortable with Eigensystems, and rotations and invariants etc. So this problem kind of fell into my lap when I stumbled on a poster that my colleague, Denis Le Bihan, had presented showing that they could characterize some aspects of anisotropic diffusion in brain tissue. And, because I had worked in these areas like flow in the brain and, obviously, when fluid is pumped into the brain in convection-enhanced delivery, it'll flow along the white matter pathways and with greater speed, and in an anisotropic way electrical fields and currents will flow along fibers as opposed to perpendicular to them. So anisotropy was sort of in the back of my mind anyway. So when I stumbled across this poster, I realized that this diffusion measurement could be expanded, and you could measure all of the different components of the diffusion tensor, and if you did, you could characterize this very important transport process for the first time. And it really hit me like a lightning bolt, and I basically dropped everything I was doing and I kind of figured out how to make these measurements. And worked with Le Bihan and later Jim Mattiello, who was also a postdoc in my group.

ZIERLER: And why do you think this research caught on so widely and so quickly? What was it about what you were working on?

BASSER: Well, part of it has to do with the fact that diffusion imaging was the first methodology that allowed clinicians to see a stroke in progress.

ZIERLER: Mm-hmm.

BASSER: Strokes were mysterious and there was no radiological means to be able to see the ischemic tissue.

ZIERLER: Mm-hmm.

BASSER: And Mike Moseley, who is now a friend of mine at Stanford, was at UCSF at the time, and showed with some of his postdocs like Yoram Cohen, that when you cut off blood flow to a certain part of the brain, let's say the—all of the MR parameters that you typically measure, T1, T2, proton density, no change that you could really see that's appreciable. But the diffusion signal, it dropped like a rock. You'd see a 30, 40% decline in the apparent diffusivity. That created a tremendous amount of interest in the clinical neuroradiology community that, for the first time, you could actually see a stroke in progress. But the anisotropy created a confound for radiologists because, if you happen to be looking at displacements in a particular direction and those directions were parallel or perpendicular to nerve fibers, you get different contrasts. So it was very difficult to interpret those images. You would see a drop in the areas where the stroke was happening, but you'd also see a lower signal in regions that just had fibers perpendicular to the gradient direction that you were playing out in your imaging. So you'd see a hodgepodge of dark and light in these images, and one of the things that DTI did was it cleaned up that orientational artifact entirely and all you saw was the region where the diffusivity dropped because there wasn't any blood flow to that area.

ZIERLER: Mm-hmm.

BASSER: And that was a very important, I think, clinical driver. And there was a lot of interest in diffusion in general 'cause it was a new parameter and people were thinking that—the next thing, too, is that you could visualize nerves and white matter pathways in the brain which, before, were kind of invisible. And you could also see their orientation because the diffusion tensor allows you to find the three principal directions of diffusion. And one of them is that the direction of maximum diffusivity most of the time coincides with the directions that the fibers are running in. So you could, for the first time, see fibers, map them out, and actually follow their trajectories. And back in 1992, I also talked about, in one of my abstracts to the ISMRM about the fact that you could follow fibers like "link sausages," you know, ellipsoids that would string together like Wienerwurst that I used to eat as a kid.

ZIERLER: [laugh]

BASSER: That's the tie-in to my Austrian background. That you could actually follow pathways. And you see these pathways now all over the place, on TV and YouTube, brain maps that show where axons emanate from and where they terminate. So I think that the neuroscience community also found this information very, very interesting and helpful in terms of understanding brain architecture and organization. So that was another driver of why this became a bigger deal. I think that partly answers your question about what drove interest in this. So it was part clinical and I would say part neuroscience, so it's sort of a long-winded way of getting there, but—

ZIERLER: And so, when you moved over, did you leave that research behind or did you continue on in any way?

BASSER: The diffusion imaging work or the—

ZIERLER: Yeah.

BASSER: No. Happily, I was able to hire Carlo Pierpaoli, who is a neurologist and wonderful scientist, and he was much more interested in translational aspects at the time. So we worked closely together and kind of divided the world up. He was working a little bit more towards translational imaging. And I was doing more of the modeling and other things at that time. And I was developing applications for him to translate. And so for a long time we worked in that mode. So, no, I was working very regularly, assiduously on diffusion imaging applications. Hold on for a second.

ZIERLER: Please. [unrelated conversation] Peter, if you need to take a quick break, there's no problem. Life is happening all around us. It's fine.

BASSER: My daughter wants me to make some hotdogs for her.

ZIERLER: [laugh]

BASSER: It won't take too long. But I bet that there's somebody else who could help her, though, so I'll roll the dice on that one.

ZIERLER: [laugh] OK.

BASSER: So go on.

ZIERLER: So what other new projects did you take on at this time?

BASSER: Well, I had been working with Alice Maroudas, who's a very wonderful chemical engineer and physical biochemist from Israel who did a lot of great work in cartilage. And cartilage is a kind of gel. And, as I told you, I was interested in gel sciences. And I worked with Alice in the mid-'90s, before I started working at Child Health and Human Development. We developed a physical modeling and experimental framework for measuring features of cartilage that explain its load-bearing properties.

ZIERLER: Mm-hmm.

BASSER: And, to make a long story short, cartilage consists of proteoglycans that are highly-charged polymers that swell, encapsulated or entrapped within a collagen network that doesn't like to that is like a nylon stocking and it just doesn't like to expand. And it's that yin-yang kind of tension between those two components that give cartilage its remarkable load-bearing ability in making a very stiff material out of very soft, flexible components. And that prestress turns out to be a very important property. And when I started my own lab, I wanted to have a polymer science component that would look into problems like cartilage prestress. Tissue engineering was a very big field at the time, but I felt that it was a little bit immature scientifically -- engineers usually don't build things until they understand the physical or whatever scientific principles that underlies the things that they're building. And a lot of my colleagues were putting together matrices to serve as scaffolds for growing cells, and I can't say that they knew the local "rules of the road" -- the physical principles that underlie the basic biological function of those tissues and how they grow and what are the requirements to make them grow. So I created this lab of tissue sciences, or a section that involved tissue sciences and biomimetics to try to provide this scientific background that could help guide tissue engineering.

ZIERLER: What does biomimetics mean?

BASSER: Well, to different people it means different things, but in my mind, it's the use of simpler maybe synthetic systems that mimic critical features of biological tissue that give you a way of understanding the physical "rules of the road" for how those more complex biological systems work. As an example, my colleague, Ferenc Horkay, and I just wrote a paper in *Scientific Report* describing a synthetic composite polymer, which mimics cartilage's load-bearing behavior. So it consists of polyacrylic acid microgel beads, which love to swell in water, entrapped in a polyvinyl alcohol mesh that prevents that swelling from occurring. And Ferenc showed that he could actually match many of the cartilage's load-bearing properties using these synthetic polymers. And now we can talk about crosslink density and charge density and composition of the different components, and use that in models to explain the load-bearing ability of the tissue. And that level of understanding doesn't really currently exist for cartilage. So it gives us an anchor, a way of using scientific principles to get at a critical functional property of tissue. I mean, mathematical models obviously help you do that, too, but physical models can be helpful, as well, especially complex systems like these where you have polyelectrolytes and polyelectrolyte gels and crosslinked polymers. There are a lot of variables and a lot of "moving parts." So at least it gives you a starting point and helps you ask the right questions: What happens in aging? What happens in development? Are crosslinks lost; are they gained? What happens when you change the charge density? What happens when you change the branching of the polymers? And it gives you a physical way of dealing with a biological problem.

ZIERLER: And how much are you thinking specifically about the potential clinical value of the research? Is that a separate consideration or is that sort of baked into the research from the beginning?

BASSER: You know, for me it's been an evolving process. When I first got here, I wasn't as wired towards translation, but that has become an increasingly important and, frankly, satisfying part of the research, that people can really benefit from it and use it. And there's been more of an emphasis on translational research. I mean, you really have to connect the dots between the basic research that you may be doing and the benefits that it may have down the road towards public health.

ZIERLER: So you're saying that this is a broader trend and your own experience is representative of that trend?

BASSER: I think it's a broader trend. There are many basic scientists at the NIH, and I think all of them aspire for their work to have a clinical benefit. Sometimes it's so basic that, in their lifetime, it doesn't necessarily happen. But I think that there's more emphasis on translation and—

ZIERLER: Peter, I'm sorry to interrupt, but I'm curious, what do you mean it's so basic that it doesn't necessarily happen in their lifetime? I'm curious about that. What does that mean? Like, foundational?

BASSER: Yeah. Well, foundational, but, you know, sometimes you do something and—well, as an example, if you do these coughing sort of physics problem, there was really no clinical benefit or understanding that came out of that, or at that point it wasn't useful information. Maybe it was for a very small number of people. But sometimes you discover something or you're working on a system that's very abstracted from the real clinical world, and it may not make its way, it may not percolate up to being used and exploited. Or it may be too difficult, or there may be reasons why things are "lost in translation." But I think that scientists that I work with really want to make a difference, not just in basic science but they want to be able to help people in either improving diagnostics or therapy in some way.

ZIERLER: Mm-hmm. So speaking about your own experiences and how you've developed this interest about the clinical value over time, what do you think explains that? Is part of it confidence in your science? Is part of it maturity and wanting to make a more concrete difference or what—it seems to me so obvious that scientists want to make a difference. Why shouldn't that be sort of part of the equation from the beginning?

BASSER: Well, I think all of those things. I think maturity is one of them. I mean, you can see by the half-white on my beard that I'm older than when I started out, clearly.

ZIERLER: [laugh]

BASSER: But I think, living in the world and seeing people affected by problems, and understanding that you can make a difference. I mean, I have a lab, I have resources now that I can deploy. It’s not just one person working trying to do his own thing. And I'm part of an Institute that has a mission, which I strongly believe in. And I think it's a lot of different factors. You raised a couple of them. But I didn't come into this bioengineering, biophysics-type research with the idea that I would create some medical devices, necessarily. And there's a tremendous amount of work that can be done—you interviewed Hari Shroff. It's possible that most of the things he does probably are not moving in a clinical direction but they're really breakthrough inventions that help neuroscientists and cell biologists discover things about the systems that they're working on, be able to see cells dividing in real time, light sheet microscopy. These are tremendously important, but they may not result in clinical devices, therapies, diagnostics anytime soon.

ZIERLER: Mm-hmm.

BASSER: And I'm not sure that it's driving everybody. They want to be able to make great measurements and help understand biological and physical systems at a deep level, and that's sometimes its own reward.

ZIERLER: Mm-hmm.

BASSER: I happen to be at an institution where you're also rewarded if you do something that has clinical potential and is used to help people.

ZIERLER: Now, when you single out your own Institute, is that to suggest that that's not true across the board, or at least it's valued differently at different Institutes?

BASSER: No. I would say that largely the NIH is really interested in improving the health of the nation. And I think all the different Institutes pretty much are reading from the same page about that.

ZIERLER: Do you have opportunities to collaborate with MDs or even see patients or understand what's happening really at a direct level in terms of the things you're studying?

BASSER: Well, many of my collaborators have been MDs, and that's one of the reasons, too, I guess culturally why I find translational problems more interesting, because I have friends who are radiologists and friends who are neurologists and friends who study the kidney, things like that. So I typically am not walking with physicians when they see patients, but I go to grand rounds and I hear about clinical problems routinely. We have many, many lectures—well, not right now because of COVID-19, but usually we have 50 lectures a week.

ZIERLER: Right.

BASSER: And I try to catch them, and I would say half of them are clinically oriented.

ZIERLER: Right. And what about patients, do you ever have the opportunity to interface with patients?

BASSER: Not really. I don't. We do scan subjects sometimes. I'm involved as an Associate Investigator on some clinical protocols, but I don't have patient responsibilities. And I think it would be a lot of fun to do that, but I'm not really involved.

ZIERLER: Is that because there just isn't the appropriate opportunity?

BASSER: Well, in clinical protocols, different people have different roles to play. So the physicians will deal with the patients directly, and the people who are, let's say, designing MRI pulse sequences or analyzing data don't necessarily interface with patients. Frankly, it's not something that I've pushed hard about, and I know that there are investigators with my kind of background who have more contact with subjects and special volunteers than I do. But it just hasn't—that's just not how it's played out.

ZIERLER: Have you directed research towards cancer research?

BASSER: Yes. In fact, we have some problems that we've been looking at relating to the application of electric fields to cells and tissues, and whether they can help cells grow or, conversely, whether electric fields could be used to kill cells, particularly rapidly-dividing ones. And we have had an involvement in that area. It hasn't been a major focus, but I would say it's been a back-burner problem for me for a while.

ZIERLER: And I wonder if you could talk about your position as Associate Scientific Director at the institute. What does that mean, both administratively and scientifically?

BASSER: Well, it's a position that was created by the previous Scientific Director, primarily it's an advisory position. So we might be asked to give advice about hiring priorities, if capital equipment requests are made we might opine about whether one capital equipment request is better for the Institute than another. We sometimes provide guidance about patents and intellectual property. It's really an advisory role more than anything else, but we do get together about once a month, now more frequently over the internet because this is a rapidly evolving situation. We're also put between the Director and the—we have about, I don't know, 75 or 80 principal investigators in my Institute alone--and we sometimes deliver the good or bad news to the Principal Investigators, maybe in sometimes a more efficient way so that the Director doesn't have to do it individually for so many people.

ZIERLER: Uh-huh.

BASSER: So really it's an important role, but we don't have a lot of line management authority.

ZIERLER: Mm-hmm. Well, Peter, I'd like to ask now some questions that might take a more retrospective approach to your career. And the first is, of course, I'm coming from the perspective of physics, and I wonder if you can talk a little bit about how basic concepts in physics or, you know, that amazing story about Newton's second law and how it doesn't apply here, things like that—I wonder if you could talk a little bit about how certain laws in physics are either close to you, you have a particular affinity, or they really inform the way that you see how your research should proceed or understand the data that you're coming up with?

BASSER: Well, I think I'm very influenced by my training at Harvard from Max Krook and all these people, applied mathematicians, George Carrier, Bernard Budiansky, and a whole host of folks who drilled continuum mechanics into my head. And, fortunately, that stayed with me. So in biology, it's interesting, you see a lot of cartoons in journals, picture of how systems might work, and I always try to subject those cartoons to the constraints of conservation of mass, momentum, energy, Maxwell's equations …. And sometimes plausible pictures that you can draw in a journal don't really work, 'cause they don't really—like, Richard, my friend, had shown--satisfy F = ma or some other law.

ZIERLER: Right.

BASSER: So I carry with me this continuum mechanics worldview, which helps me because it allows me to sometimes separate the wheat from the chaff. That's not to say that cartoons aren't important, and they sometimes help organize your thinking about something. But that discipline, I think, has been very helpful to me, this basic continuum mechanics perspective in fluids and solids and the way electric fields interact with matter. Polymer physics I'm somewhat self-taught in, but I think that's a beautiful subject. And I work with people like Ferenc Horkay and Jack Douglas and others, and I've learned a great deal from them. So I think those perspectives are very important for me.

ZIERLER: Mm-hmm.

BASSER: In MRI, actually the way in which diffusion is described, quantum mechanics is obviously the foundation of a lot of magnetic resonance properties, but continuum mechanics actually gets you very far in terms of looking at transport processes of magnetization. And so, instead of looking at water flowing, now you're looking at a magnetization vector. And there's diffusion and there's convection, and then there might be advection, and there's creation or annihilation of magnetization. So you have, again, these concepts of conservation of mass, momentum, conservation of energy just keep coming up and in different ways. And those have been very helpful in organizing my thinking in this area. I don't know if that's an answer.

ZIERLER: No, it's great. Absolutely. It mostly definitely does. I want to ask, the research that you've been involved in, it's remarkably diverse in terms of the kinds of projects and the kinds of health issues. And I wonder, are there any projects that stand out in terms of the satisfaction that you've derived from them, not necessarily the ones that you're the most well-known for, although that might be the case, but are there any that really stand out as being, like, really personally meaningful to you?

BASSER: Mm-hmm. Well, in some ways, you become connected sometimes with these problems when you're working on them, not to say you develop and emotional attachment, but you're spending a lot of time thinking about them. I would say the convection-enhanced delivery problem and the problem that I worked on with Brad Roth sort of discovering or modeling how induced electric fields cause nerves or axons to fire are—I'm very proud of those because those are really complete works. And we took things that were—particularly magnetic stimulation, something that was mysterious, that was almost like magic, and explained them and gave people a way of making a lot of progress in the field.

ZIERLER: What do you mean by a "complete work"?

BASSER: Well, that there were experiments and theory, and the theory and the experiments were dovetailed. And we developed a theory and then we made predictions, and then we tested those predictions. And I think the interplay between modeling and experiments is something that gives me a lot of satisfaction that when you can describe something mathematically and then you can measure—now, your predications aren't always correct, and then you have to go back to the drawing board. But this iterative process of modeling and experiments I think is very exciting. And, when that happens, that increased my level of emotional attachment. Let me put it that way.

ZIERLER: Mm-hmm.

BASSER: But, I don't know, I think I really liked all the things I worked on. I probably wouldn't have worked on them if I hadn't.

ZIERLER: And that's a good segue to my next question, which is, particularly in your role as Associate Scientific Director, I'm sure you're all too well aware of the limitations of resources, either time or budgetary, and so, by definition, there's just too much to work on to take all of it in at the same time. And so I wonder if you can talk a little bit about your decision-making process in choosing a particular research project to accept, right? What are the narrative through-lines that might connect—as diverse as all of your projects are, what they all share in common, obviously, is that you said yes to do all of them, right? So what's that commonality that connects all of these in terms of why you wanted to accept these projects and not others?

BASSER: Well, when you say "accept projects," I just want to be clear that it's not that people handed me things, it's—

ZIERLER: No, no. I mean commit. When you commit—

BASSER: Yeah.

ZIERLER: —to a particular project.

BASSER: Well, that's something I think about quite a lot. It's a great question. Because there is almost an infinite number of things you could be doing.

ZIERLER: Right.

BASSER: So the filters that I very often put things through are significance; is this a really important question or fundamental phenomenon? Is this knowledge really important and needed? More and more, is there a kind of clinical translational payoff? Can somebody really benefit from something like this? There has to be—again, going back to continuum mechanics, if there's a modeling framework or if there's a experimental framework that could be well established, that's very important. Or could we build new instruments or invent new things that would help us get there? Collaborators is also important. I mean, you can't do everything yourself. So are there people out there whom you can work with who are trusted, whom you could develop a research relationship with? I guess there are a lot of things that I've done that are obviously not in my comfort zone. So I'm not always thinking about—

ZIERLER: Yeah.

BASSER: —gee, whether—like with NMR, candidly, I had taken a high-resolution class with Ted Becker, but I didn't know anything really at that point about MRI. And I just jumped into it 'cause it seemed like such a beautiful problem, so interesting. I figured I could figure it out. But I actually have a list of—it must be about 10 pages long, single spaced—things that I want to work on that I probably—most of them will never get to.

ZIERLER: [laugh]

BASSER: And another thing that I do is I keep looking at that list, and I've been doing it less and less, but I used to look at that list and see which things I really wanted to focus on. And the good problems kept on bubbling up to the top.

ZIERLER: Uh-huh.

BASSER: And I kept looking, gee, is that a great problem? Is that something I should be working on? Fred Abernathy, who was one of my mentors at Harvard, a fluid mechanician, said "the question is critical." And he understood that you could start working on something, it may not be such a great problem, but you end up working on it and 10 years later you're still working on this not-so-great problem. So I look a lot at the question, significance, importance, possible impact; is it a fundamental problem? Could it go in many different directions? Can I assemble the resources to work on this? A lot of things come to mind.

ZIERLER: And so the next level of that question, asking about how you commit to a project in the beginning, is what are the feedback mechanisms that let you know that the project that you're working on is worth continuing, that it's going in the direction that you want it to go and that it continues to deserve the resources that you're putting into it? Or, alternatively, when do you know—if this has ever happened to you—when do you jump ship because there's other stuff to work on and you kinda hit a wall with this?

BASSER: Well, I'm a very stubborn individual.

ZIERLER: [laugh]

BASSER: I mean, I think of it as being persistent, but some people might call me stubborn. I keep subjecting the work that we're doing to the same kind of questions about significance and importance. And once you start doing something, it's easy to do--if you've done N things in that area--to do the $N + 1^{th}$ task sometimes doesn't require a lot of energy. But I am looking around at other things that we could work on. I think reinvented the lab and myself a few times in the last two decades. Now, for instance, we're working on pushing the envelope in NMR to look at objects that are much smaller than people thought that we could probe with conventional displacement imaging methods. And I think that that's a hugely important problem and could have great impact in cell biology, and even in subcellular biology problems, looking at organelles, … looking at mitochondria. So I've turned the ship to work on problems that are more along those lines and have not continued developments in some of the more mature areas that are going on their own. That's another thing you have to consider. Sometimes you do something, and a lot of labs pick up what you've been doing, and you don't have to continue to work in that area.

ZIERLER: Right.

BASSER: The work gets continued, and sometimes in strange and beautiful ways. And you don't have to keep controlling everything that comes out. So I would say I am looking forward still towards possibly important findings and developing important methods, inventing new stuff. I get a little restless sometimes—I like to invent stuff. I like to develop, make new measurements, and that's always fun to do. And if I can think of something, that's a nice way to spend my time, obviously!

ZIERLER: So this might be a difficult question given how diverse your research interests are, but I wonder, if you could reflect from the beginning of your career to now, what is something that jumps out at you that you truly feel like you understand now that you didn't when you started?

BASSER: You mean a physical process or—

ZIERLER: Or just the way the human body works, physical process, something that was really mysterious you just didn't get, and now you feel like you really understand what's going on, you really have a handle on it.

BASSER: You know, I'd have to think about that. Nothing really comes to mind. I mean, as I've worked on problems, I've come to understand better the framework around them and have been able to see things maybe from a 30,000-foot perspective. But I'm not sure I can answer that one right now.

ZIERLER: OK. Maybe we can come back to it, maybe not. What about your style as a mentor, either with postdocs or with younger colleagues, what are the kinds of constants in terms of advice that you see yourself giving people that are coming up in the field in terms of the way that they should go about doing their work, the kinds of things that they should focus on? What kind of advice to you give your younger colleagues?

BASSER: That's a great question. I often tell them about my own thought process, about how I approach problems. First of all, I listen to them a lot. When they first come, I want to find out what they're interested in, are they thinking about going into industry or do they want to become professors. And I listen to them talk about the things that they like to do and not like to do. And I try to pick projects or suggest projects that they might want to start on that seem to me to be a good fit for them. And I'm not a micromanager. You could ask anybody in my lab. I don't go over to everyone's office four or five times a day and ask them, "Have you done this, have you done that"? I want people to work on projects that are important and move the ball forward and have all of these nice features that are mission critical, etc. But I want them to really enjoy it and move in the direction that their fields—where they're developing expertise in particular areas that they may not have had before. So, you know, you might want to ask some of my postdocs and graduate students.

ZIERLER: [laugh]

BASSER: Yeah. I mean, I sometimes tell war stories, issues with certain publishers or publications, with editors, reviews. There's always a review that comes back that's scathing and I try to really deal with that. Oh, I had that happen to me so many times. You just have to pick yourself up, and sometimes reviewers just get out on the wrong side of the bed or they don't like something for a particular reason, and it's not an indictment about your work. And it isn't. And you develop a certain judgement, maybe wisdom over time and don't take things as personally. And I think a lot of people are also wondering, “Am I good enough to do this or will I be able to run my own lab”? And also try to involve people in grant writing, even though NIH doesn't have a grant-writing culture. My lab does—we are involved in a lot of grants with agencies like the Defense Department or with the NIH BRAIN Initiative, and I try to involve postdocs in the grant-writing process, and giving them the confidence and the management wherewithal so that when they run their own lab they're gonna be able to write their own proposals and they'll know what to do. So I try to make them fully enabled to run their own show, if that's what they want to do. Some people go off to industry, some people do other things. But I really let them determine what their interests are, and I try to help them get there.

ZIERLER: Mm-hmm.

BASSER: So it's a hard question. I mean, there isn't a general rule, but I joke with people in the lab that coming to my lab is a little bit like the mafia. You know, you never leave, you're always—

ZIERLER: [laugh]

BASSER: So I would say overwhelmingly that's true. I have very good relations with people who were former postdocs and who were trained in the way I've been—almost weekly I have conversations with people who are now assistant professors, associate professors, running huge imaging centers in different parts of the world.

ZIERLER: Yeah.

BASSER: And I feel very close to them and very proud that they've achieved so much. So I hope that some of their success can be traced back to some of the things that—

ZIERLER: I'm sure it can. Those treasured instances of real scientific discovery, when you've come to the conclusion that you really—we've talked a little bit about that, and on several of your research projects, where you feel like you've really pushed the ball forward, that you really have advanced the science, right? I wonder if—obviously, the ingredients to that are there's the hard work, there's the grind, there's the luck, there's the insight or genius even, or bursts of genius, and then there's always the advances in technology that allow all those other things to happen. So I wonder if generally you see a rough breakdown of those ingredients in terms of the relative importance of each of them in contributing to these moments of discovery?

BASSER: Well, that's a great question. I mean, the moments of discovery really are quite unique, because there is a "Eureka moment," and it's really great when it happens.

ZIERLER: Have you experienced that, a real eureka, like, Hollywood moment, aha!?

BASSER: Yeah, yeah. I mean, when I was standing in front of Denis Le Bihan's poster, when I learned about diffusion imaging and figured out that you needed to use this tensor formalism, a whole bunch of ideas went racing through. It really was exhilarating.

ZIERLER: Mm-hmm.

BASSER: But that wouldn't have happened if I hadn't been hitting the textbooks, and taught fluid mechanics to graduate students and undergrads, and hadn't done all these problem sets. And there's a lot of grunt work that made it possible—other people looked at that poster, too. In fact, people looked at a lot of articles about this and it didn't occur to them. So some of it's just preparation, some it's luck, putting yourself in an uncomfortable situation, reading a poster that you maybe don't understand and sitting in front of it and get somebody to talk to you about it. I don't know. I'm not sure that I'm addressing your question, though.

ZIERLER: On any one of them, if it's basically like a pie chart, I've logged in the grunt work and the sparks of insight and the technology, right? Do you see all of those moments of discovery are very unique in and of themselves, but are the rough—

BASSER: Yeah.

ZIERLER: —proportions of each of those elements—do they usually play out the same in any given moment of discovery?

BASSER: Yeah. Well, for instance, when I was working on the problem with Brad Roth trying to explain how induced electric fields might get nerves to fire, I had spent a lot of time reading a book by Schwan on bioelectric phenomena. And there was an equation that I had remembered, a partial differential equation, it was basically a transmembrane potential equation where there was a source term. And I showed the equation to Brad and I said, "I think what's happening is that this source term, instead of being a transmembrane current that's caused by an electrode, is caused by an electric field gradient." And he said, "Yeah. I know how to do that." And he knew how to write the equations—the cable equations--in intracellular and extracellular spaces, which produced the new source term. And so part of it is just being prepared, having a knowledge base and fitting together the knowledge that you have or that you could aspire to have with what a question requires. And I think that that's also true with the convection enhanced delivery problem, that I recognized that that was a poroelastic flow problem. That's a framework that was used by soil engineers in the '20s. It wasn't really widely used in tissue modeling, but you had a fluid flow and then you had deformation of the surrounding network, and they were coupled together. I had been reading a book by Baer on—it wasn't poroelastic but porous media and porous media transport--and it made sense that that was the right approach to use. And so you prepare yourself, then you stumble on good problems or good opportunities. And you might have the resources to approach it … or not.

ZIERLER: Mm-hmm.

BASSER: But I guess being interested in learning about a lot of different areas can be helpful. And you never know where something you know could be useful in the future.

ZIERLER: Right, right. Well, on that note, Peter, for my last question, I want to ask you one that's sort of more forward looking. And you really teed it up for me with your reference to this 10-page single-spaced dream list that you have of projects to work on. And so I guess my question there is, not just asking you about any one of them, but just sort of generally, what are you excited about in the future, both in terms of your own career and your own research endeavors that you want to undertake, but then more broadly in the many fields that you represent? What breakthroughs do you see sort of on the cusp of becoming a reality, and how might your own efforts play into those breakthroughs?

BASSER: Mm-hmm. Well, that's many questions you just asked. [laugh]

ZIERLER: Yeah. Well, I got to pack them in. This is a great opportunity. I got to get them in there.

BASSER: Well, I would say I'm very excited about looking at small objects, subcellular objects, with techniques like NMR, which haven't really been probed before. I think that there's a whole rich area of cell biology to discover. Don't forget that NMR is great for looking at water, and water is invisible to virtually every other methodology. I mean, you can put fluorescein labels on molecules, and there's a tremendous amount of great biology that's been done with fluorescent markers and GFPs, but water is an incredibly important biomolecule and—you can't see it. And I think we're starting to be able to see water exchanging, we're seeing water trapped in pores that are very small. And I think that's an area that's going to be growing a lot, and I'm very excited about that direction. And I'm also working with some very talented people at Mass General Hospital to try to take this kind of 'microstructure imaging' and bring it into the clinic. That's also been a fun aspect for me. And, I mean, I don't really see myself slowing down. I'm not thinking about retirement. The polymer science stuff …I think the work with composite gels is a very, very interesting area that has a lot of practical applications with tissue engineering. And I think we've just started thinking clearly about that, how to make materials that have these Yin-Yang kind of properties that I was telling you about.

ZIERLER: Mm-hmm.

BASSER: And nature figured out how to do this 300 million years ago. Sharks are called Chondrichthyes because they don't have bones, but they have cartilage. And cartilage has been around a long time, and it's one of the most successful materials known to man. And very, very little is known about extracellular matrix, its lubricating ability, its load-bearing ability. And I think some of these insights that we're going to get from looking at these physical model systems is gonna be very important, developing mathematical models that are companions to that.

And I think whenever you can see something that's invisible and make it visible, it has a huge impact, and that's always a lot of fun.

ZIERLER: That sounds like a superpower to me.

BASSER: Well, it's not a power, but if you can pull out some feature or signal that wasn't apparent before and creative scientists, biologists, clinicians can take that and either make a diagnosis with it or a therapy or discover something new, that's very satisfying. And I see myself continuing to do stuff like that henceforth for as long as I'm able to.

ZIERLER: Well, Peter, it's been awesome talking with you. I'm so glad we were able to connect and I'm really appreciative of our time together, so thank you.

BASSER: Well, thank you very much. This was a great interview and you really asked a lot of challenging and important questions—

ZIERLER: Good. Thank you.

BASSER: —so it's a great credit to you.

ZIERLER: OK.

[End]