CHEMICAL HERITAGE FOUNDATION

MARVIN L. VESTAL

Transcript of an Interview
Conducted by

Michael A. Grayson

at

Orange County Convention Center
Orlando, Florida

on

3 March 2010

(With Subsequent Corrections and Additions)

ACKNOWLEDGMENT

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This project is made possible through the generous support of the

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MARVIN L. VESTAL

1934 Born in Pendleton, Indiana on 8 September.

Education

1958 B.S., Engineering Sciences, Purdue University
1960 M.S., Engineering Sciences, Purdue University
1975 Ph.D., Chemical Physics, University of Utah

Professional Experience

U.S. Army
1954-1956 Signal Corp, Fort Huachuca, Arizona

Johnston Laboratories, Baltimore, Maryland
1958-1967 Scientist

Scientific Research Instruments Corp., Baltimore, Maryland
1967-1970 Senior Scientist and Founder
1970-1976 Consultant

University of Utah, Salt Lake City, Utah
1970-1975 Research Instructor, Chemistry
1975-1976 Research Associate Professor, Chemistry

University of Houston, Houston, Texas
1976-1981 Associate Professor of Chemistry
1981-1987 Professor of Chemistry

Vestec Corporation, Houston, Texas
1983-1993 President

PerSeptive Biosystems, Framingham, Massachusetts
1993-1997 Vice President, Mass Spectrometry

Applied Biosystems/PerSeptive Biosystems, Framingham, Massachusetts
1997-2001 Scientific Fellow and Vice President, Mass Spectrometry

Applied Biosystems, Framingham, Massachusetts
2001-2004 Principal Scientist, TOF Mass Spectrometry Research

	Virgin Instruments, Framingham, Massachusetts
2004-present	Founder, CEO, CSO

Honors

1985	Lester W. Strock Award
1997	Scientific Fellow, PerSeptive Biosystems
2005	Field and Franklin Award for Distinguished Contribution in Mass Spectrometry
2010	Distinguished Contribution in Mass Spectrometry

ABSTRACT

Marvin L. Vestal grew up in Pendleton, Indiana, one of two children. Vestal's father was a farmer and self-taught engineer whose father refused to allow him to attend high school. He encouraged Marvin and his brother to get an education, because they were "too damn lazy to work for [a living]." Marvin obtained both bachelor's and master's degrees in Engineering Sciences from Purdue University, taking a break after two years to volunteer for the draft; he was assigned to join the U.S. Army Signal Corps. He finished his undergraduate degree and master's degree on the GI Bill, coming out of Purdue with no college debt. During college he worked part time at Johnston Laboratories, meeting there Henry Rosenstock and Merrill Wallenstein, who had studied at the University of Utah under Austin Wahrhaftig and Henry Eyring, and who developed the quasi-equilibrium theory (QET) of mass spectrometry (MS). Rosenstock left Johnston Laboratories, so Vestal continued the coincidence time-of-flight (TOF) project on which the two had been working; he also improved the machine with his invention of an electron multiplier.

When Johnston Labs moved to Baltimore, Maryland, Vestal also moved. He began a physics PhD program at Johns Hopkins University but quit after two years to work full time at Johnston. He left that company to found Scientific Research Instrument Corporation (SRIC), with cofounders Gordon Fergusson, William Johnston (of Johnston Labs), and Bob Jones. The company licensed the new process chemical ionization (CI) from its inventors, Burnaby Munson and Frank Field, and Vestal was the first to commercialize it. Ever restless, Vestal decided that the academic world held appeal, so he went to the University of Utah for a PhD in chemical physics, studying under Wahrhaftig and Futrell. He published some papers along the way; he built a triple quadrupole MS for photodissociation. With Calvin Blakely he built a crossbeam MS for his dissertation.

PhD in hand, Vestal accepted a position at the University of Houston, where he stayed for eleven years. During those years he invented and patented thermospray and started another company, Vestec, which did so well he had to leave the University to work at Vestec. The company commercialized MALDI/TOF instruments and sold "a bunch" all over the world. Vestec's merger with PerSeptive, led by Noubar Afeyan, eventually led to the merger with Applied Biosystems. Internal problems caused MALDI to be sold to ABSciex. Vestal retired from ABSciex but soon came out of retirement to found a new company. Virgin Instruments, working to find the theory for optimizing any MALDI, has produced instruments in sizes from desktop to two-story vertical. At AB Vestal and his coworkers were again first, this time to commercialize the revolutionizing delayed-extraction MALDI/TOF and then to develop the first commercial TOF/TOF.

Vestal discusses his views of a number of things: sources of innovation; grants; biases of reviewers; increasing complexity of science; dearth of American graduate students; persistence of professional managers and wasteful meetings ("less talk and more do"); interesting people he has met through science; publishing; friendly competition; his wife's career; patents (he has at least fifty), licensing, and lawsuits; women in science, particularly MS; influences of Rosenstock, Wahrhaftig, and Futrell on his thinking; his influence on others; and the Distinguished Contribution to Mass Spectrometry award given him in 2010 by the American Society for Mass Spectrometry (ASMS).

Vestal concludes his interview with a discussion of his newest company and his ideas for the future. He thinks biology is extremely important and has already driven a huge expansion of

the field; he hopes his instruments will continue to drive research into biological applications. He talks about electrospray and MALDI's superiority, but thinks MALDI is reaching its limits. His company will have a new instrument within a year. His advice to young would-be scientists is to do science for love, not money. Thinking about his own career in science, he says he has always "followed [his] nose."

INTERVIEWER

Michael A. Grayson retired from the Mass Spectrometry Research Resource at Washington University in St Louis in 2006. He received his B.S. degree in physics from St. Louis University in 1963 and his M.S. in physics from the University of Missouri at Rolla in 1965. He is the author of over forty-five papers in the scientific literature dealing with mass spectrometry. Before joining the Research Resource, he was a staff scientist at McDonnell Douglas Research Laboratory. While completing his undergraduate and graduate education, he worked at Monsanto Company in St. Louis, where he learned the art and science of mass spectrometry under O. P. Tanner. Grayson is a member of the American Society for Mass Spectrometry (ASMS), and currently is the Archivist for that Society. He has served many different positions within ASMS. He has served on the Board of Trustees of CHF and is currently a member of CHF's Heritage Council. He continues to pursue his interest in the history of mass spectrometry by recording oral histories, assisting in the collection of papers, researching the early history of the field, and preparing posters recounting historic developments in the field.

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INTERVIEWER: **Michael A. Grayson**

LOCATION: **Orange County Convention Center**
Orlando, Florida

DATE: **3 March 2010**

GRAYSON: I hear you there. Okay, we're going to start by saying my name is Mike Grayson, and I'm with Marvin [L.] Vestal at the Pittsburgh Conference or Pittcon in Orlando, Florida. Today is 3 March 2010. We're going to do a little oral history interview of Marvin's career. With that starting introduction, please ignore the date that's on this audio file, because it has nothing to do with the real date when you see it. We're sitting in a quiet space at Pittsburgh Conference, and I'm going to ask Marvin, when he was born, because I don't know if we have that information anywhere.

VESTAL: You want me to sign this?

GRAYSON: Yeah. So, when were you born Marvin?

VESTAL: 8 September 1934.

GRAYSON: 1934, very good. So, you were born in, what part of the country?

VESTAL: Indiana.

GRAYSON: Indiana. Seems we've got a lot of people from the Midwest that have gone through this exercise.

VESTAL: I was born in the house that I grew up in for the first eighteen years.

GRAYSON: Okay, and what exact part of Indiana was that?

VESTAL: Little town of Pendleton, [Indiana], which is about thirty miles northeast of Indianapolis [Indiana], along the Fall Creek. It's located at the Falls in Fall Creek. East of Indianapolis, by the way.

GRAYSON: Were your parents farmers?

VESTAL: Yeah. My father would have preferred to be a full-time farmer, but in fact, he was a part-time farmer and a full-time employee of Delco Remy in Anderson, [Indiana], for at least [twenty years] during the time I was growing up.

GRAYSON: And your mother was a housewife?

VESTAL: Well, she was originally a schoolteacher. She went to the Indiana State Normal School [now Indiana State University], it was called then. I think it was Indiana State University. I'm not sure which one it was now. Anyway, she [took] a two-year teacher's course, and then spent about eight years teaching…I'm not sure exactly. They got married in 1924, and she had my older brother at 1930, so roughly in the period of 1920 to 1930 she was a teacher. After that she was a housewife and, and co-farmer, because in the society we had women didn't confine their activities to the house, by any means. I mean she could drive the tractor and look after the animals […].

GRAYSON: So, the Normal School education was essentially a Bachelor's […]

VESTAL: No, it was just a teacher certificate.

GRAYSON: Teaching certificate…

VESTAL: Two year, it was a two-year program at that time to be a teacher. She taught in a one-room school, I think most of the time. In fact, she taught the parents of some of my classmates, because she was…she was a little older when she had children than some of the others. So, quite a few of the parents of my classmates were her students. [laughter]

GRAYSON: Yeah, wow. And, so basically, ah, in terms of education your father probably just went through high school or…

VESTAL: No, he didn't go to high school. He intended to. It's one of the family stories that he often told. I think my mother told it more often, but he was…you know, he really was a very bright man, but when he got to the age of going to high school, he had…and at that time, where we lived was, sort of, between Pendleton, which is a small town, and Anderson, which is a large city. And Anderson had much better schools and kids in that area could go to either one. He had enrolled in Anderson High School and arranged for a ride and bought his books and everything. Last minute his father said, "No, you can't go to high school. We need you to stay home and work." So, he never went to high school.

GRAYSON: So, ah, they were kind of limited in the higher education side. I was just curious when your mother, ah, started to have her family did, did she have to quit teaching then?

VESTAL: Yeah.

GRAYSON: That was the norm?

VESTAL: I don't know if she had to or not, but I think she did. She certainly didn't teach after that.

GRAYSON: Yeah, interesting. So, there was…since she was an educator, there was a certain kind of an attitude towards education in your family growing up. Did, did they encourage you to go to school or keep you intellectually challenged at home, when you were growing up?

VESTAL: I don't think they needed to really. I mean, I read a lot and they certainly didn't discourage it. And my grandmother who lived with us, read <**T: 05 min**> to me as a small child. So, I grew up reading nonstop. You know, I worked on the farm too, so you know it wasn't…we were having responsibilities. But, ah, I think my parents' attitude was if I wanted to go to college, they were all for it. If we didn't, that was okay, too.

GRAYSON: Okay. So, it was, kind of, let's say laissez-faire, so to speak…

VESTAL: Yeah. They were a little disappointed my brother didn't go to college. I mean, he was just not inclined that way, at the time, at least. And never had a question in my mind, I was going to go to college.

GRAYSON: Ah, okay. So, he was a few years older than you, right?

VESTAL: Yeah, four years older.

GRAYSON: Four years older. Okay.

VESTAL: Well, my father was one of these people that had all these rather pithy sayings that he used quite frequently. And one of the ones I remember best really is that when we were down pulling corn, hand hoeing which he liked to do because it was very effective, but also hard work. He would come down and find us sitting in the shade instead of out there doing what we were supposed to, and he would shake his head and say, "You boys better get an education. You're too damn lazy to work for [a living]." And I took my father's advice. [laughter]

GRAYSON: If you're going to be this lazy, you better [get educated]. I love it. I love it. So, you went…obviously, you and your brother did go to high school, I assume.

VESTAL: Yeah, we both to Pendleton High School and we both graduated from Pendleton High School.

GRAYSON: And so when did you first kind of get a buzz on science or did you get a buzz in school or, in your younger years?

VESTAL: Well, my buzz was more mathematics, than science, I suppose. The science teaching at that time was not all that great, but the physics teacher was quite good and he was also the principal and superintendent and quite an intellectual for Pendleton. But I was very fortunate that when I was in the first year of high school, they got a new math teacher, that was a dedicated—as most of our teachers were unmarried women, you know…making their life teaching. And she was really good. And one of the things that she had done in previous schools was to take some students to these math contests that the state ran. And so she ran a little competition to see who would be most appropriate to take. In fact, the first pass, I didn't do very well, and for some reason she did it again, and a friend and I were the ones who were eventually picked to go. And by some miracle, I […] won the state algebra contest against all these big schools.

GRAYSON: What year would that have been, do you recall?

VESTAL: Well, I guess that would have been 1949.

GRAYSON: 1949, okay. And do you remember the name of this teacher?

VESTAL: Yeah, Clara Barton Borden.

GRAYSON: Ah, how do you spell that last name?

VESTAL: B-o-r-d-e-n.

GRAYSON: Clara Barton Borden…

VESTAL: Named after Clara Barton you know…

GRAYSON: You said that the principal was a physics teacher. What was his name?

VESTAL: Ernie Harris.

GRAYSON: H-a-r-r-i-s.

VESTAL: Yeah.

GRAYSON: Okay.

VESTAL: The chemistry was Fred Wolf, who was still…who is actually still living is a very good friend.

GRAYSON: Oh, wow.

VESTAL: He was the ag [agriculture] teacher, and of course, he'd taken chemistry as part of his ag courses at Purdue [University]. He was a very, a great teacher, but he didn't like chemistry. But, I guess I learned more chemistry than I thought I did at the time, because he kind of had to stay ahead of the class by keeping his book open, and we had to keep ours closed. He was a very nice fellow, and I still…he's still a very good friend.

GRAYSON: And, ah, his name, the chemistry fellow is…

VESTAL: Fred Wolf.

GRAYSON: Wolf, W-…

VESTAL: W-o-l-f, ah, I don't know if it's spelled with an "E" or not, no, I don't think he does. I think it's W-o-l-f.

GRAYSON: So, once you got this little buzz with math, you, kind of, decided that was good fun stuff, and you wanted to keep…

VESTAL: Yeah. […] I mean, I won. In the freshmen, I didn't win […] this other friend, Presley Peak, […] went on to become a Ph.D. in mathematics and teach math…he did better in the geometry and the things that required proof and theorems and I was problem of the problems. I liked to do problems, and do the algebra. The algebra test was [all] problems. And then we both took the advanced trig [trigonometry] and everything, but neither of us did all, well we did okay. But you know it's a big state and there were a lot…I don't know how many thousands of kids there were at the start of this.

GRAYSON: Yeah. But I mean you did win state for the one time…

VESTAL: Yeah.

GRAYSON: So, that's, that's really impressive.

VESTAL: Ruined my social life…which wasn't very good anyhow. Nothing like being a nerd in a small school.

GRAYSON: Yeah. I guess nerdiness was rewarded then, as it is now.

VESTAL: Yeah. Well, you know, it's a mixed bag of young people.

GRAYSON: So, when did you graduate from high school?

VESTAL: 1952.

GRAYSON: 1952, and then, you were pretty sure you wanted to go to college then.

VESTAL: Yeah. I went from there to Purdue. I was in the first year…I enrolled in engineering. And Purdue <**T: 10 min**> at that time you, you know you enrolled in engineering and you didn't have to decide on which kind of engineering you were going to do for the first year or two. Ah, so you took pretty much the same courses the first year. And I was really thinking I wanted to be a chemical engineer, because I had this vague idea that I was going to somehow make useful things out of the waste products that we had on the farm.

GRAYSON: Well that's…you were ahead of your time on that, weren't you.

VESTAL: Well, yeah. I never did it. [laughter] But when I got there, I realized that what I really was most interested in was applied mathematics, and they had a course, which originally had been called Engineering Mechanics, but then they changed and expanded it and called it Engineering Sciences. So, it was…what do they call it these days? Well, I suppose you'd call it engineering physics…

GRAYSON: Yeah, that's…

VESTAL: [There] was some chemistry we took. We took physical chemistry and some advanced physics courses, and lots of differential equations. It was really a background for doing almost anything theoretical. And so I enjoyed it very much. The only problem was when I was, I got both a bachelor's and a master's degree in that, and while I was doing my master's degree, I started working part-time at this company which had been started in West Lafayette [Indiana].

I should digress a little bit. I didn't go straight through. I went two years to Purdue. I did okay the first semester. I did very well; after awhile, "This isn't so hard," so I didn't work so hard. But, and the last semester I was there, I decided I was really kind of bored with engineering. I had taken some English courses that I really liked, so I decided, I think I want to be a writer. So, I took all English courses one semester, in both writing and mostly.

GRAYSON: These were at Purdue?

VESTAL: At Purdue, yeah, mostly writing courses. And I enjoyed it. I worked harder than I'd ever worked in my life on something, and I'd realized I could do it, but it wasn't…it didn't come that easy you know. It was a hard way to…I really didn't have a great talent for it, but I could express myself, but to do creative writing it takes something more than that, and that wasn't, that wasn't my thing.

And then I had…you know, I was a young person, and often your physical attributes overtake you in the summertime, so anyway, that summer I ended up getting married and volunteered for the draft, because this was right at the end of the Korean War. The GI Bill was still a fact, but it wasn't going to be very long.[1] There wasn't anybody shooting anybody, so I thought well, gee. You know, I really need some…if I'm going to go onto college the way I want to, my parents are certainly willing to help whenever they can. But they had limited resources, and since I got married and started having a family, it wasn't fair to ask them to support me. Well, I think it really makes sense to go in for two years, and get the GI Bill and then, come back and then it'll be easier to go on and it will be easier to go. So, I didn't get paid very much while I was in the Army, and I got more money the four years afterwards going to college. That really supported me through my master's degree at Purdue.

GRAYSON: So, it was kind of a little bit of…it looks like a two years.

VESTAL: Yeah, it was right at two years out in the Army and then I went back and finished the degree, the B.S. Degree. And as I was finishing the B.S. Degree, I'd been working at various part-time jobs. I drove a cleaning truck, one of these route trucks that goes around and picks up the clothes from the little stores, and the cleanings, and takes them back to the central place and, you know, I did that every day for a while. And you know, it was really a good job for a part-time job, and later I worked in a motel for a while as a night clerk. There you could…you worked about an hour and the rest of the time you had to be there, but you could either study or watch television, whichever you wanted to. Hopefully I did some studying while I was there.

Anyway, as I was about to finish my [bachelor's] degree, there was an ad in the placement service of the, at the college. And I was looking for a real full-time summer job that I could make some money and use some…all that [I had], supposedly learn at that point. And I was still planning to go on to a master's degree. But I hadn't…I wasn't sure I was going to do that. So, anyway, this company was advertising for somebody they wanted to hire. And it sounded pretty interesting. So, I went out to the interview, and the company was run by a guy named Bill Johnston, who was an assistant professor at Purdue in chemistry <**T: 15 min**>. He was a student of Bill [Willard F.] Libby's [who] had been involved in a peripheral way of the Manhattan Project working for Libby.

[1] Servicemen's Readjustment Act of 1944, P.L. 78-346, 58 Stat. 284m.

GRAYSON: Is that L-i-b-b-y?

VESTAL: Yeah.

GRAYSON: Libby. And then Johnston was, was it "t-o-n?"

VESTAL: Yeah, right. Willard F. Libby, the Nobel Prize winner [in Chemistry, 1960].

GRAYSON: Oh, yeah, sure. So, this company was doing what?

VESTAL: Because at that time Libby was of course, the one…was on the Atomic Energy Commission. I think he might have even been Chairman of the Atomic Energy Commission at that point. He was a big wheel in the Atomic Energy, and so Johnston got contracts mainly from the Atomic Energy Commission to do various things. And he also had some contracts with private companies, but Libby's main thrust was using atomic energy for useful purposes…he had these plans. There wasn't anything you couldn't do better than using atomic energy. One of projects they had done when I went there, was they were doing secondary oil recovery, and they were tagging the propane, or whatever it was they were injecting into the oil wells, and then with radioactive—I guess, C-14. I don't think they were using tritium. Then, monitoring the activity at the emerging wells to get the full patterns over the oil field and all this stuff. And so, he hired me to try to analyze some of the data to come up with a model for how to interpret some of this stuff. And I, actually, did some of that and that actually turned out…I learned quite a lot about the flow through porous media […].

The funny thing was, when I went out to the interview…well this was before I graduated, you know, and he was one of these people who could be quite intimidating when he wanted to be. And he asked me a whole series of technical questions, all of which I was familiar with, but most of them I said well, I, I would give sort of a half-ass answer, and say, "Well, I know where to find it, but I don't remember the answer." And we went through a few of those, and I guess I must have answered some of them right, but all I remember was that most of the questions he asked me, I really didn't answer very well. Anyway, he hired me anyhow. So, that's how I got into Johnston Laboratories, the company was called. And the summer then, I worked part-time through the rest of the year, and then I worked full-time in the summer. And during that time, he expanded the company by bringing in a couple of fresh young Ph.D.'s from Utah, Henry [M.] Rosenstock and Merrill Wallenstein, who…

GRAYSON: Oh, wow, Rosenstock.

VESTAL: Just finished their degrees a year or so before, with [Austin L.] Wahrhaftig, a good friend in Utah.

GRAYSON: Okay. So, let's go get some name spellings in here, so that, let's see. We've got Rosenstock…

VESTAL: You know how to spell Rosenstock.

GRAYSON: R-o-s-e-n-s-t-o-c-k.

VESTAL: Right.

GRAYSON: Okay, and […] this last one name.

VESTAL: Wallenstein, W-a-l-l-e-n-s-t-e-i-n.

GRAYSON: Okay. So, these were new, newly minted Ph.D.s that he brought in to work with the problem.

VESTAL: And of course, you know Henry was…well, they both had worked on quasi-equilibrium [theory]. They had published original papers on the quasi-equilibrium theory of mass spectra with Austin and Henry Eyring.[2]

GRAYSON: Eyring, that's E-y-r-i-n-g.

VESTAL: Right.

GRAYSON: So, I didn't know we got two Henry's here. You got Henry Rosenstock and…

VESTAL: Henry Eyring.

[2] H.M. Rosenstock, M.B. Wallenstein, A.L. Wahrhaftig, and Henry Eyring, "Absolute Rate Theory for Isolated Systems and the Mass Spectra of Polyatomic Molecules," *Proceedings of the National Academy of Sciences U.S.A.* 38 (1952): 667–78.

GRAYSON: And Henry Eyring. So, you're talking mostly about Henry Rosenstock.

VESTAL: Yeah, Henry Eyring was only involved titularly in that, because he was the big gun in Utah, and everybody wanted him to be a coauthor on their [papers]. Well, it was based on his absolute rate theory which he had published earlier in the summer. It was a spin of that. And of course, equivalent to RRK [Rice, Ramsperger, Kassel theory] in terms what the reactions of the gas phase. So, that's how I got started in mass spectrometry was working with Henry.

GRAYSON: So, well, actually I'd like to back up a little. What did it cost to go to school in those days?

VESTAL: Not much.

GRAYSON: I mean, you were at Purdue, was it a state school?

VESTAL: State school. The first two years I had a state scholarship which covered my tuition plus a little bit, pretty much covered by direct expenses. It didn't cover room and board or anything like that. But it covered my out-of-my-pocket expenses too, university. So, I basically went free. When I got out of the Army, I got a similar scholarship to cover the expenses and then of course, I had the GI Bill which paid me…I don't remember…two hundred dollars a month or something like that. Well it paid also for books and stuff, some expense. So, <**T: 20 min**> the cost of university for an in-state student was essentially nothing. I mean even, the scholarship was for about a hundred dollars a semester. It really wasn't a huge amount of money either way. But when I went to Purdue I qualified on the state scholarship on the exams, but they were also based on need. And while my family was not wealthy by any means, my father owned his farm, and lots of equipment, so his net worth looked pretty good.

GRAYSON: Oh, yeah.

VESTAL: So, I didn't qualify as having a need to get a full scholarship. Otherwise, I could have. I could have gone on a full scholarship and some of the people did. But, you know, it was very inexpensive to go to school. Let's see, I went all the way through, including having a family, and working some, but you know, without any debt whatever, I came out of school with money in the bank after.

GRAYSON: Okay. So, Purdue […] it seems to me that it has the fairly good reputation in the academic community. Was it that way then or is it better now than, it was?

VESTAL: Well, it's hard for me to say. I think it was quite good when I went there. I had the, really very good faculty. I mostly was interacting with the engineering sciences faculty; […] many of them were superb. But I have very little interaction with chemistry. I took one physical chemistry course, and Dick Müeller, who also was a graduate of the University of Utah, Eyring protégé. And, Paula Feuer was a professor in engineering sciences. Her husband, Henry, was a professor in chemistry. She was actually trained as a physical chemist, and so she taught some courses [in the] engineering curriculum that, really were very closely related, chemistry-related and was a very good teacher. She was excellent.

GRAYSON: We mentioned a number of names here, were there any teachers that really stood out or really kind of kept the fire burning for interest in science and in math?

VESTAL: No. I think, during the time I was an undergraduate, I had good professors. I didn't have any strong relationships with any of them. I was keeping pretty busy. I was supporting a family and going to school and, ah, even with the GI Bill, you know, I didn't have a lot of spare time. But, no, I felt that the program was really good. […] The applied math stuff was first-rate and we had some physics courses.

GRAYSON: So, did the military stint […] were you in the States […]?

VESTAL: Yeah, it was an interesting total waste of time. But, it did have some beneficial effect. You know, I grew up in a small town. Catholic was considered a minority, you know. We had two Catholic kids in our class. Of course, there's no Catholic church. There was a Catholic church in Anderson, but there were no Jews, no blacks, and no Orientals. And, I think everybody at least professed to be a Protestant, although many of us probably weren't Protestant. But, it was a pretty homogeneous society. When I went to the Army of course…and Purdue was not that homogeneous, but also, Purdue was at that time mostly white and mostly middle-class white. It wasn't a melting pot by any means. There were students from all over, but not like it is now.

GRAYSON: I mean, they were from all over this country—all from the United States—but did they have students from outside the country?

VESTAL: Not very many, not very many…a few maybe. But there were a few students and a lot of students from Chicago [Illinois], and a few from the east coast. But I went in the Army and you're dropped into this pot, and this is after desegregation. And you're living next to all

kinds of people, people that you never saw before, and it's quite educational. In fact, I found that these people for the most part are pretty damn, much like everybody else. And a lot of the non-commissioned officers were black, because during the Korean War the people who voluntarily enlisted were people who did it because they didn't have a better option, and most of the other people were doing their damnedest to stay out. So, a lot of the people coming back from Korea, who were career men, were black sergeants and corporals and so forth. These guys were damned impressive. <**T: 25 min**> Some of those guys I thought were some of the best people I knew, and they were smart. They weren't real educated, but they were smart.

GRAYSON: You went, I guess, to boot camp, and then…

VESTAL: I went to my basic training in Fort Leonard Wood, Missouri.

GRAYSON: Oh, my. Drive by it all the time.

VESTAL: Spent the Christmas in a pup tent out in the boondocks for a week, and it was cold that winter, but I survived. And then, you know, they have a lot of these exams you take when you're in basic training to figure out what to do with you next, you know. Of course, then by the time we finished basic training, the war really was over. I mean, it was over before that, but they had it, sort of, stopped in place. You don't have the troops going in. They were starting to send people home, and they weren't sending anybody to Korea, at least not any significant numbers, and there wasn't any shooting. So, you took all these exams and they figure out what to do with you, you know. And most of them at this point, they didn't really need us, you know. It was the inertia of bringing people in and then saying, okay. See a lot of other people [who] were drafted at that time […] had gotten earlier deferments, because you get a deferment to finish college, and a lot of people did that. But, then when they finished they were liable for being immediately drafted, so a lot of the guys who were in when I was in were college graduates. And some of them had families and so forth. So, it was, kind of, a different thing.

But, you took all these exams and then they figured out what to do with you. By some combination of things, I was assigned…I had a very unusual set of orders. There were a few others in this category and how we got picked or why, I have no clue. It was probably mostly at random, but there was this Army Security [Agency] School at Fort Devens outside of Boston [Massachusetts], near where I live now.

GRAYSON: Devens…

VESTAL: Yeah, D-e-v-e-n-s. It's closed now. It's been turned into an industrial park. But it was a big infantry [post], but also had the Army Security Agency Schools. And most of the people went there were people who had signed up for four years. But they did send a bunch of

us there. I think mostly, because they didn't know what else to do with us. But, anyway, I had unusual orders because I was assigned not to the Army Security Agency, but the Signal Corps. But I was also directed to go to this school and learn Morse code. So, I went through basic training to Fort Devens, and I was married at that time. So, I drove there with my wife. I managed to get that done, and you know we found a ridiculously small place to live outside of the post.

But at that time, it wasn't all that expensive. It couldn't be very expensive. It was government funded, but we had a little room in the back of this house run by an Italian family. And they were very nice people. But our room was the bedroom and a little kitchen, was about the size of a large closet. The bathroom was out in the hallway and share to some extent with them. And I don't remember what we paid, but it wasn't much. But it wasn't bad. We were young. We didn't have a lot of stuff, so it didn't matter.

And so I went to that school, and learned to type Morse code at thirty words a minute on a typewriter because I could type when I went there, which helped because a lot of a people didn't know how to type. After you got past ten words, you had to do it on a typewriter, instead of by hand. That slowed some people down. I actually finished first in my class in that course, but I was already assigned to go to Fort Huachuca, Arizona, the Army Electronic Proving Ground. When I finished that school…

GRAYSON: How do you spell that?

VESTAL: H-u-a-c-h-u-c-a, I think. It's down south of Tucson [Arizona], near Bisbee [Arizona] and Douglas [Arizona] and that area right across in Nogales [Mexico].

GRAYSON: So what was this fort? What were you doing at this place there?

VESTAL: Well, it was the Army Electronic Proving Ground is what it was called, and we were doing electronic warfare, all the way, highly classified, et cetera, and all baloney. […] I don't know, in my class at the school, I think only two of us that had those orders and there were a few from other classes. So, there were a few of us that were assigned to go there in the beginning. But because of the time we graduated, we had all these…this, this school was running sixty students a shift <**T: 30 min**>, three shifts a day. They were turning out hundred-eighty students every week. And because some Congressman could see, boy, this is really nice. We needed to promote the economy or whatever. Anyway, they kept going for quite a long time back then, and they had no need for the people. So, what they started doing was they started shipping everybody that graduated to Fort Huachuca because after all, they must know something about this. And so, all of my classmates, when they graduated they put them on a plane and sent them to Fort Huachuca and if they had families and things with them, the families had to figure out how to get there on their own.

GRAYSON: Oh, wow.

VESTAL: But I had these orders that said, "You will report to Fort Huachuca by certain-certain date," and that was like, two weeks, because that was sort of the normal travel schedule. And I had my car and family there, so I drove. Actually dropped my wife off at Indiana, because she was pregnant, and she had the child at the Fort Benjamin Harrison Hospital there at that time. But, no, she had gone back earlier, I'm sorry. But anyway, I drove from Fort Huachuca to Arizona and it took me two weeks […]. It took me two days to get to Indiana, and then two days to get from Indiana to Arizona, so I spent the rest of the time in Indiana. And, I arrived at Fort Huachuca through the front gate and showed them my orders, and they scratched their head, and said, "Where did you come from?" [laughter] And I said, "Well, here's my order. I'm supposed to be here, and here I am."

GRAYSON: Yeah. Reporting in on date as specified.

VESTAL: I didn't have any orders to the contrary. It worked, and so I spent the rest of my Army time at Fort Huachuca, killing time. And that's really all that. They had these three branches of electronic warfare, I don't know if the stuff is still classified or not. I don't think it is, but it was then. We had to have a secret clearance. Our Department was EW-4, which was blowing up VT fuses in the air. You know, at that time, the shells had these VT fuses that were set to go off a certain distance above the ground. They reflected the radio wave back and had these jammers which were supposed to send out a beam at the right frequency and set them off when they're way up in the air. None of them ever worked, but it was a good idea. [laughter] I don't know, maybe eventually they got it to work. It just really didn't do anything. And the equipment we had was the old obsolete stuff, that never, never had a chance of working and we weren't allowed to use any of the new stuff. That was the specialist who came from the contractor and whatever. So, we would go through these charades of going down and shuffling equipment around or something. And mostly we pulled guard duty every now and then, and pulled KP and we did our best to stay out of trouble. And we had this black sergeant who was really, really a great guy, Sergeant Hurt…

GRAYSON: Hurt?

VESTAL: H-u-r-t, from somewhere in the South, I don't know, Alabama or Mississippi. He was just big rotund guy with a big voice and he was the first sergeant for this company that was in there. And every morning they would sound Reveille and everybody had to fall out and get into formation. Then he would get us all lined up in formation and he might make one or two announcements or details, he would pull out some people to do this or that, whatever he had on his list, you know. But mostly he didn't have anything for people to do: "All right, you men get back in there and press those blankets."

GRAYSON: Press those blankets. I love it.

VESTAL: So we found lots of ways to entertain ourselves. We'd go out in the field and set up. One of the things we did, because we wanted to make it look good, is we'd take a truck. And we'd do some…have, I think Army truck with a bunch of equipment and you'd go out and set up one of these fancy looking antennas, of course, we didn't turn anything on. We turned it on and jammed all the TV sets. And it looked like we were doing something, and so, you know, we'd play cards or take a hike or there were buffalo out there in some places, and it was, kind of, an interesting desert. You know, we played cards or read, or whatever we had to do. And we'd spend most of the day and then at the end of the day, we would take the antenna down and come back in. I did that for a couple of months, every day. Then I got to become Company Clerk after a while, so I sat in the office and typed for the company.

GRAYSON: So, this was in 1950?

VESTAL: It was 1950…let's say 1954. It was when I finished my first year. It was 1956, when I got out. I was in the Army 1954 to 1956. Got out a little bit early to go back to school in September, at Purdue <**T: 35 min**>.

GRAYSON: So, did Purdue have any grief with this plan, where you decided just to take off after your sophomore year?

VESTAL: No, it wasn't any difference.

GRAYSON: And then you just showed up and […] all your credits counted and you were good to go?

VESTAL: Oh, no. The Army at that time was, you know, that was…

GRAYSON: That was standard, happened all the time.

VESTAL: Some of them didn't come back, but that was another story, for one reason or the other.

GRAYSON: So, then when you came back you […] got your bachelor's degree.

VESTAL: Yeah. Just as I was saying, I got my bachelor's degree in Engineering Science, was when I made this contact with…

GRAYSON: Johnston.

VESTAL: …Johnston. That, kind of, changed my direction, because up to that time, I wasn't sure what I wanted to do. And, you know, when I finished the master's degree […], by then I'd already decided I was going to work with them. But most of my classmates ended up somewhere immersed in the defense industry, because it was the kind of background we had. You could get a regular job working for any number of companies that were doing defense work. And, these guys disappeared. I never heard from any of them since, some of them are out there somewhere. But there were a bunch more people, but the work they did of course, never got any publicity because it was all classified.

GRAYSON: Now, what was the class size in those days? Were they…

VESTAL: Well, the engineering science group was not that big. I think we had maybe fifteen graduates in the bachelor's program […]. And a lot of people changed to something else partway through, because it wasn't easy. […] It became part of the aeronautical engineering, eventually. It surely had some interaction, but officially, it became part of the aeronautical later on. But they sent a lot of the Army, or the Air Force, sent a lot of officers there to go to school for a couple years to learn engineering and stuff. A lot of these guys had been career officers, but they sent them back to school on, on government dollar, and some others got Ph.D.s…

GRAYSON: So, what about…let's just back up a second. You took this year off, kind of, to study English. Was that like your sophomore year?

VESTAL: One semester really.

GRAYSON: One semester, okay.

VESTAL: Last semester that I was there before I went in the Army. And I did a lot of writing. I actually won an essay contest for ten dollars. And the essay I wrote was about the massacre of Indians at the massacre at Fall Creek which was […] called "The Massacre at Fall Creek," I think. Anyway, ah, Jessamyn West later wrote a novel about it, which I read and it's really,

historically quite accurate, because I did the research on the history.[3] But what happened there in—I'm not quite sure about the date, but I think it was 1816—that was a time when, you know, Indiana was filling up with Southerners. Pendleton was already established as the major town in that area, long before Indianapolis. And it was kind of the seat of government for that area. There were a lot of Indians living in the area, peacefully. There was a camp of Indians a short distance from Pendleton. And these, these men were led by some adventurer who was one of these people you read about in the Wild West, I guess. He rounded up some of the other settlers and convinced them that these Indians were taking over their land and that they needed to get rid of them and so forth. So, these guys raided the camp. There were like two braves and, you know, some women and children, and they killed all of them. It was really, really brutal. And of course, most of the settlers were really appalled by this. The guy who led it was smart enough and […] they did it just to rob them basically to get whatever they could. Now he took whatever he got and headed out and was never seen again. But, those people lived there. Before long, in a community like [that], everybody knew who had done it. And so, they arrested them and brought them to Pendleton and put them in jail. And part of the reason…of course the Indian agent pushed this, but the reason they did it really is because there were lots of Indians in that area. And they made it very clear that if, if the white man didn't do justice, they would do it their way, and their way would be to just come in and kill indiscriminately anybody that they found. You know, it wasn't easy for them to decide who <**T: 40 min**> were the ones who really knew who had done it, because I think very quickly, they had the expertise to do that.

So, anyway, it was the only time in American history, I believe, white men were tried and convicted and hanged for killing Indians. And I don't believe it happened anyplace else in the United States that I know of. And there's a little plaque up by the swimming pool, where the monument was set up. Three white men were hung for [the murder of Native Americans].

GRAYSON: There were three?

VESTAL: I think there were three that were hanged. There was one younger man who was sentenced to hang, but he went completely crazy before they hanged him, and they finally didn't hang him, but I don't think he ever recovered from that. According to Jessamyn West it was probably true, when they were doing the hanging, they, they looked around at the, there's some hills around the area that it was lined with Indians who were watching to see if they really would do it.

GRAYSON: Made sure, yeah.

VESTAL: There were never any more problems in that area, but, ah, it's historical. I'm surprised it's not publicized more in Indiana. When we had the meeting [of the American Society for Mass Spectrometry] there in Indianapolis a couple years ago, I went to the

[3] Jessamyn West, *The Massacre at Fall Creek* (New York: Harcourt Brace, 1975).

Indianapolis Historical Society and they had quite a lot about the Indians, about Chief Anderson, who was the Chief in that area and so forth. They have nothing about this episode. I'm a little surprised because it is quite well known.

GRAYSON: And the ringleader left town and he got away?

VESTAL: Yeah. And the local people…you know, they […] certainly weren't innocent, they knew what they were doing. […] It was the same kind of people that do lynchings and so forth in more recent times. Anyway, I thought that it really did speak well for the community, I think, actually did it for whatever reasons.

GRAYSON: Justice was done.

VESTAL: Anyway, I wrote an essay about this after doing some research, because there's quite a lot in the literature. I won, I think, second prize in the historical contest and for ten dollars. I think that's the only money I ever made out of writing.

GRAYSON: But probably stood you in good stead in the end, because you use a lot of writing in science.

VESTAL: Yeah, yeah.

GRAYSON: It's not creative writing. Well, maybe it is creative? [laughter]

VESTAL: More creative than…it, it's supposed to be factual. No, I realized, you know, I could have earned a living as a journalist. There's no question about that. That wasn't really what I wanted to do, and I decided, I'm not a poet, and I don't have that whatever it is that…

GRAYSON: Not [Ernest] Hemingway?

VESTAL: …really sets people apart. And I needed to make a living, so I thought, "Well, I'll go back to science and engineering…"

GRAYSON: Well, a semester spent finding it out probably isn't a bad way to go.

VESTAL: Well, I think some people tried to do things because they're hard, you know. That's not necessarily a bad thing, but after a while, you think well, gee, why should I do something that's really hard, when I can do something that I find really pretty easy? And also, something I enjoy doing…

GRAYSON: Well, that's the important...

VESTAL: I've always enjoyed doing science […]. I never regretted the decision. I'll just put it that way.

GRAYSON: So, you're working for this Johnston company. Did that have an influence on your career and where you went from there?

VESTAL: Yeah, everything. I mean, my whole, my whole life changed with all that. I spent, I guess probably about two years working with Henry…

GRAYSON: This is Eyring…no?

VESTAL: No.

GRAYSON: Henry Rosenstock.

VESTAL: Rosenstock. When I went there, they just had arrived just about the same time I did, virtually. They had a couple of grants, but Henry of course, had done the…Henry and Wallenstein, both were involved with the quasi-equilibrium theory. Their theses were on that with Austin. But Henry had this idea of the coincidence time-of-flight mass spectrometer. And he'd written a patent application on it, which was actually awarded, you know for…and of course, everything was electron impact in those days.[4] So, you ionized things by electron impact and you accelerated the secondary electrons to one detector and the ions to another detector and by [timing] the flight between them, you could determine the mass.

And so, […] one of his projects was to build such an instrument. And I fairly quickly became his assistant in doing that. Now, Henry was a great guy. I really enjoyed working with Henry, and we stayed friends until his death in, what was it…1982, I guess.

[4] H.M. Rosenstock, "Method and Apparatus for Ionization Investigation," U.S. Patent 2,999,157, assigned to William H. Johnston Laboratories, Inc., 1961.

GRAYSON: He was fairly young when he died, wasn't he?

VESTAL: Yeah. He had a heart attack unexpectedly. He was sixty years old. He and Joe [L.] Franklin died within <**T: 45 min**> hours of each other, I think, both in Europe.

GRAYSON: Wow.

VESTAL: I know it was before the international meeting that year. Joe never made it out of Houston [Texas]. That's right. He died in Houston. Henry was in Spain or somewhere.

GRAYSON: When I talked to Keith [R.] Jennings, he mentioned that Rosenstock had been in Europe, I think on one of those things that Jennings arranged.[5]

VESTAL: Yeah. Now, he was traveling around before the international meeting. I'm not sure exactly, but anyway…I worked with Henry on this coincidence time-of-flight machine, and Henry, as I say, was a great theoretician and a great guy to work with. But he'd grown up…well, his family had migrated from Germany, you know, during the early days of the [Adolf] Hitler takeover in the 1930s and settled in New York. And like, I think, many Jewish children of that time, the emphasis is on culture things, studying music, et cetera. And Henry was very much immersed in all those things. But he'd never done anything with his hands. So, he was trying to build this instrument, and he, in the laboratory, was a total klutz. He doesn't know how to make anything. So, he and I would talk and he'd say, "I'm going to do this." And so, that was my job to figure out, how to get it done. You know, get the machinist to make something. But we had a good working relationship.

But, I guess after a year or so, I don't remember exactly the time of this, but it may have been more than a year, Wallenstein left and went to the [National] Bureau of Standards, and a few months later Henry followed. And I think it was good for both of them, particularly for Henry, because I think he really flourished at the Bureau of Standards. Anyway, I was left with this project which we'd gotten a ways along. But we'd not really seen any effect yet. So, I inherited this project, and then Johnston basically, didn't get tenure at Purdue because [Earl T.] McBee, who was the chairman at that time, said, "Okay. You've got to make a choice. You're either a professor or you're an entrepreneur. You can't be both, which is it going to be?"

GRAYSON: This is McB-e-e?

[5] Keith R. Jennings, interview by Michael A. Grayson at Leamington Spa, Warwickshire, United Kingdom, 24 and 25 April 2008 (Philadelphia: Chemical Heritage Foundation, Oral History Transcript #0419).

VESTAL: Yeah, Earl McBee. Fred [W. McLafferty] probably mentioned him as well, I suspect, because he was the chairman when Fred was there. Anyway, Johnston left the University and the company was doing okay, but he really needed funding to do more. And so, he brought in this partner, Manuel Dupkin, D-u-p-k-i-n—Manuel Dupkin II, by the way. He was from Baltimore, [Maryland]. His father had made a lot of money in the clothing business in Baltimore, I'm not sure exactly of that. Dupkin was not interested in the family business. He had been an assistant to Admiral [Hyman G.] Rickover. He had been in the [U.S.] Navy. He was [U.S.] Naval Academy graduate and served time in the Navy and he had worked as assistant to Admiral Rickover in the nuclear submarine thing. Somehow through that connection and Bill Libby, he and Bill Johnston got together and he put some money into the company on the proviso that it be moved to Baltimore. So, we packed up and this was just as I was finishing my master's degree in January of 1960. So, they moved to Baltimore, and shortly thereafter, I followed them and continued this work. And at that time, we still didn't have the coincidence machine working, but we were close.

And so, I went ahead and built the coincidence time-of-flight and made it work, and got some data. We made some for NASA [National Aeronautics and Space Administration], and various things, and it never became a practical device, but it was a lot of fun. And I learned a little a bit about time-of-flight in the process. At that time, time-of-flight was much harder of course, because the electronics were a little, pretty, pretty primitive. So, our first spectrum was done with an oscilloscope and a Polaroid camera, and you looked at the display on the oscilloscope and took a picture of it, and you got more intensity, that's where the peak was. I wish I still had one [Polaroid spectrum], because I used to have some photographs of the first spectra. There really were peaks in there. They were pretty bad. But we eventually got to the point we could separate the isotopes of xenon. That time it was pretty good.

GRAYSON: Yeah. Just [then] Bendix came out with their time-of-flight machines almost contemporaneously, I think.

VESTAL: Yeah. They were actually out a little bit earlier. I think it was [William C.] Wiley and [I. H.] McLaren's work was in the early 1950s and that Bendix machine came from that. So they had a commercial machine using the Wiley patent. The coincidence time-of-flight had the advantage that if you have a really low ionization rate, you could do things that otherwise couldn't be done <**T: 50 min**>. So, for electron impact, it didn't make a lot of sense, because you're having to sort electrons from electrons. You learned how to do that […] but you had to use real high energy electrons, and then you had a better chance of not seeing scattered electrons, secondary electrons. But it did work. But where it really worked well was for photo ionization, particularly x-ray ionization. […] So, we did some work with x-ray ionization, which some of it is published and is around, I think.

Manfred [O.] Krause, K-r-a-u-s-e, came and joined us from Germany and worked on that project with me. He went later […] to Oak Ridge [National Laboratory]. But […] because

of the coincidence principle, if you've only got one ion every hour or so, you could still get a spectrum, you know, if you waited long enough. So, of course, the things [with] heavy atoms, it's probably easier. But we actually did the x-ray ionization spectra of methane […] I think we had about a hundred counts in twenty-four hours or something like that. But again, Manfred continued that work measuring [secondary electrons]…especially doing photoelectron spectroscopy on x-ray. […]

GRAYSON: So, you decided to go to Johns Hopkins [University] when you moved up to Baltimore.

VESTAL: Yeah. I applied to […] the physics department at Johns Hopkins, the graduate school and I was accepted. I don't know. I remember did I start the first semester there; I may not have started till after the first during the fall. As far as Johns Hopkins is concerned, I was a full-time student, and I wasn't supposed to work more than ten hours a week. In fact, I wasn't probably full-time. I mean, I could work as many hours at the company as I wanted to, basically, and they paid me for whatever I worked. I tended to work about thirty hours a week. But I was young and drank a lot of coffee in those days.

GRAYSON: So, both Rosenstock and the other fellow had gone off to National Bureau and you were really the prime mover for the coincidence time-of-flight instrument.

VESTAL: Yeah. Because the other thing that happened during that time… Bill Johnston who was very good at going out and finding resources, he had made contact with Austin Wahrhaftig, who was the professor. And he became the consultant to the company.

GRAYSON: Where was Wahrhaftig at that time?

VESTAL: Still at University of Utah.

GRAYSON: Still Utah, okay.

VESTAL: Yeah. He spent his whole career there.

GRAYSON: Okay.

VESTAL: But, even before we brought him in, after spending some time with Henry, [the] first thing he did, he gave me a copy of this thesis on quasi-equilibrium theory. And I read it with great interest and thought about it, and wasn't until after he left, that I really came up with the idea, because when a theory like this comes out and everybody does experiments to further test the theory. And while it qualitatively answered a lot of things about the way ions fragmented, the quantitative directions in terms of appearance potentials and the way the currents went was totally off, you know. And you could fudge around with it and make it closer, but there was clearly a problem with it. And of course, the people who did the experiments could say well, this assumption of approaching equilibrium, just can't be right […].

And, ah, so I got to looking at it, and I realized that they had used a classical approximation for doing the statistics. And so, this is equivalent to RRKM theory, which under certain circumstances is okay, but has the same problem as the classical [theory]. And [Rudolph A.] Marcus came along and did the quantum correction to that.

GRAYSON: M-a-r-k-u-s or c-u-s…

VESTAL: Rudolph Marcus in Illinois. Anyway, before he had done that actually, when I was working on quasi-equilibrium theory which was…at least two were considered separate theories, when in fact: one was a micro-canonical ensemble [for isolated] systems which would be the mass spectrometer [version] and the other one was a canonical ensemble where you were averaging over a thermal distribution. But the basis of the two theories is exactly the same. And of course, the QET [quasi-equilibrium theory] grew out of Eyring's absolute rate theory which also was another [statistical] version of the same thing.

But anyway, after I started studying it a little bit, I said, I think I understand what the problem is. They're not counting <**T: 55 min**> the states where you include the zero-point energy, because we did the classical approximation. You know there's zero probability that you have zero energy. But in a quantum case of course, you have the ground stage which is one, and the difference…the ratio of zero to one is infinite. So, when you add up the states near the threshold, one case you get zero and the other case you get a finite number, and that makes a huge difference. The theory was wrong, in some places by ten orders of magnitude in absolute numbers, even though you know, when you did the average and all it wasn't that obvious. But anyway, I came up with a way of counting the states more accurately, which was just simple statistics and actually, Marcus did something similar about the same time or maybe even earlier. I'm not sure exactly when he…but he formulated it in terms of the…

GRAYSON: Who was this fellow?

VESTAL: Marcus…

GRAYSON: Oh, Marcus.

VESTAL: Marcus. Famous…go back and look at physical chemistry. You can't miss RRK and RRKM…

GRAYSON: Yes.

VESTAL: QET is not as well known, but it measured the same thing, applied to an isolated system. Anyway, I spent quite a few years doing those kind of calculations.

GRAYSON: Now, when you say calculations, this is by hand, right.

VESTAL: Well, I was using a computer.

GRAYSON: You were using computers.

VESTAL: Yeah. In fact, this came out of interaction in with Jean Futrell. When I was first with…how this stuff comes back, I may go on all weekend. [laughter]

GRAYSON: Well, that's why I wanted to get started on Saturday.

VESTAL: During the first fall that I worked at Johnston Labs, while I was still in West Lafayette [Indiana], I hadn't really made connection with Jean Futrell. He was at the Aeronautical Research Lab in Dayton [Ohio]. And he was interested in supporting some work on calculating fragmentation because he was doing radiation chemistry, and he had the idea that fragmentation in mass spectrometry must be related to the fragmentation that […] was leading to the processes [he was seeing in] radiation [chemistry except the] time scale [was different]. So, if you could calculate [it as a] function of time, then you could predict what would happen under the circumstance […].

So, he was interested in funding a program to do these calculations. So, he invited Henry to come over and visit, and it snowed the day Henry was supposed to go. And while Henry [drove], he had never driven much in snow. So, I was recruited to be Henry's driver, the day [we went] over to Dayton. So, since I drove I sat in the meetings…you know, I was a nobody. I'm just getting started. I didn't know what they were talking about.

But anyway, I met Jean for the first time at that point. That was…let's see, when was that? 1950?

GRAYSON: Probably been…

VESTAL: Oh, okay, 1960…no. It wouldn't be 1960, would it…1958 or 1959. Yeah, it was that winter of 1958, 1959, I don't remember exactly what, what month it was, but anyway. Jean did actually fund a project to do that. But by the time it got funded, Henry left. So, I inherited the project. So, that's how I started working with Jean. And we did all the calculations on the computer at Wright-Patterson [Air Force Base]…

GRAYSON: Oh, okay.

VESTAL: Using IBM cards. And so, I would write a program and get the cards…

GRAYSON: This is all FORTRAN?

VESTAL: Yeah, it was all FORTRAN…and get this big deck of cards and submit it to the computer, and the next thing you'd get back what you'd done wrong. And then you'd correct that, and put it back in, and then eventually, you'd get something [useful. And] when you got something, you got a pile of paper like this tall. So, all those calculations we did that way. And we beat propane to death. But I realized after a while, [that this] theory really does work in a qualitative sense. It does really explain most of what you see in terms of, the metastables, which depended on the rate—the absolute rate—of reaction and you could use this to extrapolate back to the timescale for radiation chemistry. And it, sort of, worked for that.

The problem was that […] you needed to characterize the whole potential surface of a molecule of the size of…what we considered a large molecule which was propane, or methane for that matter. But, it wasn't diatomic molecules; it had to be something of significant size for the statistical approximation to be anything reasonable. And the problem is you could come up with numbers, but you <**T: 60 min**> had to have […] oscillator frequencies for all the bonds, and also know the activation energies and so forth. There was a lot of experimental data, and what I [was able to] show is you could come up with parameters which matched a wide variety of different experiments quite accurately. But, the trouble was you couldn't *a priori* come up with parameters to be able to predict something in the absence of [values]. So, [in the end], you had more parameters than you had results. So, you know you'd say okay. It does explain it, but it's kind of a dead end. And even today, fragmentation still works the same way. Well, quantum mechanics has gotten a lot better, so you can predict a bit more out of these pretty large scale calculations. But it isn't easy.

So, after a while, I realized that we'd probably done about all we could do with this. We'd shown that the basic idea was okay. It was predominantly statistical, but Austin was always […] uncomfortable; he really felt that all this statistical stuff was an approximation. He didn't expect it to be that good an approximation. He was always a little hesitant to say that it was good enough. But, it really was for most molecules, where there wasn't some obvious area where the energy didn't equilibrate between different modes. It really worked quite well. Even Fred McLafferty came in after a while. He initially was quite, quite skeptical. But Chava Lifschitz from Israel spent some time in his lab, and she had gotten convinced this was a very useful theory. She was a very, was and is, I guess, a very persuasive and very good at presenting things, so she convinced Fred that this really was doing the same thing he'd been doing all the time, but in a more…

GRAYSON: How do you spell her last name?

VESTAL: Ah, L-i-f-s-c-h-i-t-z, I don't…C-h-a-v-a is the first name. I think I got that last name right.

GRAYSON: Is she still alive? Do you know? [Lifschitz died 1 March 2005]

VESTAL: Far as I know. She lives in Israel, but I haven't seen her in some years, so I really don't know.

GRAYSON: I thought I had heard perhaps that she'd passed.

VESTAL: It's possible. Like I was saying, I don't know.

GRAYSON: So, she brought Fred around.

VESTAL: Yeah. So, Fred and I had some interesting conversations about this over the years. Anyway, I don't worry about that stuff anymore.

GRAYSON: So, you were only at Johns Hopkins for a couple years?

VESTAL: I guess.

GRAYSON: It says here in your CV, 1960 to 1962.

VESTAL: Yeah. That's probably right. I did the coursework; pretty much finished all the coursework. And I got involved more [with ion] molecule reactions than chemistry, so I was interested in doing fundamental things on molecules and ions. And the problem was at that time at Johns Hopkins there was a chemistry department and there was a physics department, and there was a big wall in between. And you were either one or the other. And actually, I got to know Walt [Walter S.] Koski, who was chairman of the chemistry department, he interacted with some of the students, but knowing later what I knew, I would have gone to the chemistry department and I probably would have stayed there.

But after two years it was clear to me, if I wanted to do what I wanted to do I would have to drop out of physics and move to chemistry and start all over. And none of the coursework I had taken in physics was any use to me whatsoever in chemistry. So it had been, basically, just a wasted two years. And by that time I was at the point where I was doing interesting stuff at the company, things were going well, and I was building instruments and getting results. And I was having a lot of fun, and I was getting paid for it, and I didn't have to spend all my time going to school.

So I dropped out and spent basically until 1970 working in companies in Baltimore, initially Johnston Laboratories' [until] 1967, I guess. And then, Dupkin and Johnston had a bit of a falling out in a sense. And Johnston and Gordon Fergusson and I and Bob Jones, who was the accountant, left Johnston Labs and started the Scientific Research Instruments Corporation. Then there was a lawsuit, and he basically settled the lawsuit by giving Dupkin our shares in Johnston Labs, because we didn't think it was worth anything, anyhow. But, we had planned to go ahead and continue doing the research we had been doing, and he did a pretty good job of ruining that, because he was pretty well, politically <**T: 65 min**> tied in. But we found other things to do, like building chemical ionization sources.

GRAYSON: So, basically, Dupkin took the Johnston Lab part and when he took that and more or less kicked you guys out, what did he have?

VESTAL: Well, he had the grants and stuff that we had, and he managed to get Walter Koski to come in and sort of help him out. So, Walter…

GRAYSON: Koski, how do you…

VESTAL: K-o-s-k-i. He was chairman of the department for a while, of chemistry at Hopkins. I don't know if he's still living or not. I don't think he is, but anyway. So, they continued some of the work we were doing. But there was one rather simple device which, in fact, made his day because Gordon Fergusson, my colleague that I worked with there and later in mass spec, had

developed…Gordon was an expert in radioactive counters and electronics. But he had built his own carbon-14 lab in New Zealand, and his lab was so good that Bill Libby twisted his arm to come and build a similar lab for him at UCLA [University of California, Los Angeles]. So, he had built up a carbon-14 lab for Libby, and then, Bill convinced him to come work with us in Baltimore.

So, he was building radioactive counters of various special kinds. And one of the things, he built was something that he called BacTec, which was basically monitoring…I think it must have been carbon-14 that is given off when bacteria grown on radioactive media. And they used this for…I'm not even [sure]. I must profess, I'm a little lame on exactly what it's good for, but it did have some real biological applications that people were really very interested in, so that instrument, actually they did a lot of those. They also continued to make the electron multiplier that I developed when I was there. They sold those for a number of years, too. I don't know what they were used. But, then he sold the company to BioRad [Laboratories], I believe. No. I'm sorry it was Becton Dickinson. They bought it really for quite a bit of money, mainly for this BacTec thing. They continued to sell for a long time, and they still could be sold, I don't know.

GRAYSON: So, the Dupkin guy just…he finally got some money out of it in the long run.

VESTAL: Yeah. He made some money. There's no question about that. And in fact, I believe he's still living in the Baltimore area, as far as I know.

GRAYSON: So, you left with Johnston and…

VESTAL: Yeah. So, we started this other company, you know. By that time we were really, strongly, more into mass spectrometry, because a lot of the radioactive stuff ended up with the other company. And it was, kind of, running down anyhow. One of the things that we did is…Bill, had been a consultant for Exxon/ESSO for a long time…

GRAYSON: Bill…?

VESTAL: Bill Johnston. So, he was smart enough to realize when Frank [H.] Field and Burnaby [Munson] patented the chemical ionization, that there was a commercial opportunity there, so he got the exclusive license to chemical ionization from Exxon or ESSO. I believe they were ESSO. Or Humble [Oil and Refining Company], really. Anyway, we had the exclusive license to the instrument, to chemical ionization.

GRAYSON: Chemical ionization.

VESTAL: And, so we started thinking about building sources, et cetera, and Frank came down and spent time with us, Burnaby, came over occasionally. And, I had started thinking about, you know, designing a source, which was an area that I was doing at that point. We built a medical mass spectrometer and some other things— little magnetic machines, but what you need to build sources for these different instruments. Sort of by chance, I was invited down to the discussion group meeting in Washington by [Henry] Hank Fales and [G. W. A.] Bill Milne…The reason I got invited, this was shortly after we formed the company, is they wanted to have somebody come and talk about quasi-equilibrium theory. They had this thing [where] they were studying different things […]. They'd have people read up on it, and then they'd have a real discussion on it. It was a great group.

But anyway, they called me. Bill Milne called me out of the blue. I didn't know him from anyone. Well, they talked to Henry Rosenstock and he was going off somewhere traveling, and he couldn't do it, but he recommended that I probably could do it. And <**T: 70 min**>. So, they called me, and I said, "Oh, okay. I can do that. I've been doing some stuff." And so I went down and had a great meeting, and they served beer during the meetings, so I had a couple of beers while I was talking. Bill and some of them had really read up on the theory. They had a lot of really good questions. We had about a two-hour discussion about it, and as many beers as people wanted, I don't remember, I don't think I had that many…at least a couple. And after the meeting, I was talking to Hank, who I had never met before either. And he said, "You know, I've been hearing about this chemical ionization stuff that Frank is doing, and I wonder if we could use it for biological molecules?" Some of these polar molecules seem like they ought to work pretty well. And Frank had been doing hydrocarbons.

GRAYSON: Yeah, yeah.

VESTAL: That was what paid his salary. And he had done a lot of work with hydrocarbons, but they hadn't done much with anything else. And I said, "Strange you should bring it up because we just took the license for technology." He said, "Well, you know, how much would it cost to get a source for my MS-9?" I said, "Well, I don't know, maybe seventeen thousand [dollars]." [laughter] I had no idea, really. And he said, "Okay, let's do it."

GRAYSON: Wow.

VESTAL: We built the source and spent a lot of long nights down there to make it work.

GRAYSON: But this is just on the concept that it would work for polar molecules. No one had ever demonstrated it?

VESTAL: No, not really. But it had to work, pretty much.

GRAYSON: Yeah. I mean, if you understood how the source worked. It should work.

VESTAL: Yeah. And we published the first paper in *JACS* [*Journal of the American Chemical Society*], I think on that, showing that it did work.[6] But that was a lot of fun, because those guys…you know, they wouldn't quit. Usually, I'd go down after work in the evening from Baltimore, and work with those guys, and at 2:00 o'clock in the morning they were still ready to go.

GRAYSON: So, this was fairly close. Let's see, Baltimore and [Washington] D.C.? Probably about sixty, fifty miles?

VESTAL: Oh, it was less than that, because we were on, on the west side of Baltimore, and they were, kind of, on the north…usually about…not more than forty miles. I had some really good times with those guys. That was, I suppose, what...1968, 1969, I guess...1968. I don't remember when I first did it. I don't know what the date on the paper is.

GRAYSON: So, they were trying to get these biological molecules to [fly].

VESTAL: Yeah, and they very quickly showed all those things that they were used to worked very well. [They] got molecular ions on things that they…they could do electron impact, but they couldn't get [molecular ions on by electron impact].

GRAYSON: Sure, yeah. Yeah.

VESTAL: No. They were...they were very happy. And we built a number of them for MS-9s and then we built them for the Finnigan 1015. We made a deal with Finnigan and they took off with it. Then we licensed to other people to do their own, if they wanted to or we built them. I think mostly we built a number for the MS-9s and some of the [other instruments, but] a lot of the manufacturers just licensed and built their own, because it's a pretty easy thing to do, once you know how.

[6] H.M. Fales, G.W.A. Milne, and M.L. Vestal, "Chemical Ionization Mass Spectrometry of Complex Molecules," *Journal of the American Chemical Society* 91(13) (1969): 3682-5.

But, you know, we built [sources for] the MS-9s. […] That was the first one, I think, Ralph [C.] Dougherty's may have been the second one. He bought two MS-9s when he moved to Florida State [University] from Ohio State [University]. And we put one on one of those. It's amusing because Ed Kratfel, my machinist, guy that does it all, and I went down to install it. And of course, because we had all these pumps and everything, and it had little mechanical pumps on it. So, we basically, took a hacksaw and sawed off a piece of the frame and took the whole vacuum system apart and put it back together. And a couple of the guys from, I guess, AEI at that time. Whoever they were, ah, were installing and bringing up to speed the other instrument. And you know they're there in their coats and ties and this one guy was really good. He's like playing an organ. He's doing this and we came in with our hacksaws and using a torch and starting cutting this machine up. And they were on the floor. You can't do that. You can't do that stuff in the field. I said, "Well, why not?" [laughter] We, kind of, tore it up and put it back together and it worked pretty well. We built one for Don Hunt and I don't know how many more we did. I don't remember all of them…

GRAYSON: So, CI really was the way to go for…there wasn't any other alternative ionization technique.

VESTAL: No. For getting molecular ions on, you know modestly volatile things. I mean, you couldn't do really complete non-volatile…

GRAYSON: Yeah.

VESTAL: Then you can run on the solids probe. You could get pretty good spectra. So, of course, one of things Don showed fairly early <**T: 75 min**> on was that negative ion [worked] for a lot of things. I know sensitivity was good […]. And then of course, we worked with Finnigan to put it on their 1015 originally, and they sold a lot of instruments. They had that dual stack like, like Bill was talking about with chemical ionization on one side and electron impact on the other.

GRAYSON: Does anybody know whether there's a 1015 around, because we're trying to…CHF [Chemical Heritage Foundation] would like to find one, but apparently, they've all been [discarded].

VESTAL: I had one at one point, but I think I dismembered it. Honest, do you know what happened to the thermospray machine in Utah? I know Jim [James A. McCloskey] was trying to figure out somebody to donate it to, and I don't know what they did with it. It was in a 1015 cabinet. But it wasn't a 1015. The last time I was up there, while he was still there, they had moved it. You know, they had quit using it, and put it in another lab and had it just sitting there. And saying, really we ought to give this to somebody […]. I don't know happened to it. I

suspect they ended up junking it, but anyway, the [Chemical] Heritage Foundation didn't get it, I guess.

GRAYSON: No. No.

VESTAL: Yeah. He was talking about trying to donate it to somebody, but I guess he couldn't find anybody wanted it. God, this is about ten years ago.

GRAYSON: Yeah. It's probably long gone [now]. You know, ten years ago.

VESTAL: Yeah. I'm pretty sure it's not in Utah anymore.

GRAYSON: So, this SRI [Scientific Research Instruments Corp.], you were fairly successful with this. This brought in some jack to the company […].

VESTAL: We were doing okay. We had two major products, the chemical ionization stuff and then, Gordon and I developed this little medical mass spectrometer…

GRAYSON: Gordon…?

VESTAL: Gordon Fergusson. And it's just a little permanent magnet machine with multiple collectors, and a little tiny magnet. The reason we started it, there was a group up in Buffalo [New York] that had shown that they can sample oxygen and CO_2 out of blood with a membrane into a mass spectrometer. So, we built this machine to do blood gases. Of course, they had the electrodes and so forth for doing blood gases. And they made these probes with a silicone membrane and heparinized and so forth. It actually worked okay. It worked great in our arterial blood. It didn't work so well in venous blood, because […] the surface [of the membrane gets contaminated] and you couldn't get the consistent responses. Of course, the problem was in arterial blood is nobody wants to poke a hole in an artery unless they really have to. As a routine diagnostic thing, it just isn't going to happen.

But what we also found, [was that] the respiratory mass spectrometry had been around for a while. There was a real demand for respiratory measurements, both for looking at CO_2 and oxygen, but also, mainly looking at anesthesia gases. And we found fairly quickly in some of our working with people that a whole lot of people recognized that they needed to have a better way of measuring the anesthesia gases during surgery, because they were having lots of problems with people dying because the anesthesia was what's killing them, not the surgery. So, actually that was around 1970, when I left to go back to Utah. But Gordon continued that

and really built it up into quite a good product. And for a while, they were using them in almost every operating room. PerkinElmer had also developed a similar instrument which came from their submarine program, and eventually, Gordon moved to PerkinElmer, but that's another story.

Anyway, that was a very successful product. And Gordon and one of our financial people managed to sell the company to G. D. Searle [& Company]. They had bought Nuclear-Chicago and were in the business of buying up instrument companies at the time. So we all made a little money out of it, not a lot. But I was […]—well, I guess I'm getting ahead of myself. Bill Johnston died within a few months after I went to Utah in 1970. And Gordon took over running the company, and Gordon was a much better manager than Bill was. He didn't have that [flamboyance]. But he did a good job of making things work and making the company solid. And between him and one of our financial people, they managed to sell it to G. D. Searle for a reasonable amount of money. I think they spun it off to another company or whatever.

But anyway, that product continued for quite a long time after that. And I think some version, of those are still used in…maybe the PerkinElmer version now, I'm not sure. Those are still used in operating rooms. So, the blood gas thing was never really [a success]. We also measured gases in tissues, and so forth; that was interesting from a research point of view **<T: 80 min>** but never really viable. But the respiratory gas became really an outstanding tool. Doesn't need much of mass spectrometer.

GRAYSON: No, no.

VESTAL: They seem to be reliable and so forth. But anyway, that was, sort of, the [Scientific Research Instruments] story, I think.

GRAYSON: So, for some reason or other you decided to go to Utah. What was that about?

VESTAL: Well, I was at the point where I had published quite a few papers [by then], and I knew all the people in mass spectrometry, and most people assumed I had a Ph.D. by then, and I was getting to the age I should have had one. […] I realized that before Bill died, […clearly] he was having some problems, not physical, but he was becoming increasingly difficult in many ways. And I felt like I was sort spending most of my effort propping him up and I had to go to meetings with him, and have to step up to do the talking, because he [would] go out wandering off in some direction. And so I thought, you know I don't want to spend my life doing this. And there's no real future in it, and I really […] wanted to get an academic job. So, I thought, well if I want an academic job, I got to get a Ph.D. At that time, it was essentially impossible, I mean, to move into any academic job without a Ph.D. It probably still is to a great deal.

GRAYSON: Yeah, I think so.

VESTAL: People have done it, but not very many. Ah, so I thought, well, I'll go get a Ph.D., and I had two possibilities. Fred [Frederick W.] Lampe, who was a good friend and collaborator [had gone to] Penn State University. He would have been very happy for me to come and join him. But he could really only offer me a standard graduate stipend. And by that time, even though I could get some money from consulting with the company. I did really consider it, because I liked him and I liked Penn State.

But Jean is and still is a very good operator, entrepreneur. And, of course, also Austin Wahrhaftig was there, who I really enjoyed working with him…Henry Eyring. I mean really, the chemistry department at that time was really very appealing. I liked them a lot. And Jean was able to come up with a plan where they gave me the title of Research Assistant Professor. And so, that way they were able to pay me a little more than the ordinary graduate stipend. And he had enough grant money coming in to actually support that, so I really was supported by his research grants, but at a level that was not great but better than the ordinary. And that, plus my consulting, I was able to do okay. So, I went to Utah. The other thing, there was a feeling about Utah was they had this chemical/physics program which was really something that Hopkins would never have considered. And which I could come in from the physics side using the courses I had taken eight to ten years earlier.

GRAYSON: Oh, yeah.

VESTAL: And if I could pass the exam in physics, then I can get into the program that way, and then I could take courses in chemistry or do research in chemistry or whatever I wanted to do. So, I did it that way, and I entered chemical/physics. I spent the first semester reviewing all the stuff that I had done before, and had to stay the course, which basically, somebody sort of taking me through it, reviewing all the things you ought to know if you were going to pass the exams, and that was helpful to know what to study. The guy that did it was quite good. So, I took the exam and I passed it. Of course, I talked to Austin about what I ought to take, what courses. He said, "Well, you know, I think while you're here you really ought to learn a little chemistry." I said, "Yeah, I think you may be right." I had managed to avoid it so far. I had one course in physical, one semester of physical chemistry at Purdue. I can kind of back into chemistry, but the amount that I didn't know was even more than I knew I didn't know.

GRAYSON: Yeah.

VESTAL: So, I took undergraduate organic and inorganic, which was…well, I only took two quarters of organic, but it was enough, and I managed to pass it. I wasn't very good at it. And I took, you know, physics, I took graduate courses in physics. I took Henry's course, and so forth, and Henry Eyring's course. And I taught a p-chem lab. I guess [the University] did pay

part of my salary, because I did teach a little bit. I had taught a p-chem lab. So, Milton [L.] Lee was one of my students, the first year I was there in the p-chem lab. He has become quite famous.

GRAYSON: I'm sorry, Milton…

VESTAL: Milton Lee. He's here.

GRAYSON: L-e-e, Lee.

VESTAL: Yeah, L-e-e <**T: 85 min**>. Ah, but you know it really was a good department. They had good students. The only, ah, problem, teaching physical chemistry was a lot of the students were med students. Because Henry Eyring being a physical chemist—and how big the guy really [was]—almost ran the university at one point. He had stipulated that medical students should learn physical chemistry.

GRAYSON: Wow.

VESTAL: Not many medical students, I mean pre-med programs, do that.

GRAYSON: No.

VESTAL: So, they were required to take the same physical chemistry courses as the chemistry majors.

GRAYSON: Oh, that must have been exciting.

VESTAL: So, when you taught p-chem, you had big classes and they were 80 percent or so medical students. I mean, these guys wanted a grade. They didn't give a damn about how anything worked, right.

GRAYSON: Yeah. They go for the grade.

VESTAL: […] When I first went there, I wasn't teaching the course. I was teaching a lab that went along with it. I had this one guy that was really a good student. He was very bright. I didn't know just how well he'd done before; but people have their diversions. He didn't turn in his lab reports until after the final exam, basically. Obviously, he hadn't really paid much attention to what that…he hadn't turned in his lab reports on time. He did very poorly on the final exam. And when he did finally turn in the lab reports, they were excellent. He did a really good job, and I knew by then, he knew the material. If he took the exam again, he probably would have done well. But [as the result of all that] he got a B instead of an A. And he came bugging me for months afterwards because it was the only B he had ever had, straight A's and he was worried about getting into medical school. I said, "Well, I'm sorry, you know. I understand your problem." But I said, "Based on the numbers, I mean, I can't do anything else. I mean, I can't change it now. You could have changed it, you know. I know you could have [done better], if you would have…"

GRAYSON: Done it right.

VESTAL: But you know, I think he probably learned more from that than he would have if I'd given him an A, I hope so. He was a good kid.

GRAYSON: Yeah. It's unfortunate that happened. That happened to me when I was in graduate school, taught a physics lab. And I went back up. I was in Maryland at the time. This guy called me back in Missouri wanting to get his grade changed, because he needed to get it bumped up. I, well, you know, what can I do? I can't change everybody's grade.

VESTAL: Yeah. Well, it's unfortunate, there's that much emphasis on grades […]. I understand there's a concern, but he was concerned at the wrong time, as far as I was concerned.

Anyway, later, you know, after I got my Ph.D. and I did spend one year there as a research associate professor, and I did teach the regular p-chem class at that time, and of course, Henry Eyring always made a point of teaching one section of p-chem as well […]. He always taught one of the graduate sections. And, in those days he traveled quite a lot, so he always would need somebody to trade off with him or take his class. So, I had the honor of teaching a few of Henry's classes for him.

GRAYSON: There you go.

VESTAL: I did it on one stipulation […]: part of the one section he covered was the absolute rate theory and kinetics. It was his thing. And I said, "I'm glad to do it, and I'll do as many as you need me to do, but I want you to come and teach my class when we get to this section." So, he agreed to do it. I sat and listened, and of course, I'd taken his course as well. He's one of

these spellbinding professors. He can stand up there and lay this out and it just seems so clear, you know. Just no doubt, you understood everything, except a little later we sat and thought about it, now wait a minute now.

GRAYSON: Yeah. I've had some of those professors. It's transparently clear, until after it's over you…how did they do that?

VESTAL: And, you know, Austin Wahrhaftig was the exact opposite. Austin was a guy, if you sat down with him personally and asked him questions, he knew everything. It was really very helpful. He had an abundant store of knowledge and wisdom and all, and on an individual basis, he was the best guy I ever worked with. I never came up with a question that I had to ask that I didn't come, away feeling like, I didn't know the answer. In the classroom, he was absolutely, awful. I didn't take his class. He taught a class in molecular spectroscopy, I think from Herzberg. And he was an expert at it. He knew [the material]. You could ask him anything about it, and he could [tell you all about it]. But teaching in class, I'm glad I didn't sign up. At that point, I wasn't needing any more courses for credit. But I thought I would sit it on it, and I'd learn something. But, I could not even sit through the whole class.

GRAYSON: Wow.

VESTAL: Because he was one of these people that couldn't help but contradict <**T: 90 min**> himself. He'd start off to explain something, and then, stop all…no, we have to…and he would go back. And by the time he got done…

GRAYSON: Everybody was confused.

VESTAL: Nobody knew what the hell he had said. A lot of personality, I guess. Anyway, he's still one of my favorite people, although he died a few years ago.

GRAYSON: So, you ended up getting your degree, in what?

VESTAL: In 1975, I guess, when I finished up the work in 1974, but I didn't get my degree until 1975.

GRAYSON: And that was chemical-physics.

VESTAL: Yeah. Everything I did after the preliminary stuff was in chemistry.

GRAYSON: So, you were able to make use of the Hopkins coursework?

VESTAL: Oh, yeah. I didn't take any more courses, except for that one review course; I didn't take any physics courses.

GRAYSON: Well, that's good. That's a nice program. That seemed like the [easy way to go].

VESTAL: Oh, yeah. I thought it was great. And you know, there were a lot of people in it, and they had some really good people in physics, as well. […] Frank [M.] Harris was the professor in physics. I may have taken his course. I don't remember now, but I didn't take very many. And that was kind of a statistical mechanics sort of course.

GRAYSON: So, finally you got this…what do you call it? Union card as they say and you need to find a job.

VESTAL: Yeah. Well, I picked up the degree. I mentioned this in the talk the other day. Around that time, I had been doing this [photo] disassociation work with the triple quad, and I believe I built the first triple quadrupole. Jim Morrison built his very nearly the same time, but he was in Utah when I built mine, and he finished his up and went back to Australia. That's the one—[Richard A.] Yost and [Christie G.] Enke used for their work. And I built a triple quad to do photo disassociation and I considered collision disassociation to be a royal pain in the ass. […] So, anyway, ah, I was doing that work and was funded by the Aeronautic Research Lab [ARL], Tom [Thomas O.] Tiernan had taken over the position Jean had had formerly at ARL, and he had assumed [the job] of project monitor and I was supported doing this work. And we were making good progress and we did a lot of atmospheric ions and stuff.

It didn't work for some of the things we originally planned to do. It didn't enough work for some of the things we originally planned to do because we didn't have enough charge. But for a lot of the ions, like O_3^- and O_3^+ and these things, we got some really nice data...CO_3^-. Anyway, we were doing that work, and Richard [M.] Nixon and one of his fits of wisdom, I guess, I'm not sure exactly, what the background was…but they cut the research funding for the Air Force and in doing that, they cut out this Aeronautical Research Lab which was a basic research group. They kept the group that was doing "applied" research which was a total waste of time, because those guys were just doing [something…. But] the Aeronautic Research Lab had a really great group of scientists, doing good work internally, as well as for the outside. But anyway, the whole thing was just shut down. So, my funding disappeared.

GRAYSON: Ouch.

VESTAL: At that time, getting funding for fundamental chemical physics or physical chemistry was not that easy, and particularly if you're in my position where you're not really a [professor], you're in between. I was dependent on Jean for lab space as it was, but that was no problem. He was glad to supply the space, if I supplied the money. He even [supplied] some of the money, but it was clear that there was no future for me there in that position. So, I started applying for real [positions], which is what I intended to do anyhow. It kind of encouraged me not to fool around and get after it…

GRAYSON: Right, get on the stick and get going.

VESTAL: So, I started applying for all the openings that appeared in *C&E News* [*Chemical and Engineering News*]. The professors in chemistry were either analytical or physical or something in between. And one of the places…I applied for a number but, one of the places I applied was in Houston [Texas]. In all my applications… because they ask you for references and so forth, of course, in addition to Austin and Jean, I picked Frank Field and Joe Franklin as references, even though I never worked with either one, I had interacted with Franklin, you know.

GRAYSON: Sure.

VESTAL: […] And I'd known Joe from the meetings, and Joe was one of those guys, once you went to the meeting you knew him for life. He was very friendly and always looking for people to go out to lunch or dinner with him, and he always went to the best and most expensive restaurants in town, but they were good. But anyway, I got to know Joe pretty well. Scientifically, we [never] really [worked] together. So, I listed them both as references.

GRAYSON: At the time, probably both [of them were], still in Texas…

VESTAL: No. Joe was at Rice [University] <**T: 95 min**>.

GRAYSON: Oh, okay.

VESTAL: And [Frank] had already moved up to Rockefeller [University]. So, I'm sure Frank wrote me a very nice letter. I don't know that I ever saw it, but I do know that the main reason I

got the job is because Joe not only wrote a letter, he called the chairman of the department, and said, "This guy is a really good guy. You ought to hire him."

GRAYSON: There you go.

VESTAL: I think that did more for me. Then I went down for the interview, and it went well. This is the place where Gerry [G.] Meisels had been. I sort of was Gerry's replacement, although he was chairman of the department or something, and I wasn't going to do that, but I was filling the slot that he had left open, because he was leaving there.

GRAYSON: Yeah. I guess he went to [University of] Nebraska, then.

VESTAL: I don't remember the continuity. He went to Nebraska, then to [University of South] Florida, and later... anyway, he left Houston a year before I went there. Yeah, must have been Nebraska. I knew Gerry quite well, but we didn't overlap.

GRAYSON: So, you're ready to go down to Houston, and what did you guys…what did you do when you got to Houston?

VESTAL: Well, I took the triple quad and photo dissociation project with me. Jean had no interest in that. It was really my project from day one. I didn't have any funding for it at that point. I had the instrument, and I could get graduate students in. I could do that. But what I'd done, right around the time that I was finishing my Ph.D., I think it was actually before I even got the degree, I wrote a proposal to NIH [National Institutes of Health] to do LC/MS. And, this is the thing I talked about yesterday, that CO_2 laser that I talked about. And Jim McCloskey, who had been in Houston at Baylor [University], had come to Utah for part of his sabbatical, around the time I was finishing my degree or even before I finished my degree. While he was there he got an offer to come full-time to Utah, so he went back to Baylor briefly, and resigned and then moved to Utah.

So, he was then professor at Utah by the time I finished my degree, 1974, 1975, I'm not exactly sure. Anyway, he was a co-investigator on my proposal, because I needed somebody with some biological clout. Because at that time, us metal-heads could get money from NIH, but they were very skeptical about funding people to do things that didn't obviously have some biological utility. So, having Jim aboard helped immensely, and also because he, he knew how to write grants for NIH, and I had never done that. I learned a lot from him in terms of what to do and what not to do, and so forth. So we wrote the grant application. In those days, they had a lot more site visits than they do now. So, we had a site visit of one for that project. Klaus Biemann came out, with a string tie and his western garb. It was really…and of course, Jim was a student of his…

GRAYSON: Oh, yes.

VESTAL: So, anyway, the three of us sat down and we talked about the project and satisfied his concerns. I think he went away with a strong recommendation because we did get funded then, fairly quickly. So, I started the project in Utah, in Jean's lab. And of course, Jean was a co-, whatever. I was principal investigator but it was understood [that] it was in his laboratory and of course, at the University. Then shortly after that project came through, I got the offer from Houston, which I accepted.

And the problem which I hadn't realized until then, the project had already started in Utah; we started designing, and building the machine. Calvin Blakely was working with me at that time. He and I had worked together building a crossbeam machine […] for my Ph.D. dissertation. So, we had worked together and very effectively on that one and he agreed to work on this new project with me. So it turned out that after I got the grant, and then I made the move, and I hadn't realized until then, the grant was to the university, not to you. And so they really couldn't move the grant to Houston. They could terminate the grant in Utah, and fund a new grant in Houston, if I could get it approved. And they were willing to do that. But the problem was we [bought] all this equipment on the grant in Utah. So, at that [point], Utah's position is well, that belongs to us.

GRAYSON: Yeah, right.

VESTAL: You can't take that. You know, I don't care if the NIH did pay for it. But if I move to Houston, then I had <**T: 100 min**> to agree to duplicate that, and that wouldn't have been real good. So, we had a little go around on that and, I must confess that, one of the reasons Jean and I had a fairly cool period…I'm not sure I should put this in. But, as you know, he really sided with the University on that. He would have been very happy to keep that equipment. And you know how professors are: they don't like to let anything out of their lab. And he and I had started a separate project. It was funded by a different agency, which he was PI on, and I had contributed to, and I was perfectly happy that that stayed there. It was a little rocky for a while, but Jim really stepped up and made it possible, because he and I worked out this plan which the university agreed to is that, they would let me…it would belong to the university, but they would let me take it to Houston with the proviso that [when] we had a working system that Jim could use, we would put it back in his laboratory in Utah. And for all practical purposes, we'd call it the same equipment—it didn't necessarily have to be. They terminated the grant after nine months, which was somewhat after I had moved to Houston, but Cal Blakely stayed behind to sort of look after it until we did that, and then he [moved] to Houston.

GRAYSON: Well, it's kind of interesting that McCloskey was able to pull those strings, because he was just fresh on at Utah.

VESTAL: Yeah. But, you know, he made a good case to the University […that] they were getting something more valuable than just a bunch of equipment. They were going to get a functioning [instrument].

GRAYSON: Well, yeah. I mean, what was the equipment going to do? Like, you said Jean wasn't particularly interested in the project, right?

VESTAL: Well, not too, but he would have done something [with it]. But he wouldn't have had that grant, because it was going to end, because I was PI. I controlled the grant, but I didn't control all the equipment. But anyway, you know Fred went through a similar thing at Purdue…

GRAYSON: Yes. That seems to be a common problem…

VESTAL: I'm not the only one. Anyway, we did work out a compromise that worked very well. We shipped the equipment all down to Houston. We built the instrument, and eventually got something that worked, and we put it back in Jim's lab, and he did wonderful things with it. So, it worked out really well. I really enjoyed the collaboration with him over the years. Very successful…

GRAYSON: I'd like to back up a little bit and talk about some of these "firsts" items. You were saying that [James D.] Morrison is credited with the first triple quad, but you said you're…[7]

VESTAL: I don't know if he's really credited it with it. [But certainly the one that my] friends, Enke and Yost, used was the one he built. I don't even think they were even aware that I had one to tell you the truth. I published the first paper for triple quad.[8] It's a photodissociation paper; Jean's the coauthor on that.

[7] Christie G. Enke, Richard A. Yost, James D. Morrison, "Tandem Quadrupole Mass Spectrometer for Selected Ion Fragmentation Studies and Low Energy Collision Induced Dissociator," U.S. Patent 4,234,791, issued 18 November 1980.

[8] M.L. Vestal and J.H. Futrell, "Photodissociation of CH_3Cl^+ and CH_3Br^+ in a Tandem Quadrupole Mass Spectrometer," *Chemical Physics Letters* 28(4) (1974): 559-61.

GRAYSON: Okay, okay. So that kind of ties down some of the early…because I mean there's always this issue, it's first, and it seems like it's difficult. Because there's always this dual…you know, somebody does something similar, the same thing…

VESTAL: Well, I don't claim that Morrison got the idea from me, and I don't think he claims I got the idea from him. I'm sure we did discuss it. He liked to come and spend six months or so about every year up in Utah. He liked Utah. Great guy. He and Austin were really good friends. So, he came quite frequently, and I know he was there at the time I built the first triple quad. And I started out...you know, Austin had been trying to do photodissociation on a magnetic instrument when I arrived there. He had this huge magnetic instrument that actually, Henry Rosenstock and Merrill Wallenstein had been involved in building […]. They were not lab geniuses by any means, but Austin was. He was very good in the laboratory. Anyway, this is a very fancy machine and they were trying to do photodissociation in the field desorption space between the magnet and the electric sector. And [they had] this mammoth xenon light source and stuff. And I spent a few months trying to make that thing work. The electronics were all vacuum tubes and it drifted like crazy. And I, I tried very hard for a month or two to make that thing work, and I decided, no, this just isn't going to do it.

GRAYSON: But also, the ions weren't spending very much time in between, in the [field free region].

VESTAL: No, they didn't. The ions were going real fast, and you don't have the interaction. [If] you really sat down and did the numbers [carefully], there just is no way we were going to get enough signal to make it useful, even if we did everything perfectly. By then […] I had worked with chemical ionization quadrupoles with Finnigan and several others. When we went to a quadrupole, we could put an RF only quadrupole in the middle and then we could probably make it work. And in fact, we did make it work with an ordinary mercury light source, initially. We got dissociation. In fact, that first paper may actually be that, I think. Then of course, the tunable dye lasers were just coming out <**T: 105 min**> at that point. So, we very quickly got a tunable dye laser and then we were really in business.

And of course, it's amusing what ignorant things you do sometimes. My original reason for doing this was that […] we had gotten very interested in the question that Field and [Paul] Kebarle and other people were looking at, the amount of energy of these clusters, like an H_3O^+ and H_2O and so forth. We thought we were measuring…there was a controversy, [in the work] between Field and Kebarle, and what those numbers really were. We could do photodissociation of these clusters and make them by chemical ionization. And of course, the trouble is in the wavelength range that was accessible at that time, there's no charging. And if you don't get a charge, you don't get any disassociation.

GRAYSON: Yeah.

VESTAL: And I guess I was kind of naïve about that at the beginning, but we soon found things that do absorb, like a lot of the CO_3^- and O_3^- and O_3^+ and the whole series of atmospheric ions that do have strong absorption bands in the accessible region […] and we got really good results. And of course, this is what Morrison was interested in doing too, was spectroscopy of ions. But, I was more interested in the energetics […]. He really was interested in the spectroscopy. And that, that's what was really good for him because you could get really exquisite mapping of the spectroscopy of these ions. But you couldn't really do that any other way. That's what he built his instrument for was to do spectroscopy. To do essentially, the same thing I was doing, but do spectroscopy of some of these ions. And, I'm pretty sure his instrument was built after mine, but I don't know that it really makes any difference.

GRAYSON: Would you have any pictures of your triple quad in your file someplace, do you think?

VESTAL: I doubt it.

GRAYSON: That's too bad.

VESTAL: I did have. In stuff I threw away [there] was certainly some. Well, I may actually have…yeah, I probably have a…well, it's in the papers.

GRAYSON: Okay. I can certainly, get the paper out of the literature and take a look at that.

VESTAL: Yeah, I think…

GRAYSON: More of the problem is most of the stuff that they…when they reproduce photographs they use a second rate, ah, scanning system […].

VESTAL: Well, I'm not sure. There may not be a photograph, just may be a schematic. I'm sure there's a schematic with it. I probably don't have it. I don't recall even making a photograph. Those days you didn't do much of…we didn't photograph the stuff much. You drew a picture of what it looked like and what it did. You know, we weren't trying to sell anything.

GRAYSON: No. But I mean, as a documentation thing or whatever.

VESTAL: I'm sure there's a schematic of it, and you know certainly, in the day and what we did. I'm sure that first paper is with a conventional light source.

GRAYSON: Okay. Well, I can certainly, dig that out of the literature without any [effort].

VESTAL: And that's the first time we showed…far as I know, photodissociation of ions in the gas phase. [Under mass spectrometer conditions].

GRAYSON: That was with Futrell?

VESTAL: Yeah. I think Jean was certainly coauthor on that, because that was before I had the grant from [ARL].[9] […] There it is. What does it say? Methyl chloride, methyl bromide and tandem quadrupole mass spectrometer…

GRAYSON: All right.

VESTAL: *Chem Phys. Letters* 1974.

GRAYSON: All right. I'll dig out that.

VESTAL: And then there's a whole bunch of other papers with me, and [G. H.] Mauclaire and then my other students—a whole series.

GRAYSON: Yeah, okay. I'll make a note here.

VESTAL: That's the first one.

GRAYSON: This paper here. […] Futrell, when he was at Wright-Patterson, had a crossbeam mass spectrometer that he…

VESTAL: No. No. He had a tandem mass spectrometer.

[9] Ibid.

GRAYSON: Tandem mass spectrometer, okay. I thought he had [a crossbeam instrument].

VESTAL: I think by CEC. Wasn't it. It was built by a commercial [company] put to his specifications.

GRAYSON: Right. So, I think he refers to that as one of the first MSMS instruments.

VESTAL: Well, yeah. It really was, perhaps the first one, I don't know.

GRAYSON: Because I think, McLafferty, […] is in that category as having an early MSMS instrument […].

VESTAL: No, I think Jean's was before that with Jennings.

GRAYSON: Okay, very good.

VESTAL: Because he, he had the idea of doing it, and he had the money from the Air Force. He got the companies to build it for him to his spec, and it really worked very well. It was a good instrument. I think he thought he could do well with the low energy collisions and what some of the others built. No, I built a crossbeam machine for my thesis in Utah with the help of Calvin Blakely. And <**T: 110 min**> these were all the calculations from it. This is my Ph.D. dissertation in 1975. This is probably the first paper, I guess.

GRAYSON: Crossed beam study reaction of...

VESTAL: It's H_3^+ and D_2, *Chem. Phys. Letters* 1974.[10]

GRAYSON: All right.

[10] M.L. Vestal, C.R. Blakley, P.W. Ryan, and J.H. Futrell, "Crossed-beam study of the reaction $H_3^+(D_2,H_2)D_2H^+$," *Journal of Chemical Physics* 64(5) (1976): 2094-111.

VESTAL: That's very close to the same time. But, there was a bunch of crossbeam papers after that. And Jean continued [work with] that machine. He moved it to Delaware and was running it up until a few years ago.

GRAYSON: Yeah. So, then you really got situated in Houston, and then you spent a fair amount of your career there, about eleven years in Houston…

VESTAL: Yeah. Well, I went to Houston in 1976, right.

GRAYSON: Ah, yeah.

VESTAL: 1976, yeah. And, I started Vestec in 1983, but I stayed at the university full-time at first, and then part-time, and I guess I left in 1985 or 1986. I don't really remember exactly. [laughter]

GRAYSON: Okay. Well, you put down 1987 here.

VESTAL: Okay, maybe 1987. I was full-time the first year or so, and then, there was some complaints about I wasn't spending enough time, so I went to half-time, and then I went out the door altogether. It was, kind of, a gradual transition. I stayed in Houston, but not at the University.

GRAYSON: So, did you have any teaching responsibilities?

VESTAL: Oh, yeah, sure.

GRAYSON: So, it was the standard…?

VESTAL: No. I was professor of physical chemistry and most of my students were analytical chemists, and I had a couple of physical chemistry students. I had analytical chemists beating down my door that wanted to come and work with me. But I had money and I also was doing things that were interesting to a lot of people. And I was doing mass spectrometry, and there wasn't anybody else doing that.

The best students I got were the analytical students. I had a couple of good p-chem students. So, Joe [Joseph F.] Hiller, [Jr.], worked on the photo disassociation work and, later

[K. Viswanath] Vish Katta worked on […] ion production from charged liquid droplets, but you know physical chemistry students were few and far between in Houston. And but the best thing by far that I had in Houston was Hee-Yong Kim, who everyone in ASMS knows these days, I think.

GRAYSON: How do you spell that?

VESTAL: Last name is Kim, K-i-m. H-e-e – Y-o-n-g. She's a former officer […]. She got a promotion recently; she's a fellow of some kind at NIH. She did the work on applying [this to] protein sequencing. She's rather a petite Korean lady, who was made of iron, but you wouldn't know that at first. But, she came to see me shortly after…her husband had an appointment in biophysics, I believe. She was in chemistry in the analytical division. She had a master's degree in pharmacy, I think, in Korea. And she came to me and said she wanted to work with me. I said, "Well, I don't know, [what] your background is…I'm not sure that […this is] really right for you." I said, "I think you ought to really talk to some of the other professors and see if there isn't something that fits in better with what you want to do." And she did that. She went back and she came back, and said, "I want to work with you." She kept doing that until I said yes. And she was great.

GRAYSON: So, what did she end up doing? You said she was doing protein [sequencing].

VESTAL: Yeah. She was the one that did most of the work on the application of thermospray to peptides and proteins. Working with Doug Dyckes. She really blossomed, had a baby while she was…the baby was in the lab and in a bassinet when she was writing up her thesis. [laughter] Didn't slow her down at all.

GRAYSON: Interesting. So, then you had kind of a standard, what you would call academic career at Houston for that period…

VESTAL: Well, I suppose. How standard it was, I don't know. I never really felt all that comfortable in the academic world to tell you the truth. I thought that's what I wanted to do. But it, you know it's really different when you're there than what you might imagine. I mean, it could be quite comfortable if you just wanted to, do your thing, and have an easy comfortable life. You could do that.

GRAYSON: But you had to still get grant applications.

VESTAL: Well, the thing is, with the thermospray I was getting plenty of money. I had more money than anybody else. And I was doing things…academically it's…oh, that's engineering or something other than [what we do]." And the department was such that, the department [gave] very little <**T: 115 min**> support maybe because the University didn't give them much support, I'm not sure. But I basically…I wanted secretarial help, and I pretty much had to arrange it myself.

GRAYSON: Did it yourself, yeah.

VESTAL: We had a machine shop, but they were overwhelmed with doing nitty-gritty little things for the organic chemists. And they were pretty good, but it just…any major project just took forever for them to get it done, so I ultimately went outside to get machine shop work out. Some of this electronic stuff, we did ourselves for the most part, you know, had a good glass blower. [laughter] We didn't need a glass blower very much. He was a nice guy. But, so the university support was really very minimal and, […] they were happy to have the money I was bringing in, because of course they get the overhead.

GRAYSON: They get the overhead. So, what was your overhead rate there?

VESTAL: Oh, it was pretty high. I don't know what it was, but you didn't get anything back for it, you know. That's what was irritating me. You know, I was bringing in all this money, and I was a second-class citizen as far as the university was concerned. Then this thermospray patent came along, and we did it through the University and which was fair enough.[11] But then we looked around and, you know, there wasn't anybody really going to do anything with it. They were interested, but they weren't going to put resources into it. So, I said well, it looks like if this is going to happen, we're going to have to make it happen ourselves. So, since none of the companies ever stepped forward, and I talked with Finnigan and other people about licensing, you know. Said I'd be happy to consult for him or whatever. But […] they weren't ready to put out any money at that point; it hadn't been proven to me.

So, basically I ended up forming the company and taking exclusive license to the patent with the right to sublicense to other people, which we did. And of course, because we developed the technology outside the University, what we charged for the license was somewhat more than what we paid the University. I think we charged about twice what we paid the University, which we kept because we'd licensed not only the patent, but the technology. And we also, had some additional patents that we'd done through the company. And so, you know we licensed.

[11] Marvin L. Vestal, "Thermospray Methods and Apparatus for Interfacing Chromatography and Mass Spectrometry," U.S. Patent 4,902,891, issued 20 February 1990.

We built OEM [original equipment manufacture] things for a lot of people, and we licensed pretty much all of it. Well, everybody that we built an OEM for, part of the deal was we'll build it for you, and this is what it's going to cost. But we also would like for you to just take a license for the technology. So, if you want to take over manufacturing that's fine with us. And that way there's no conflict in the future, and so we did that basically, with all the major manufacturers except VG [Vacuum Generators]. It kind of took off. It got to be quite a pretty good business and, so I was kind of in this awkward position of straddling the fence. Some of my colleagues were kind of unhappy that I was doing it, first of all. And secondly, thought it was taking away from my duties in academia, which it did sometimes, I suppose. Then my good friend, Fred Findeis at NSF [National Science Foundation]…. He and I had worked together in Johnston Labs, many years ago, before. He was an interesting character…

GRAYSON: How do you spell his name?

VESTAL: F-i-n-d-e-i-s, I believe. Alfred, ah, let's see. Fred was actually his middle name. […] Arthur F. Findeis. That's his [name]. I only really know him as Fred. But he was in charge of the analytical group at NSF for a long time. He was a good friend, but he also was one of the people who had very strong opinions about things and I had an NSF grant at that point. He found out I wasn't spending full time at the University, he killed my grant. [laughter] You know, this is work…Vish Katta was doing, really was doing some nice work, but that's another story. Anyway, so it was pretty clear, I really had to make a decision, go one way or the other, and I guess my rationale was that the University would probably survive without me, and the company might not. So, I decided to go with the company.

GRAYSON: So, we need to back up a little bit and talk about how thermospray came into being. It didn't just hop out of your head one day.

VESTAL: I did that yesterday [during the Waters Symposium on the origins of LC/MS].

GRAYSON: But that was more about, how it ended up…did you develop it primarily for LC/MS?

VESTAL: Oh, yeah, it was strictly LC/MS.

GRAYSON: That was the motivation?

VESTAL: […] This is what people want to do with LC/MS. They want to do a milliliter [per minute] of water, and all these other techniques like what [William H.] McFadden was doing,

and [Jack] Henion and other people were just not going to be able to do what people said they wanted to do. Let's come up with a way of doing that. And so we listed these design goals <**T: 120 min**> and concluded all these things. And we actually did, except for sensitivity, as I mentioned in the talk, we pretty much achieved all the rest of them. Although, what we proposed initially had no chance of working. [laughter] And it was a very expensive way to do it. […] That's probably the best thing to do to get funded; propose something that is far enough out that nobody could say it won't work, because otherwise, if you [propose] something simple, they'll know why it won't work. They'll tell you that won't work because of this, even though they might be wrong. So, anyway, I proposed this fancy thing with the CO_2 laser and liquid jet, because see the conventional wisdom was that it was pretty well established that, on some work that had been done in various places, but particularly, Lew [Lewis] Friedman…

GRAYSON: At Brookhaven [National Laboratory].

VESTAL: Brookhaven, yeah…had published some work that showed that if you heat it really fast over a short region, you get things into the gas phase that wouldn't [otherwise vaporize]. And he had this model at that point in time, which is, I think is bogus, but that's the story. Anyway, you know, it was pretty well accepted. We believed that if we could just heat this liquid really quickly and minimal contact with surface, maybe we could get these things into the gas phase.

GRAYSON: Without destroying them.

VESTAL: Essentially, that's right. That's what electrospray is of course, except it's doing it not with heat, but with something that, something else, but it is the same basic concept. So we thought, well, how could we do that? Well, you know, powerful lasers were available at that time, so we got a […] 50-watt CO_2 laser which is enough energy to do vaporize a milliliter of water if you get it all absorbed. […] It worked with molecular beams and with this crossbeam machine, so we knew a lot about how to do it. So, we basically built a beam apparatus with the laser beam crossing a liquid jet, and it was going to vaporize the liquid jet, and shoot it into the mass [spectrometer]. We had differential pumping […]. So, it sounded pretty good. As I said, I thought it was a reasonable proposal.

And it was, I suppose, in a way, except that what we soon discovered was that when you try to heat a liquid jet with a laser, it's not so simple. Basically what we made was beautiful ice crystals and stalactites. Look in the window and you'd look in and see these things growing, and didn't matter if you had the laser on or off. You still see them. Well, of course what happens is the damn things won't sit still. You hit them with a laser and they move. They vaporize on one side and go flying away, so you get a little energy, but no way you get any significant amount of energy absorbed and so that didn't work.

But, as I showed in the talk, we very quickly realized the way to do it is just…I think we did it intentionally, or might have accidentally. We moved the laser over to where it just heated the end of the metal tube. Then we could get vaporization. Then we could also get relatively non-volatile molecules to fly with EI and CI and we had this enormous apparatus with multi diffusion pumps and fuel pumps and the biggest diffusion pump I had ever seen, although John [B.] Fenn had bigger ones. We got it working and we actually, met most of the criteria we had proposed. Of course, there was a huge expensive, user-unfriendly sort of mess. You say, "Gee, I'm heating the end of a metal tube with a 50-watt laser. Now, what am I doing?" So that was still with this concept that we had to keep the heated region short.

We went to these oxy-hydrogen torches. So, we had an array of about four oxy-hydrogen torches. And heated up a just a little longer, but actually worked better than the laser. We had more power in, and that actually worked. We got data, and we did both CI and EI and, ah, we built a simpler apparatus [that just did] CI, and we didn't need so much pumping. And that was something that was usable. And that's [what] actually went to [James A.] McCloskey's lab to fulfill our requirement to send it back to Utah. We kept all the big pumps, and stuff, because we didn't have any use for them either. But, we sent him an apparatus which included a lot of the stuff we bought the first year, but some stuff we bought later. Anyway, we sent a working instrument to his lab.

GRAYSON: What year would that be, do you recall?

VESTAL: Well, it would have been around 1980, 1981. Ah, I think 1980, because…well, something…I mentioned this the other day. This of course, is almost a famous story. In, I guess it would be late 1979, we <**T: 125 min**> had this apparatus built on a smaller vacuum system with the torches and so forth. And we were working on optimizing its performance with chemical ionization and with filament. And this really is a true story. I didn't make this up, although I could have. The student, John Connolly, and I were working on the instrument. It took two people to do it because optimizing the torches and, and running around doing things. But we were taking data and I think we were using adenosine as the test sample and trying to optimize the intensity of the—minimize pyrolysis—and get maximum sensitivity. And we'd been playing with it for a while, and then suddenly, the parent ion intensity got two orders of magnitude larger than it had been. And we said, what the hell did we do, you know, to change them that much?

I'm looking around and about that time, Calvin Blakely walked in and looked over our shoulders. We were kind of fussing around with the instrument. And he said, "When did the filament [burn out]?" We were shooting, kept shooting more samples and getting this really fantastic…before the peaks were down here and suddenly the peaks are up here, you know. And he looked at it and said, "Did the filament burn out before or after you shot that last sample?" I don't know. I assume it's still on. We're getting ions and I looked and the [trap current] gauge was reading zero, so we shot another sample, and sure enough, we got this big signal. Ah, that's pretty interesting.

Making ions from magic…it's a magic way of making ions. And so, of course, we scratched our heads about that a good bit, […] and then we started reading the literature and discovered the paper by [B. A.] Thomson and [J.V.] Iribarne, who looked at ions from charged droplets earlier, a different [approach]. But, clearly when I read their paper, I said oh, this explains a lot, and I went back and read a lot of other literature about charging the liquid droplets and so forth, and there's a whole bunch of papers from the - damn, that guy's name [keeps] eluding me—[Leonard B. Loeb]

But, Dodd and Chapman, and a couple of other guys, but they all came from [Loeb]. It's on the tip of my tongue. Anyway, very famous physicist and he studied these phenomena for a long time, and he was one of the few people in that period that didn't put his name on student's work. So, all the papers are written by the students.

GRAYSON: Oh, wow.

VESTAL: This guy, it was in his laboratory and clearly he had a lot to do with what they were doing, but that was just his way of…

GRAYSON: So, it's hard to trace in the literature?

VESTAL: Well, you can trace it, but I can come up with his name, it's just right now one of those names eludes me for the moment. I know it very well.

GRAYSON: With the idea of having the filament in the system was to ionize…

VESTAL: Yeah. We were just doing chemical ionization.

GRAYSON: Right.

VESTAL: And that worked.

GRAYSON: So, when you turned off the filament, then the chemical ionization process stopped.

VESTAL: Well, yeah. But we still had ions.

GRAYSON: I know, but why were the ions less when the [CI was operating]?

VESTAL: Well, because I assume the electrons were actually […] neutralizing the ions because we were making the ions by the spray process.

GRAYSON: So, or so you were making…you were getting positive ions out.

VESTAL: Yeah.

GRAYSON: So, when it went into the ion sources with the [electrons], you were [neutralizing your positive ions].

VESTAL: Well, because we had a plasma in there at that point, a pretty intense beam…

GRAYSON: Yeah.

VESTAL: Some ions were being discharged by the […] chemical ionization you make a reasonable…plasma. Anything that was charged coming in gets modified.

GRAYSON: So, then did you just withdraw the CI gas altogether?

VESTAL: Well, we weren't using CI gas. We were just vaporizing solvents. Vaporized solvent was the CI gas and, it went right through and pumped away on the other side…

GRAYSON: Sure.

VESTAL: But that was pretty, that was a little hair-raising, to realize we discovered something that we can now make molecular ions from all kinds of non-volatile things, and nobody knows how to…well, that's not quite true. Field desorption could do it. I mean, with some pain. And just shortly before that, [Ronald D.] Macfarlane had published his californium stuff, I think in 1974 or 1975.[12] He was getting ions from non-volatile [compounds with his] equal amazing

[12] R.D. Macfarlane and D.F. Torgerson, "Californium-252 Plasma Desorption Mass Spectroscopy," *Science* 191(4230) (1976): 920-25.

approach. If you were trying to think of a way of not making intact molecules, you'd think that might be the way. You hit it with a bowling ball, and it's colored with some fission fragments. But it works. I don't think Ron thinks he gets enough credit <**T: 130 min**> and he probably doesn't because while his approach never became really practical, it, sort of said you could do it, you know. There are ways you can do this, so you can…

GRAYSON: I mean, at least in my mind that's an important, Macfarlane's contribution is important in my [view].

VESTAL: Well, it is in mine too.

GRAYSON: And I try to reference that paper in everything that I present about that period, and what's happening in ionization.

VESTAL: Oh, yeah, because that was really seminal. And it's unfortunate the Nobel Committee has…what they've done is, if they didn't want to give it to Franz Hillenkamp, they probably should have given it to Macfarlane [in 2002 when John B. Fenn and Koichi Tanaka won the Nobel Prize in Chemistry]. His work was really the one that laid the foundation for this ability to make non-volatile ions, because when we saw ions, we said, "Well, you know this is somewhat like he said." Even, so we're doing it differently, but what, what's going on?" And then, of course, the Thomson and Iribarne led us in the right direction, I think.

GRAYSON: How do you spell that second name, [Iribarne? I think] Fenn refers to this work all the time.

VESTAL: Yeah. I'm sure he does, because that was quite early, and as I say, it's one of those things you don't read the literature until you have some good reason to, and it's bad. [laughter] But, I went looking for references, and I found that. And what was really annoying is it was right in the same issue, and I think right back-to-back with one of my papers on photodissociation. And I probably at least passed it. […] They have decided a lot of the old literature, [on charged] droplets and so forth. It was a good lead in. Their work was very important too, and probably not [recognized as much as it] should be.

GRAYSON: I noticed in Tom Covey's presentation yesterday…

VESTAL: Yeah, he mentioned them.

GRAYSON: But someone I worked with published one by…I think it was a Thomson paper. It was published in [something] like *J. Chem. Phys.*[13]

VESTAL: Oh, yeah. Some of [it was] probably published in obscure places. There was one paper published in [*Journal of] Chemical Physics*. I know. But I think their first paper was published somewhere like…the one [you] cited may have been the first…

GRAYSON: Yeah. That's kind of weird.

VESTAL: In the [*Journal of*] *Chemical Physics* paper they did a pretty nice job of discussing how this could happen, the ion evaporation model, and so forth.

GRAYSON: So, I sat in on a session a couple of years ago at ASMS, where Bill Budde talked about the fact that, ah, he had been given the opportunity to review Fenn's proposal, grant proposal, and had turned it down because by that time, I think, thermospray was working.

VESTAL: Maybe. I don't know whether Bill may have been the guy that wrote that. I don't know. I've always been appalled by what happened there, because one of the comments when John's first proposal was turned down was, "What do we need this for? We've got thermospray." And I told John, I said, "That's crazy." I certainly, wouldn't have said that, and I'm really appalled that anyone else would say that, to say that we don't need a better technique, when we've got something. That might have been Bill. I don't…

GRAYSON: Well, he more or less came clean at this ASMS Session. And said that he had [turned down Fenn's proposal].

VESTAL: Well, I'm sure a lot of people had that attitude. You know, we've got something that works.

GRAYSON: Yeah. Why, do we need something else […]?

VESTAL: And of course, John is a great guy, and I knew him long before that from his molecular beam work, but I never worked with him. But I was involved in reviewing some of his proposals, which I always gave good scores to, but, you know, John wrote a lousy proposal.

[13] B.A. Thomson and J.V. Iribarne, "Field Induced Ion Evaporation from Liquid Surfaces at Atmospheric Pressure," *Journal of Chemical Physics* 71(11) (1979): 4451-63.

I mean, he had this kind of offhand…he'd write about two pages and tell you [we're] going to do this and this and this. And that was it. If you knew him, and you knew what he could do, you would accept it and say, okay, that's fine. You know he's going to do it, and know he can do it, but for a lot of people, if they didn't know him…

GRAYSON: You didn't know him.

VESTAL: He didn't, he didn't cover all the bases that one could cover in [an NIH] proposal, and I guess because he never really had been in that situation before. I think maybe he learned a little more as he went along, but by then, once he was beyond a certain point, he didn't have to say anything. But it was already done. So, well, that's a great piece of work. [Malcolm] Dole started off in the right direction, but he didn't really understand the stuff about molecular beams that John did, and so he went down some blind alleys.

GRAYSON: That's an interesting background [that a] totally, completely different field [ends] up being applicable to the ionization process, and so it always brings [me back to] this <**T: 135 min**> issue, […] that science and development is a nonlinear process; unfortunately, that's not understood by a lot of people.

VESTAL: Oh, yeah, no…

GRAYSON: Particularly people with money.

VESTAL: Yeah. Well, I think people tend to think of science and scientists as being objective and all gray…you know, not bang, bang, bang in sequence and…

GRAYSON: Well, I think it's taught that way.

VESTAL: It's from that, it's probably true, but ah, you know it is today probably even harder to get a new idea funded. Anything. If you're, you know, going down a certain track and it's an improvement over well-established things, then people understand it. It works pretty well. If you go off over here somewhere it is damn hard, because the first reaction's going to be, it won't work. I've just been through two of those and actually took a couple of tries, but we got there.

GRAYSON: That's the problem, is having ideas that are a little bit outside of the mainstream. So you've actually, then you abandoned academia.

VESTAL: Yeah.

GRAYSON: Pretty much completely by 1987, according to your CV.

VESTAL: Yeah, that's about right. I was working with the company before that, but I went full-time by the time.

GRAYSON: So, you formed Vestec in 1983.

VESTAL: Yeah. We actually…I think, we incorporated in 1982, but we actually started, we showed our first thermospray interface at the Boston meeting in 1983.

GRAYSON: So, who is "we"?

VESTAL: Well, initially, Chris [Christina Vestal], and I, and Gordon Ferguson at that point. [laughter]

GRAYSON: Chris…

VESTAL: My wife…

GRAYSON: Oh, okay.

VESTAL: She just got her Ph.D. about that time, and we…Gordon did the electronics, and I did the rest of it. And we, I think we put it together on our kitchen table. [laughter]

GRAYSON: Oh, wow.

VESTAL: It was a little box with a gray panel. We had a mimeograph flyer that we handed out, and we put out […] around the tables ASMS. That was before they had the booths in there, put you could put out the literature.

GRAYSON: Yeah. Well, they had kind of stringent rules about what could be done. I think because they didn't want to become an instrument show.

VESTAL: Yeah. I'm sure financially it's good they did what they've done, but [from a] scientific point of view, I think it's terrible. It was a much better meeting before, but that's the way it goes.

GRAYSON: So, this was a fairly successful venture.

VESTAL: Well, yeah. We did pretty well. We, you know, we sold quite a few OEM […] Shimadzu for example […] they gave us an order for fifty units at one point. Wow, you know this is...this is great. We did quite a few of them. And they had a quadrupole which they tried to sell and were unsuccessful in the United States because the data system was not up to speed. But, you know, they dominated the market in Japan.

GRAYSON: Oh, yeah.

VESTAL: So, they've done a lot on the premise and sold them to all their customers and family. I guess they use them. I don't know. And I spent a few times in Kyoto [Japan] working with them, which I always, enjoyed Kyoto. So, we built them for that. We built some for, for HP [HewlettPackard], but they took the license and built their own rather quickly. So, did Finnigan. We sold to them. We made a deal with HP, where we bought the guts to the MSD from them and we built it into a cabinet with a thermospray source and sold it as a stand-alone instrument. We sold a few of those. I don't remember how many. Ah, so we [had not only] the interface, [but] we did have [our own] system with the HP and mass spectrometer embedded in it, but strictly, LCMS.

Ah, as I mentioned in the talk that was going along pretty well until about 1990, but in 1988 I realized the handwriting was on the wall, because once I heard Fenn's paper on the high molecular weight electrospray [at the 1988 ASMS Conference] and the MALDI paper by Karas and Hillenkamp [at the 11th International Mass Spectrometry Conference in Bordeaux, France in 1988], and also, shortly thereafter, the work by [R. C.] Beavis and [B. T.] Chait on MALDI/TOF. I decided you know that, the days of thermospray were numbered, but we needed to move into this new technology. So, we did some work on both electrospray and MALDI, but it was pretty clear after we put electrospray source on our dedicated system that we'd done thermospray on. But it was pretty clear to me right then…you know, everybody was jumping on electrospray and been coming… there wasn't much incentive for us to try to compete on that level.

But nobody was doing that much with MALDI. So, we decided to <**T: 140 min**> build MALDI systems […]. It started really in 1989 and worked initially with…our first commercial instrument really was patterned and we actually licensed and we signed a license with Rockefeller. And our first instrument was really, basically, a modified version of what Beavis and Chait had done at Rockefeller. This big blue bomb that we sold. […] That was the first one, first commercial one was for Biemann at MIT. Second one went to Proctor & Gamble, to [Martin] Marty Lacey and Tom Keogh. And, I think, yeah I think the third one went to a guy in California at Johnson & Johnson. He was a protein chemist, but didn't know anything about mass spectrometry. And the fourth one, I was reminded yesterday, went to Lloyd Smith, which I had really forgotten about. He reminded me yesterday, he got he fourth instrument that we had done. And [T. William] Bill Hutchens] had one at Baylor and [Richard M.] Caprioli had one at the University of Texas Medical Center.

We sold a few of those. It was a big monster. Randy Nelson worked with me on that, and he was our sort of "do everything" guy. He did the installations and service and training and held the customer's hand. He was a maestro at making the thing work because you had to sort of fiddle the probe and the laser and get the bit spots and produce ions. And you know, it produced excellent spectra, but, you know, it was…as I was saying it was not so awful user-friendly.

GRAYSON: What was the mass analyzer on that with a MSB?

VESTAL: No. No, this was a time-of-flight.

GRAYSON: Oh, time-of-flight.

VESTAL: This is linear time-of-flight with 30 kilovolts. And the one thing we did, which is kind of interesting because we used the electron multiplier that I had built earlier at Johnston Labs, which Becton Dickinson...we're building as a product at that time. In fact, the guy who was doing it was the guy that I trained back twenty years earlier.

GRAYSON: So, what so special about this electron multiplier?

VESTAL: […It was] what we called a mesh multiplier. Actually, Hamamatsu builds a version of it now, but instead of box and grid […], it had […] device [a stack of photo-etched plates] with custom holes, so that each stage was […] about half a millimeter, so you only had about a half millimeter uncertainty where the front was and it had a large area front surface. They mainly use them for space projects, and various things up there at NASA.

GRAYSON: It sounds like it might have been more compact than [anything else on the market].

VESTAL: Yeah. It was compact and also very rugged. The reason they sold them to NASA was they showed [you] could shake them and [they would] withhold the vibration. But they were still making them, and for this purpose there was no reason to replace with something else. It was largely replaced with by channel [plates], but it had the advantage [that] it didn't saturate.

GRAYSON: Yeah.

VESTAL: It didn't have the resolution. No, I think it's gone now […]. Some of these things have a way of coming back after all, because for the purpose [of what we were doing then] it really was the best one.

GRAYSON: So, this is your own design for the linear TOF.

VESTAL: Well, it was [based very] much on the design that Beavis and Chait had. We made a few minor changes, but it was starting to move on. Later we changed a number of things, but the first one, we just pretty much…you know, it was even using this old LeCroy digitizer which I think we got rid of that pretty quickly […]. I know we were using a big…I think that [we were] using the [YAG laser].

GRAYSON: What kind of resolving power did this thing have?

VESTAL: Well, time-of-flight, […] you know. Anyway, you look at anywhere from a thousand to a hundred, and the resulting power was always the same as you get today from [a linear instrument]—that hadn't changed.

GRAYSON: Yeah, yeah.

VESTAL: It's pretty…

GRAYSON: Yeah, yeah.

VESTAL: In fact, the sensitivity at high mass was better than most instruments that have been built since then. Something, ah, I think that Brian, particularly, had paid good attention to was the design, optimizing the performance for high mass…

GRAYSON: Now, did they [patent] that […] or did they give you the design?

VESTAL: They applied for a patent. We licensed it, although I'm not sure the patent…the patent they got [amounted] to much in the end.[14] The claims <**T: 145 min**> they got were something that really weren't necessary. They also had a patent, which actually I wrote for them, and did the work on using it for DNA sequences. And that's why Lloyd got one.

And there was that patent, and of course, Sequenom got major patents on some of that, but we…or they had the first patent.[15] We licensed that as well, although we paid them for a while, and then AB quit paying them on it, because we really we weren't doing that, and we weren't really using their design. And I think they were a little unhappy for a little while about that. Actually, we paid them quite a bit of money for that, and you know it really gave us a leg up to get started. And I had worked on time-of-flight back you know as I say, back in coincidence time-of-flight. But in the intervening time, I hadn't done anything with time-of-flight. So, it took me a little while to get up to speed again on time-of-flight. That's what I've been doing the last twenty years, and I think I'm now up to speed.

GRAYSON: So, this was in response to the fact that you saw that the thermospray method was not really going to [be competitive much longer]. I mean, it had its moment of fame and now it was [going to be passed up].

VESTAL: No. There wasn't much we could do with it to make it better, you know.

GRAYSON: Better…

VESTAL: It did what it did. And people were using it, and they continued to use it for quite a long time, but…

GRAYSON: The electrospray was that…

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[14] R.C. Beavis and B.T. Chait, "Instrument and Method for the Laser Desorption of Ions in Mass Spectrometry," U.S. Patent 5,045,694, issued 3 September 1991.

[15] R.C. Beavis and B. T. Chait, "Instrument and Method for the Sequencing of Genome," U.S. Patent 5,288,644, issued 22 February 1994.

VESTAL: …the electrospray was doing much better.

GRAYSON: Plus, being able to do problems that thermospray couldn't do.

VESTAL: Oh, yeah, sure. No question that it was a big improvement. And of course, the beauty of it is, it was even easier than thermospray to mate with an existing instrument. The main reason electrospray took off as fast as it did, as you said the fact that those are the things you couldn't do in a while, but it was very easy to just bolt onto an existing quadrupole mass spectrometer. And all these companies had quadrupole mass spectrometers. It was not so easy to hold onto magnetic instruments. We had some…we did one or two [magnetic instruments] ourselves, but it was a real pain in the ass.

GRAYSON: Well, I know when I was working at Wash U. [Washington University in St. Louis], they had an electrospray source on their ZAB machine and it was a pain in the butt to be sure. It just […] wasn't a good fit.

VESTAL: No. They were virtually incompatible at first. The other side of that is though, the reason the MALDI didn't take off faster, I think is it really *only* was a good fit in time-of-flight, you can live with other things, but you sacrifice your main…you lost the advantage of doing it, if you went to a continuous scanning machine. And at that point in time, time-of-flight had languished forever. Bendix had done their thing. There was a little bit of resurgence. And one reason, Bruker got in as quickly as they did […] they had built some time-of-flight for doing this MALDI photon ionization stuff. And so, they had a pretty […] good time-of-flight instrument for that purpose. So, they fairly quickly adapted their instrument to do MALDI, and also, built linear MALDI. Finnigan of course, they did something really…we were the first ones out with this instrument—this monster—that we built. We progressed from the early, the original. But Finnigan came in with the first one in with a commercialized one with this little box that they sold. And I know how that came about, and they were trying to solve [it as if it weren't a] mass spectrometer, it was just the kind of thing you put…

GRAYSON: The black box…

VESTAL: You put samples in, you get…of course the performance was pretty poor compared to what we were getting. Their attitude was well, performance doesn't really matter. They don't know any better. [laughter] But that's not a good…that doesn't work very well. But, I talked to Mike Story later, about how they happened to get in as quickly as they did, and it's because…I'm sorry. Mike Story and Caprioli were both involved in this to some degree, and Caprioli may be the one that told me about it. They had started off to do a californium machine.

GRAYSON: Oh, really. Wow.

VESTAL: They had this group set up in England that was going to build a small bench top californium instrument and they built this time-of-flight. And then part way through the project, that's when MALDI came out. And Caprioli and other people said, you're kidding yourself. That isn't going to fly at this point. It was dead. You need to switch over to MALDI. So, that's what they did. They switched to MALDI […]. They say the performance was pretty poor. And they did build it. In Bremen, they built a reflector machine which was much better, [and] they did <**T: 150 min**> sell some of those.

GRAYSON: So, the black box, the instrument actually had a time-of-flight that Finnigan was building at the time?

VESTAL: Yeah. It was earlier time-of-flight, very simple device. I don't know whether any of them are still in existence or not. We used to have one [at AB] that they had bought before I got there. We used it as a coffee table.

GRAYSON: So, that's interesting, [these] side diversions that come about from time-to-time. So, Vestec […]…it looks like went out of existence in 1993?

VESTAL: Yeah. Well, basically, by 1993 […] thermospray was dead. We'd pretty much converted to doing MALDI, with a little bit of electrospray, but primarily doing MALDI. And so we were up to sales of, I don't know, between four and five million, I guess, and we were profitable. We weren't rolling in money, but we were surviving. But it was clear that MALDI had great potential and it's going to be [the wave of the future]. Finnigan and Bruker both had started to come in with decent instruments.

[…] We were still selling more instruments than anybody else, but if we were going to continue, we needed to [develop] more muscle in terms of sales and marketing and money so we started looking around for people to work with. And of course, we'd had a relationship with Hewlett-Packard for quite a while. They had put some money in. We had collaborated on this using their MSD so I had a good relationship with a lot of people there.

Anyway, there was a small company it was […] at OI Analytical up in […] College Station [Texas] and the guy had just taken it over. [He] was one of these kind of guys who's a real entrepreneur. I guess […] he was one of these companies that was public because it had been taken public very early and had gone more or less broke, and then had been resurrected as a new company but with public stock.

GRAYSON: This is OI Analytical.

VESTAL: Yeah. They're right next to Agilent in the show [Pittcon 2010] in there, and they survived. This guy, Will [William W.] Botts, who was the president, the person who thought maybe, they'd like to buy us and, you know, they could give us stock, they could exchange our stock for their stock which could be sold on the market, et cetera. So, at that time, I [thought] well, it might be a good fit. So, we went through that exercise. And after we were done I realized that, where he wanted to go and where I wanted to go were so different. And my evaluation and his evaluation…he was one of these people that wanted to go through everything, and due diligence, to turn over every rock, and find every reason to reduce the price that he was going to pay. After a couple of weeks of that, I said, we ought to think and maybe forget it.

So then I got serious […], I really should go look and see what the possibilities are and do something with somebody somewhere. I talked to my friends at HP. Larry Cattran was in charge of the scientific instrument division, I think, at that point. He was our sort of unofficial board member, because they'd put some money into us, and they owned a piece of us, but they didn't want [to officially be] a board member. But he came to our board meetings as if he were a board member. And he was very helpful, I liked him a lot. And Karl Schwartz, who was his boss, at least at some point, was also somebody I interacted with for years. And they both were very enthusiastic about us joining HP. And so they went off with a proposal to their management, et cetera, to see what they could do. And that was grinding through their system somewhere, and they said…they were encouraging whenever I talked to them.

But I went to the Protein Society meeting in San Diego [California], which I guess would have been…oh, 1992, I reckon. We'd be gone by 1993. And we had our new little benchtop instrument at that point that, it was the first of the Voyager Series—what became the Voyager Series. It looked exactly like […] most of the ones we did later… small, vertical instrument. And we showed that there for the first time. […] Well, it was working, but it was far from being really complete in some ways. But we had [done] all the basic work.

And a couple of booths over was PerSeptive Biosystems, and Fred [E.] Regnier, one of the founders of PerSeptive, along with Noubar Afeyan. And I'd known Fred for many years. And Fred and I were talking and I said you know I was thinking about, it was time we really <**T: 155 min**> [need to] do something to get better sales and marketing and get a more competitive organization. We were sort of looking at the possibilities. "Come on, I want you to meet Noubar." And he took me over to meet Noubar, and he was this young energetic [fellow]. At that time he was huge. He's lost a lot of weight since then. Anyway, they were very enthusiastic and very interested, so I went up and visited them. We ended up basically making a deal. And of course, their stock price is going up like crazy, because they'd gone from nothing to eighteen million [dollars] in a short time, and we were four or five million [dollars]. So, […] we agreed on a price and […] general terms. We didn't sign anything.

I got back and the day after I got back, I got a call from HP. [laughter] They had got their deal through the board, and they wanted to buy us. They were offering basically the same

price as what PerSeptive had bought. And when I told them, I said almost made a deal with PerSeptive. I agreed to a deal. They came down too—a couple of their business people […] they were very sharp people. But they started laying extra money on the table for me, personally, if I took the HP deal. From a strictly business point of view, and as far as all my investors were concerned, I should have taken the HP deal because it assured you're going to get what you think you're going to get, and HP was this huge, well-established company versus this little fly-by-night company.

And the sticking point for me was really—I mean, I suppose I might have done it anyhow—but the one difference was that I could see, you know, PerSeptive had no capabilities for manufacturing in this area and no real expertise in the whole area, so they were dependent on us. And they intended to keep the facility in Houston, at least for the foreseeable future. And to the contrary, HP was quite clear that they reckoned within six months they could move us They could crank up in Palo Alto [California], and they wanted me and a few key people to move to Palo Alto, and they would give other people severance and they would close [Houston] down. And that kind of bothered me, and I really was turned on because Noubar's a great salesman. I mean, he really is…

GRAYSON: How did you spell that name?

VESTAL: […] He's of Lebanese origin by way of Canada. He got his Ph.D., I think in Canada University] and postdoced at MIT [Massachusetts Institute of Technology] with [Daniel I. C] Danny Wang. He was fairly young, thirty-two, I think, a real entrepreneur, and the thing he could do was raise money. They had already brought in a lot of money, and he […] brought in a lot more for research. In many ways, I made the right decision. From a financial point of view, it was almost a disaster, but it turned out okay in the end. When we made the deal their stock was selling at something like eighteen, and we valued our stock at five or something like that, I don't know, which is okay.

And their stock ran up to…I don't know…went up to twenty-five by the time we closed the deal, went up to thirty-something and then the stuff hit the fan when it turned out that a lot of their sales were bogus. They were showing sales of things that the customers hadn't ordered and hadn't necessarily been delivered. In order to keep this growth going, they were doing all sorts of recognition in sales that, you know, really weren't valid. Of course, there were lawsuits and the stock went from thirty to three over a very short period of time. And I was sort of locked in. I mean, people approached me early on, and I could've borrowed against the stock and I could have done things to get money if I wanted to. But I didn't need money. I thought the company looked good to go. You know, by the time I realized, it was too late. So, but it came back eventually…I almost bailed out of that one. At this point, I got nothing much more to lose. I might as well see how it'll end. Noubar was good and raised a lot of money to do R&D. So, we got a great R&D [operation], able to bring in a lot of really good people, Steve Martin, and a whole bunch of people. We had <**T: 160 min**> a great research organization. It worked out in the end. And we became the dominant MALDI company for quite a few years.

GRAYSON: So, PerSeptive…I see four years' period with PerSeptive.

VESTAL: Yeah. Then they merged with Applied Biosystems or PerkinElmer at the time. Sold off PerkinElmer, and kept Applied Biosystems. Very complicated.

GRAYSON: So, you get into this…I don't know what you want to call it, eat them up corporation thing…that goes on for, Lord knows how long. It seems amazing that this actually happens.

VESTAL: Yeah. Well, PerSeptive was acquired eventually by PerkinElmer. PerkinElmer had earlier acquired Applied Biosystems. And then, Tony [L.] White, who was president of PerkinElmer—and I think correctly decided—well the PerkinElmer business—the old line PerkinElmer business—was flat. It was making all these things that people do, but there wasn't a lot of growth potential. So, he sold off the old line PerkinElmer business to E. G. & G., along with the right to use the name […] I think he thought they weren't going to use the name. But they very quickly dropped the E. G. & G. and became PerkinElmer.

GRAYSON: Oh, why not?

VESTAL: They kept on building stuff. He had a good business. It just wasn't big enough. So, then they became Applied Biosystems, and we're part of Applied Biosystems. And the first [few years there] went pretty well. But I don't think I fit too well in a large corporation. There were lots of management issues. But the MALDI business was what fueled it, because PerSeptive was building some of the chromatography. But none of that had done as well as they had projected early on. And the MALDI business really took off with the research support we had there in the group, it went up…I don't know what the sales got to, but it got to a point where MALDI was at least half of the business, I think. Applied Biosystems bought us, and we were the major supplier. We were beating both Bruker, and Finnigan, and Waters. So, that was, I think, the main driving force. They were interested in the Poros business I think, but the main driver was the mass spectrometer because they thought that it'd fit in with their…[sort of] the proteomics side of genomics.

GRAYSON: So, somewhere along the line you got physically moved from Houston to the east coast.

VESTAL: Yeah. I guess, about a year. We moved up there in 1990…I guess 1995, spring of 1995.

GRAYSON: Where you're still are.

VESTAL: Yeah, same house. But the factory stayed in Houston for a long time. It was closed down very recently.

GRAYSON: So, how many people were employed in the factory?

VESTAL: We were up to thirty-five or forty people at one point.

GRAYSON: And this was primarily the construction of time-of-flight equipment?

VESTAL: Well, yeah. The whole company, by that time it was mostly MALDI. It was a lot of the same people had been hired from day one, and getting on. And then, I brought in a good manager who really was managing that extremely well. And you know that there's no reason…they wanted to move all the R&D up to Framingham [Massachusetts], which made sense, because I was working with them anyhow. I tried to get Calvin to move as well, and there were several people that would have been happy to move to Framingham, but I couldn't get the other guys to move. Electronics engineer, and Calvin, and a lot of people that if they wanted to stay with the company, needed to move to Framingham. But they decided not to. But the whole engineering production group stayed intact and took on doing their thing, and doing it very well. Like Bill Gibbs, who was the production manager I hired. He was an engineer who had taken an MBA course, but had never done management before I hired him to be our production manager. He ended up being the manager of the whole thing, and I think now he's…most recently he was manager of [all of] the production/operation side of AB.

GRAYSON: Oh, wow.

VESTAL: And, I guess, he's still there. I don't know what's happened with him. […]

GRAYSON: So, you stayed with PerSeptive and then Applied Biosystems, for up through…looks like 2001.

VESTAL: Well, actually, I stayed till 2004.

GRAYSON: Okay. Oh, yeah, 2004 <**T: 165 min**>. So, there's a kind of…

VESTAL: 2001, I think is when I…I was the first and last […] PerSeptive Scientific Fellow. That was just at the time of the transition between PerSeptive, and AB. Fred and Noubar had come up with this idea of each year honoring one of the scientists as scientific fellow of the company. They got a plaque up with my name on it, and there's a lot of blank spots after it, because after AB bought it, they didn't want to do that anymore. So, I was the first and last. [laughter]

GRAYSON: One and only, first and last.

VESTAL: First and last Scientist Fellow. But I stayed on in various capacities with AB after that, and for quite a while I more or less ran the Framingham R&D operation along with Steve Martin's help. Steve was doing more of the application side, but I, kind of, moved out of that a little bit and pushed him into taking more of it, because I didn't like doing all the travel in California. He didn't mind doing it. And he's a very capable guy. But he and I together really ran the R&D MALDI group pretty well.

GRAYSON: How big an operation was that?

VESTAL: Oh, it was too big. I think at one point we had about a hundred people. And that was part of the problem. It clearly got to the point it was bigger than could be supported by the business, even though the business was going pretty well.

GRAYSON: And these people were primarily all applications chemists?

VESTAL: Well, a lot of times software engineers, applications, chemists, all…I mean, Steve had this group that was…what did they call them? It was really a group really focused on the application and doing things with the…

GRAYSON: Well sure, yeah.

VESTAL: And they had some […] contracts, but mostly it was just, you know, doing real research with the instruments and they were a very good group of people…proteomics something laboratory. Anyway, […] once you get a big company and like I said, they started fiddling with management…and I had told them. I said, "We don't need marketing to tell us what to do. We'll tell marketing what to do." And, we got away with that for a while, but eventually that changed, and it became a point where you spend all your time in meetings and…

GRAYSON: Yes.

VESTAL: …and bureaucracy. I don't know that I would have left except that it reached…well, things started downhill when they […] had this problem with competition between the Sciex group and the MALDI group. Couldn't quite get them to work together the way they would like. And I understand that. And we were spending rather more on R&D than we should have been for the amount of business—most of it had been directed, but anyway. They decided they had to merge these groups, so they brought them together under Laura Laumann, which meant that Jay Smith who had been managing the marketing and sales side of it was fairly quickly out the door. So, which one of them was going to take over? They made the wrong decision, in my view. Anyway, from then on things started going downhill […]; led up to eventually, to them selling the MALDI business to Sciex.

GRAYSON: Oh, okay.

VESTAL: That was the end of 2004. This was done, well, rather suddenly and, in my view, surreptitiously. I was still very much involved and I managed […]. I didn't have direct management responsibility, but I was doing the new stuff, and I had moved into the office that had been vacated by Noubar, so I had this nice corner office. I had just sort of taken it. [laughter] You know, in fact, Laura never really quite approved of it, but she couldn't quite disapprove of it. She and I didn't get along real well. I tried.

GRAYSON: Laura?

VESTAL: Laura Laumann. She's still the manager…

GRAYSON: How do you spell the last name?

VESTAL: L-a-u-m-a-n-n, I think. She and Andy [Andrew] Boorn are the co-presidents of this…

GRAYSON: Andy…

VESTAL: Boorn. They were co-presidents of AB Sciex under Danaher now. But anyway, I got the word the Friday night before…I was leaving the next day for the Siena [Italy] meeting, that they'd done this…

GRAYSON: Oh, wow.

VESTAL: …and nothing could be done.

GRAYSON: How's it going over?

VESTAL: And this <**T: 170 min**> list of people that were being laid off immediately, this list of people were being offered a chance to transfer to Toronto [Canada], and the other people were going to be phased out or something in time. And up to then, I was kept completely out of it. A lot of discussions about the people and so forth, and some of the people that worked with me were involved in that, but they were instructed directly not to discuss it with me. I wasn't supposed to know about it, and I didn't, so Laura called me on Friday night before I was leaving and told me about it. I went and talked to one of the people who had been involved in making the decision, and found out a little bit more about it, and I said, "This is not going to work; if you really just want to kill it, you know you're going to succeed. I understand you need to cut back on the excesses, but," I said, "unless you keep this many people, there's nothing I can do that's going to be useful. So, if you're not going to keep this many people, then I'm not going to go to Toronto and neither are they. Then, I might as well leave." And they said, well, okay. So, I went to Siena […] some of the people who attended the Siena meeting were notified, because they had already left, were notified at the Siena meeting they no longer had a job.

GRAYSON: Oh, great.

VESTAL: But, I will say one thing. They did treat people well in the financial sense. Everybody who was laid off got a very good severance package, et cetera. They treated them well. But the way it was handled internally. I think it was very poorly done. As a result, one person out of the whole group went to Toronto, I think. They had thirty or forty people on the list that wanted to go to Toronto, and only one went. And, I think, well, the one is no longer there now. He's back in the sanctuary of the mother country. So I left at the end. It was November, 2004, when I got back from Siena and formally resigned.

GRAYSON: So, then you're seventy years old. So, you retire.

VESTAL: I retired. I didn't resign. I retired.

GRAYSON: Okay.

VESTAL: But same difference anyway. You know, I got my stock options. I exercised my options. But I was in good shape. I was a little annoyed, because I had all the stuff I still had to do.

GRAYSON: Oh, sure.

VESTAL: We were doing it. I had the group of people. But since then, I could have done…I could actually have done more quickly there, except that I couldn't have because I'd have had to fight this battle, this continuing battle to actually do it. And you spend 90 percent of your time, justifying doing what you need to do. So, it's probably the best thing that ever happened to me in my career was to leave at that point, because I would not have probably…short of something like that, I probably wouldn't have left out and kept on plugging away. Because I was doing what I wanted to do. It was just that it was getting more and more difficult, but I didn't realize how much stress I felt from it, though, until I got away from it. We built this house up in Nova Scotia [Canada] and we went up there for a month afterwards and hung around a little and said, "Well, this is isn't so bad."

GRAYSON: It isn't all bad.

VESTAL: Well, I stayed on as a consultant for a few months. I tried to help finish up some work for the next ASMS and I went up to Toronto and consulted a little bit, but it was pretty clear that […] the people in Toronto were not really interested in MALDI. They knew about it, and that was okay. That wasn't their thing. They wanted me to work on electrospray or something, and mostly go to meetings and be a figure at the meetings and do whatever. And I said, no. That's not the way I want to spend my time. So, I stayed till the ASMS as a consultant and […] at the ASMS and said I'm going to cut it off here, and there's no point in…I'm not going to do you any…you're going to be paying me. They paid me pretty well, I can't complain about that. But, anyhow, I just don't want to spend my time doing this. And you might as well save your money. That's when we started thinking about starting a new company. […] I formally left the consulting middle of 2005. And by the fall of 2005, we formed a company [Virgin Instruments] and that was basically, we started operations the first of 2006.

GRAYSON: And this is?

VESTAL: Hired people. Because that's when…we picked up a couple people that had already been laid off at that point. And then, the other guys…the ones that wanted to go to Toronto that didn't <**T: 175 min**>, they were sort of being phased out over that next period, next six months. And all of them as they phased…

GRAYSON: So, now you have a company called Virgin Instruments. And that's currently how many employees?

VESTAL: I think we're at thirteen, right now.

GRAYSON: Okay. Now, what are they doing? What's Virgin Instruments do?

VESTAL: The same thing we've always done. [laughter] Doing MALDI.

GRAYSON: MALDI, time-of-flight.

VESTAL: Yeah. It's a wonderful thing, the MALDI.

GRAYSON: So, you're actually building and selling instruments.

VESTAL: No, we're [not] selling it. Not yet. We financed this two ways. We had a number of—still do have a number of—SBIR [Small Business Innovation Research] grants from NIH. We funded it with our own money. So, Chris and I are the sole stockholders. Our employees all have options and we put in a significant amount of money, but you've got to do something with it. We're not destitute yet, but I don't think we're going to put a lot more money into it. I think Chris is getting a little bit conservative about wanting to put more money in. [laughter] We could do it if we had to. So, we're looking at making a deal with somebody that can do the sales and marketing and maybe even manufacturing, but we're going to continue doing the research. You know, we really have […] done some things that I wanted to do and just couldn't get traction to do. […] Because the philosophy was just different. [Product development] philosophy was you get an idea for a product. You get marketing to agree it has a market, and of course, marketing can never see except in the rearview mirror. You don't see anything up front. We've got to go through all this stuff to delay that. And so, they were not going to do what I wanted to do, […]. Once I did this, I can do whatever I want. And particularly, to the extent we're willing to pay for it, there's no restrictions. Of course, I started writing grant applications almost immediately. I wrote two grant applications before we even formally got into operations, late 2005. One of those didn't even get a priority score, which means it's not even good enough to be discussed. And the other one got a horrendously high priority score,

which means no chance of getting funded. And I was a little naïve on it. […] It was about this new thing they have called a "fast track" when you…

GRAYSON: Yes.

VESTAL: [Ask for] both, Phase 1 and Phase 2. And those are harder to get, considering that, you know, while I personally had a track record, the company had no track record. So, I'm going to assume that. So, I put these back in as both as Phase 1s, and we got funded the next round as Phase 1. And we now got funded on Phase 2, both of those. One of those we're in the second year of Phase 2 now, and we've written some others since then. But what I did was…okay, this is what we need to do to provide what the biologists really need in the way of analyzing proteins and other biological molecules. And I think MALDI offers something you can't do by electrospray or any other way. This is what we need to do.

GRAYSON: You're kind of in the same camp as Jack [D.] Henion, I think; didn't he show this business-related kind of, this MALDI…he's got this Advion thing, where they do the…

VESTAL: Well, they don't do MALDI. He does electrospray.

GRAYSON: Yeah. So, there's a…somebody was showing me in that pad a set of papers; there's a symposium where you end up…

VESTAL: Doing MALDI/LC/MS. That's me. Yeah, that's what we're doing. We have several different [projects], some farther out than others, but the basic things that we're going to commercialize fairly soon is high resolution MALDI MS, interfaced with LC and with other techniques. The main thing [that I did] over the first couple of years is really develop the theory to a point where I can really optimize the design of any MALDI/TOF instrument. You say, okay, I want to get this performance. What do I have to do to get that? Or, I want this size, what's the limitation on that, if I want to keep it this small? I've published some of these. But, I now can quite confidently pick the result, I think. And we've shown that a little bit. We've got up to sixty thousand resolving power on the benchtop instrument.

GRAYSON: Wow **<T: 180 min>**.

VESTAL: And we're right now building this, which I think is fun…whether it has any commercial potential or not. I'm really not sure. I listened to Alan [G.] Marshall talk on it. I think maybe we can do it, and […] I think it's worth doing even though it may not be a

commercial [instrument]. To really push whatever limits on resolving power and accuracy you can get. But right now, we're building a two-story complex…

GRAYSON: Two-story.

VESTAL: It's about six meters, twenty feet long, and the source and the temperature are on the first floor and the mirror is on the second floor. We're cutting a very small hole [in the floor] and we only need it…it's only a four-inch pipe that goes up through, so it's not very big. And that should get two hundred fifty thousand or close.

GRAYSON: Wow.

VESTAL: I'm pretty confident we'll do that, because the front end, we've got to pull it along. That's where all the action is. The rest of it slides up and back. And I've done the calculations, and I'm quite sure, if I'm able to do it and we've shown that this [works]. I think I know how to build one. I'm familiar with it, but it needs to be a hundred feet long. And it turns out there [our] upstairs lab is almost exactly a hundred feet long. So, this will have to be horizontal, but that's okay. So, we may build a one million resolution machine.

Now, the other side of that is, we can build really quite a compact benchtop machine that has resolution better than we could do in the past. You know, we built for a while, what we called the XL, which was fifteen feet long. It was two consoles—two of the normal ones end-to-end. We sold ten of them, so we were very pleased at that. We can get ten thousand resolution out of it. Now, I get ten thousand resolution in a box this size. [laughter] So, we have come a ways on these. But that's useful and interesting, but I think the really, the bigger complication is with MSMS, because what you really want to do is LCMSMS. And if we can compete with the electrospray with that, I think we can, quite practically. That opens up a huge possibility. And of course, the TOF-TOFs that I did before with AB that have been fairly successful…forty-seven hundred, forty-eight hundred, [numbers for AB instruments]and I guess it's what, fifty-eight hundred now or something like that, I think they call it. The last two are basically the same, except for some minor details. They have not done much in the way of new development on that.

But we've got a number…quite a bit of IP on…first of all, the major problem with the [earlier TOF-TOF] is the precursor selection is really rather poor. You can only get resolution of a few hundred. And you have ugly shapes. And you get some contribution from adjacent peaks. It's not clean. So, we figured out how to get the resolution of at least four thousand. That's working. And the other thing that's probably more important is I figured out to multiplex it, so that each laser shot you can select multiple precursor ions and get fragment spectra, all of them at the same time. So, you accumulate a TOF spectrum. You're not throwing all those extra ions away. The advantage of time-of-flight in the normal sense is you get all the ions all

the time. In a pulse source. But when you do MSMS you select only one of them, [then you] throw the rest away.

GRAYSON: Exactly.

VESTAL: So, now we can select essentially all of them of any significance and do MSMS on all at the same time. So, we don't throw the ions away. You look skeptical.

GRAYSON: Well, you got a busy piece of equipment somewhere doing a lot of work.

VESTAL: Well, it's doing the same thing. It's just recording the time-of-flight spectra. Now, you have to put it in the computer, and extract [from that] the fragmentation spectra. It's not as hard as it sounds. And, [there's a couple of] other things we can do with it. So, those two instruments in some form or other, probably the MS is not going to be the twenty-foot one, but one that fits in a bench. But it'll be at least fifty thousand resolution, and this TOF/TOF, the first two products sitting around, going to do. And we're talking with at least one company seriously about actually working with them to make this happen. And I think that's where you want to do it. I don't want to go out and raise venture capital and…

GRAYSON: Yeah. Well, this is an area that obviously has a lot of interest, and if you can show that your solution is better than, someone else's and you demonstrate it with equipment […].

VESTAL: Right, sure. No, you got to do that. We've got a lot of the basic stuff done. We've got a lot of other a little bit further out things that I mentioned earlier, [that] got funded after the first passes. You get to know all these people around, you know people who often say, "Well, why don't you do this?" <**T: 185 min**> […I guess] it's a couple years ago, I was out […] at a symposium of some kind in California and Dave [David A.] Herold was sitting next to me. He and I were graduate students together at Utah. He's an M.D./Ph.D. He's in the clinical laboratory kind of thing…mass spectrometer. He said I've been doing this work on calcium-41, and it was really important for some of the bone studies that we're doing at the VA [Veterans Administration] and using accelerated mass spectrometer, because [they] need to get down to 10^{-12}. He said maybe we could do that [with] time-of-flight. I don't think so. And I got to thinking about it. By the time I got home, I said well, we could do that. So, with his help in terms of supporting…actually Darren [J.] Hillegonds is another guy that's been doing it. He's at Lawrence Livermore [National Laboratory], and does accelerator mass spectrometry on carbon and particularly on the calcium. He was Dave's collaborator on that. So, they both agreed, to participate in this project to give it a little credence. When I wrote up the proposal […] it was the first time it didn't even get a priority score on it. But then [some of the stuff they raised was

in the purview of] experts. And believe me, we wrote the proposal, [resubmitted it], got a good priority score, got it funded. We got the instrument built […now].

GRAYSON: So, basically you have like a research instrument shop, is what you've got with…that's Virgin Instruments.

VESTAL: Well, it's more or less right now, yeah. Whether we'll do the manufacturing or not still remains to be determined, but we may. We'll find that out, but you know works out. Yeah. We're doing what I like to do.

GRAYSON: So, you think you have a chance at this calcium problem then, that the guy…

VESTAL: Yeah. Well, I need him to do both, carbon-14 calcium-41. But, need to get down to…the carbon-14, they can get down to10^{-15}, which is like three orders of magnitude below the natural level, and you need that for carbon dating…

GRAYSON: Right.

VESTAL: …and I don't know that we really [will even] mess with that, even if we could. But there's a whole lot of micro tracers studies [that] people are starting to do with carbon-14 work. You're working above the natural level and I'm pretty sure we can do that. Same with the calcium-41, [no one knows] what the natural levels of that is…I don't think it's been measured. There isn't any to a first approximation. There must be some, but you know it's way, way low.

GRAYSON: So, your primary problem is to up the sensitivity to…

VESTAL: Well, yeah. Up the sensitivity, because calcium-40 is next to calcium-41, […] but basically, then you've got one mass unit difference and you say, "Gee, I want 10^{-12} [abundance sensitivity]. How would you do that?" It involves ion gates and multiple detectors and so forth, and you can't do it all in one detector that's for sure. But we have a system that has all the capability, the electronics and everything. We just got it fully operational, just…well, just the days before I left. Fact, there was one problem with it, one Friday that something wasn't working right. But, I think probably while I have been gone, it has started. We'd been able to do calcium 46 which is…it's forty parts per million or so, and so always, […] it's not something you do with most types of isotope ratio machines. And that was just using a single detector and not using all the stuff that we have, because you know we have high resolution to start with, which helps. So calcium-41 is up here, calcium-40 is over here. As long as they don't go to the same place, there's not much…the scattering is not a problem. I thought it might be more of a

problem, but we put in baffles to keep from scattering. That's not a big issue […]. The real issue is being able to make enough ions [to be able] to see it.

I think it ought to do that, but that's where the research is. And that's what I said in my proposal. We don't really have all the information to figure it out. But the fact that they've been able to do it in accelerators and the ion sources they're using are not that much [brighter]. We're using lasers. So, anyway, that project is going in Phase I of an SBIR. I think we'll get to Phase II. I think we'll make it work. And Dave swears that if you had an instrument that could be put in a hospital, that this would really be [significant]; because, see, what you do […] is you take one dose of the calcium-41 at some point in your life. From then on they can monitor your calcium turnover, et cetera, just by doing a very simple measurement from your urine or blood or whatever, and the calcium <**T: 190 min**> levels are very high, so it doesn't take very much fluid to get the sample. You know, once a year or whatever time you want it to be as part of your standard physical, you run it through the machine and it doesn't cost much to do.

GRAYSON: It doesn't cost much if you don't have to use the accelerator […].

VESTAL: Yeah. But you're not going to have an accelerator in every hospital, even though there are now, commercial accelerator mass spectrometers…

GRAYSON: Oh, really?

VESTAL: Yeah. There's a company in Switzerland building them and I think a company in California. Well, Bob [Robert A.] Betham, who you probably know…he's involved in the one in California that's commercial instrument.

GRAYSON: But these, I mean, I would assume that what you're talking about would be somewhat less expensive than accelerator mass spec?

VESTAL: I think so, yeah. Quite a lot less. Well, you know cost of building is less than a hundred thousand. They're talking about they might get down to a million for these special versions. There was a use originally, where there were tens of millions. But that's one of our toys. We've got another project we just got funded and I've got a second [version of it] being reviewed in the next couple of weeks. As you know this isn't going to come out anytime soon. Some of the stuff is not yet [public knowledge]—well, not generally.

GRAYSON: By the time this gets going it'll be published.

VESTAL: Yeah, probably. We were also looking at electron transfer disassociation in time-of-flight. And what I'm doing there is a merged the beam between the negative ions and positive ions. Now, if you can get the relative velocity really low, then these processes have an extremely large cross-section. And we're actually making…we can merge an electron or a negative beam and positive beam with a relative velocity of less than a meter per second. That corresponds to temperature of 0.06 Kelvin or something like that. But, what we did is we reflect one beam and bring the other one in linear until we adjust the…we time it right and adjust the velocity, so they have the same velocity. These two beams, traveling along together [react with] a very large cross-section

GRAYSON: Well, yeah. You got the charge working for you.

VESTAL: Yeah. Then, we just analyze [them] the same [way] we do in the TOF/TOF, just a variation on TOF/TOF. So, as I say, that was one also, the first time through it didn't get a score. The second time through we got a really good score and then, I submitted a somewhat expanded version of it to an SBIR and it didn't get scored, but…

GRAYSON: You have any data or, I mean…what about neutralization problems with the two beams?

VESTAL: Well, you start out with a doubly charged ion […] well one criticism of the first one: you don't make any doubly charged ions by MALDI. Well, of course that's not true…

GRAYSON: No. It's [some], but not a lot.

VESTAL: Yeah. You don't make as many as you do with electrospray, but for anything of any size, the double charged is quite important. These are fairly small peptides it's a significant fraction. It's not 100 percent. And I think we can actually do double transfer, not quite as large a cross section, but because these things are traveling along together, […] Langevin cross sections are also pretty large…not as large as ion-ion. But I think there's a pretty good chance we can start off with a positive ion, and a singly charged ion. Get the transfer which gives you the dissociation and then, reionize it with a second process which ionizes it on over to the other charge. So, you start off with singly charged positive ions […] reacting with your [reactive] single charge negative ions and you end up with…

GRAYSON: Neutral.

VESTAL: You end up with negative ions of…they're both negative ions. You've got the leftover negative ions from the array, then you convert the other ones to negative ions. And you've got some neutrals as well. I think [by] double transfer reaction it may be possible to even look at singly charged ions. I'm not sure about that. That's a little harder. I mean, Langevin cross sections are about four orders of magnitude smaller than the…

GRAYSON: That's L-A-N-G-E-V-I-N, the…

VESTAL: Yeah. That famous Frenchman [Paul Langevin]. 1905.

GRAYSON: 1905. So, let's see. According to my calculations, you're 70 years old.

VESTAL: When?

GRAYSON: And you've got a new company that's creating new [instruments].

VESTAL: [When I started the company, I was 70 years old.] By the way, what time is it?

GRAYSON: It's 11:25 a.m.

VESTAL: Yeah. I got about twenty more minutes.

GRAYSON: Okay. Well, let's keep going forward here, and [then] decide how much more we need. But, basically, you've got a whole, at least, what? We're talking three or four <**T: 195 min**> ideas about new ways to do things. [laughter]

VESTAL: Well, it's been kind of growing, I guess. I mean, I started off with the idea of doing a better job of doing MALDI, and MALDI/TOF, and TOF/TOF And the other project was better interfaces between [separations and] MALDI/TOF and TOF/TOF, not only LC, but interfacing with gels and tissues and all this. So, the other project is more focused on that and [those were] the two ideas that I started off with and this other stuff on isotope ratios…that's come out…you know, it derived from what we've done. I don't know. I don't know how much more new things we'll do. As long as we finish up all the things we got started, we'll do pretty well.

GRAYSON: But, I mean, some of these ideas could make a reasonable amount of money if they come to fruition.

VESTAL: Oh, yeah. No, I think that all these things have commercial potential. We're not doing anything just for the fun of it. I mean it is fun.

GRAYSON: Oh, yeah.

VESTAL: I think in order to get it funded it has to have some practical sense.

GRAYSON: So, most of the people working for you now are people that have…I mean, I imagine there's some engineering expertise. There's a lot of scientific expertise.

VESTAL: Yeah. It's mostly engineering expertise. Initially, we added only one Ph.D. scientist, besides Chris and I and Steve [Stephen J. Hattan] who was doing the separations and worked with Steve Martin, before. We just recently, in the last year added two more scientists, Ken [Kenneth] Parker who was one of the people at AB […] And [P.] Jane Gayle, who you may know.

GRAYSON: I know Jane.

VESTAL: Who just recently retired from a job with Bristol Myers-Squibb. And there she was an executive, but she's a very good physical chemist and I've known Jane for a long time. So, she's working on the isotope ratio project. So, the rest are good software and hardware and electronics [specialists].

GRAYSON: Yeah. It seems like electronics for this is really […] challenging.

VESTAL: Well, I've got two guys that are really good. We built our own digital delay generators and so forth, and state of the art stuff. And the software guys are extremely good at using all the brightest and greatest tools. I was able to pick up a lot of the people that were really good and wanted to do something new and willing to take a little less money in order to do it. So, it's been a fantastic group of people. I never enjoyed things more, because there's…making the payroll…and Chris worries about. There's nothing else to worry about.

GRAYSON: So now, I'd just like to talk about the time-of-flight technology a little bit. When I had a time-of-flight, one of the old Bendix instruments when I was at McDonnell Douglas […].

VESTAL: It all goes back to Wiley and McLaren, who developed the technology down in the basement.

GRAYSON: Yeah. Now, my understanding, one of the things that they were working on at the time, is that they were trying to improve the pulse shape. They wanted to get the rise and fall times, these pulses [in the ion source] to be faster and larger.

VESTAL: Yeah.

GRAYSON: Okay. Of course, they had the detector in line of sight of everything. So, that was something that they had…

VESTAL: [Linear] time-of-flight.

GRAYSON: Yeah. They didn't realize…

VESTAL: That was before the mirror.

GRAYSON: Yeah. They didn't realize how destructive that was with regard to resolving power. So, [when you] put the mirror in. You're going to pick up the [resolving power] right off the bat.

VESTAL: Well, it's a little more complicated than that. Yeah. You [need the] mirror to get high [resolving power], but I think it's a little bit misunderstood.

GRAYSON: You're going to improve things significantly. Let's put it that way.

VESTAL: You can get high [resolving power] with a linear system, but you can only do it for one mass [using the equations of] Wiley and McLaren. You can first order focus out the velocity spread in the ion beam. You can't remove both the velocity spread and the spatial spread. You make those two focal points close together and then it doesn't matter. Of course,

you do that, you have an instrument that's not very useful, because it's too short. So, you got to measure the time-of-flight. What the mirror allows you to do is extend the flight time without introducing any new dispersions, you just reproduce what you had here much further away, until from the point [of view of timing] you get a decent amount of [resolving power]. The mirror is critical to performance, but that's completely understood now. I mean, nothing much to do there, and the original paper about a mirror is interesting, because he only looked at one limiting case in the way he described it, which works very well. But when you <**T: 200 min**> really dig into it, and I'm not the only one's done this—some other people […] have, but […] the geometry that he was using is not the best geometry. But it's the same idea, second order correction. But the ion source […] is really where most of the action is in terms of getting everything right.

GRAYSON: My understanding is the newer instruments you're getting deeper pulses faster than you were, say, if it was the old Bendix.

VESTAL: Well, I suppose. Electronics is enormously different than it was then. But the pulse itself doesn't matter that much…the rise time and all doesn't really make that much difference. You need to have a reproducible pulse. As long as it's reproducible, [that] technology is well established [now]. It's kind of developing both these switches for years before. Interestingly enough, he at one time was the electronics guy in the same laboratory where Franz Hillenkamp was.

GRAYSON: Oh, wow.

VESTAL: Hillenkamp wasn't even aware of it until much later. [laughter] I think both Hillenkamp and I were both a little disgusted with ourselves that we didn't adopt delayed extraction much earlier. I knew about the Wiley and McLaren's ideas idea and all that […] and yet, until these guys who came out of left field, Brown and Lennon published their paper, you know, was saying, "Oh, we put a delay on, you can get this." I said, "Oh, yeah. I knew about that." And somehow, […] you don't think of those things [but] when you think in hindsight and you say, "Well, how could, I not have thought about that?" And Hillenkamp really kicked himself about that, because he not only knew about it, he had [the] guy who knew how to build it right there in the building. [laughter]

GRAYSON: […] That's a bit strange.

VESTAL: But I guess I get some credit for that. I'm not sure I really deserve it, except that we did it first commercially. To take that idea and [implement] it…because that changed MALDI from "it was interesting […]," to "oh, yeah, that really works […]." I showed that the first time in this shoot-off meeting they had in Munich [Germany] one year. Or, was it in Munich or

maybe it was Davos [Switzerland]. Anyway, it was the first time we actually showed results from delayed extraction. My friend, Peter Roepstorff was there and some other people and they looked at this and they said, "Wow. This makes all the difference in the world." I said, "Yeah […]." Raimond who was one of the pioneers in laser desorption, he worked with Hillenkamp before MALDI, but he was at this meeting where I first showed the delayed extraction results. We actually, had an instrument there—the first one we shipped out of the building. He came by and I was showing him, you know how it works. He said, "Well, it can't work in the reflector mode." I said, "Well, let me show you." Because he had … well, the reflector [that's important and]…

GRAYSON: […] Well, I mean, the better you get out of the source, the better you get…

VESTAL: Yeah, the combination […] with the reflector really gives you much more. There is a limited amount you can do with the linear even with [delayed extraction, but combined with] the reflector then you can use a very short focal length and refocus the mirror and it all works very well. It's all written up in my papers I've written there […]. Wade through the equations.

GRAYSON: Well, I guess, we're probably…

VESTAL: I probably ought to go pretty soon.

GRAYSON: …getting close to the point where you can take a break […]. I'd like to get together later, when you get through this afternoon with your activities, because […] we've kind of gone through your career, but I'd like to actually […] discuss some of these other issues that […] we touched on, […that are] part of the interview. Then, I'd like to get a little bit of a video clip just for fifteen minutes or so, of you talking about some of these issues.

VESTAL: You know, I think this session this afternoon is over about 4:30 p.m.

GRAYSON: I think you're right. So, I looked at it last night.

VESTAL: It's the one that Catherine's [Fenselau] getting the [Ralph N.] Adams Award [in Bioanalytical Chemistry].

GRAYSON: Yes. I should probably go to that too.

VESTAL: But after that, I think I'm free the rest of the day if you want to get together.

GRAYSON: Why don't we plan on getting together at 4:30 PM? I'll check with Greg, who manages this room and see if we can't revisit this space, because it works out pretty well. It's quiet.

VESTAL: Yeah. This is a good spot.

GRAYSON: We don't have anybody <**T: 205 min**> traipsing through here, making a lot of noise.

VESTAL: Yeah. So, you got me talking more than I…

GRAYSON: Well, that's what I'm supposed to do.

VESTAL: You did very well. [laughter]

GRAYSON: […] But these insights into […] how things actually happened are a lot more interesting than, what it looks like in the literature […].

VESTAL: Oh, yeah, a lot of the stuff you don't put in a paper. Maybe just as well. [laughter]

[END OF AUDIO, FILE 1.1]

GRAYSON: […] So, this is Mike Grayson and Marvin Vestal again, getting ready to wrap up our interview. We really covered a lot of his career. I just want to go back and touch on a few things that we didn't get an opportunity to talk about too much. […] I know you've been through this whole business with these, what I call "churning of companies." Company A buying Company B, and all this. What is your opinion of that activity? Does it really produce anything other than somebody gets rich along the way or maybe they don't?

VESTAL: Well, I think it's a mixed bag. I say that in general, I don't think it's a plus for science. But, these things […] generate a life of their own sort of thing. I think the first merger we joined PerSeptive, and there were some problems, but I think that made a lot of sense, because we had a small company. We had got to a certain size and we were going to have to do

something different if we wanted to continue doing what we were doing, and the alternative was really to go out and raise a lot more capital from venture capitalists or somebody or join up with somebody else who was doing that. That worked out really pretty well, I think, both for the science and for me, personally.

But then, when PerkinElmer bought PerSeptive, spun off PerkinElmer to E.G. & G. and then we became part of the Applied Biosystems. I think the problem with getting into a big organization […]; the management often goes where they're interested. Like all corporations, their main concern is the next quarter. As long as you can […] grow and make a profit [the next quarter], then that's part of it, and everybody's happy, but that means it's very hard to look very far down the road. Some businesses, maybe that isn't necessary, but I think in the scientific research business, whether instruments or otherwise, it is necessary. And so, I think not only the big instrument companies, but the big pharmaceutical companies and chemical companies, and everybody suffered from a lack of vision—more like a lack of long-line vision at the expense of looking good for the shareholders now. And I'd say it's a conflict and it's kind of the way the system works, but I really do think most of the innovation comes from small companies or universities, and less so from universities these days, I think. I'm not sure why. At least in the instrumental area.

GRAYSON: Yeah. One of the things that I see, and I don't know how much was observed in your area, but for instance a perfect example, is this outfit InBev buying Anheuser Busch. So, they go out and borrow a ton of money to do it, and then they [have] to pay off the debt, and so they have to cut the people. It seems like the process of doing it is so painful and messy.

VESTAL: Well, a classic case in the business of course, is Waters in, you know, Bain & Company, which is [Willard Mitt] Romney's company, did a leverage buyout of Waters. Took it private, laid off a lot of people, cut a lot of costs, and then turned around and spun it off as a public company and made a lot of money in the process. And they walked away with a huge amount of money and people…and Waters didn't really benefit from that. They ended up being a viable company in the end, but I know the people [that were] involved in that internally felt like they'd been raped, and I think perhaps they were. I think, of course, Jim Waters sold out before that. But that happens in a lot of […] these so-called private equity companies. They do well for themselves, whether they do any good for anybody else, I'm not sure.

GRAYSON: Unfortunately, I think you're [right].

VESTAL: And it's true in the scientific instrument business, as well as lots of other businesses.

GRAYSON: So, now you had a number of interactions with the government on grants. Was that an altogether reasonable experience or did you have grief at times?

VESTAL: [laughter] Well, you know, it's a mixed bag. I've had a reasonable amount of success both in university and in the company at getting grants, but it's a difficult thing and I don't know that I know how to improve it particularly; but I think everybody feels that the probability of getting funded on any particular application is getting smaller and smaller, partly because <**T: 05 min**> the number of people doing work is getting larger, and the funding is not growing at the same pace, although recently it's gotten better, I think.

I've got to say my results are mostly pretty positive. I think the thing that changed between when I was first doing this and more recently is I served on an NIH Study Commission—study section—for a while, and I reviewed a number of proposals, in a special study section status. And at that time, I was really impressed by the quality of the people they were able to bring into the study section, because I was relatively junior, I'm [not] including myself in that. But, a lot of really very well known and very respected scientists were serving on these panels. And my impression was the higher the level of people, the better job they did of reviewing, and the more fair they were, but that's not necessarily universally true. There are always people that…but it seems to be now that […] a lot of the people in that category, I'm sure they're asked, but they don't serve. They're too busy with other things.

GRAYSON: Oh, sure.

VESTAL: So, they don't serve on these committees, and so the committees end up being made up mostly of relatively junior people with a limited amount of experience. Some of them are very bright and very capable, but I think there's an awful lot of risk-averse behavior; that is, if something is clearly going down the track and something that's been successful that's got a pretty good chance, if it's at all off the beaten track, […then] the PI has to do a really good job of demonstrating that he knows what he's doing and it is likely to succeed. And even then, you're not always successful in doing that no matter who you are.

And I've had my share of that, although, generally I've done pretty well. I think the NIH process actually works pretty well. At least in my experience, it's better than NSF, although other people might have a different impression. A lot of the other agencies, including NSF, tend to…the people who are funding research decide what they want to fund, and then they look around for people who are doing that to fund—they sort of pick out the best people, and so it's less based on original proposals and more based on track record and what people's background is. I know a lot of it's done through, somebody decides they'd like to fund some work in some area, and they make for a lot of…competition to get proposals. But I think they pretty much have a pretty good idea of what they're going to fund before, before they do it. And in some places that makes sense, probably, but it […] doesn't really make for originality, because people doing this are not necessarily looking outside of what's going on. And I think for young people, it's even worse. Someone like myself, who has been around for a while, you do have a certain amount of capital you've built up, I guess. But I've seen this on my proposals and also seen it in others where people that I knew collaborated with […]; the comments will be very positive about the investigators, "This is a great group; I really like them. But, it won't

work for this, this, and this reason, and so, therefore, we don't feel that [it should be considered]." Or it's not innovative. And some of these comments are really very subjective, and not based on [anything in the] proposal or it's one person's personal prejudice about the science.

GRAYSON: Yeah.

VESTAL: And I don't know how you avoid that, because after all, […] scientists are never as objective as they might like to think they are, including myself.

GRAYSON: Sometimes I wonder also if the people that work in these study sections and that are looking at this process or doing it, know that there's a fixed amount of money to be distributed […]. So if they're not going to fund something, they have to come up with a reason why not. So, to a certain degree, it's after you decide what you want to do, then you got to come up with reasons why you can't fund something that's actually probably quite a good proposal. But you just aren't going to fund it, and you can't say that. You know, we're not going to fund it, so you got to come up with some reasons to […].

VESTAL: […People usually] think of the same reasons why to be skeptical about it, but it varies quite a lot with the people in the study sections, and it's […] a pseudo-political process, I guess. […] And I must confess, I don't know of any better way of doing it, but I think if you have a really good idea, and you believe that you have a really good idea, and you pursue it, you will get it done, one way or the other. You'll find somebody that'll support it.

GRAYSON: They may take a couple of passes?

VESTAL: Yeah. Or, you may have to go out and raise some money and do it privately or whatever, but I <**T: 10 min**> think generally, if it really is a good idea and you really know what you're doing and you can show that […] it is scientifically sound, then you will succeed eventually […]. And maybe that's the best you can hope for, you know. Nothing comes necessarily comes easy…

GRAYSON: Yeah. Well, I guess that's the reality of it […].

VESTAL: If there's more money available, then more will get funded. And the recent expansion of the NIH budget and NSF budget will certainly help that. But we built up a fairly large scientific establishment, and it's [like] the health system, but bigger. The more advanced

it gets, the more money it takes to do cutting edge research. You can't do a lot with a test tube and a Bunsen burner these days. [laughter]

GRAYSON: That's for sure.

VESTAL: Nothing that I know [of can be done with] that. It necessarily becomes more expensive. I think the era of really big science may be waning, which I think is a good thing. You know it started off with the [atomic] bomb project, and then more recently the DNA Consortium to sequence DNA [Human Genome Project], which I think everybody would agree was worth doing, and it turned out to be cheaper than what people really expected originally, and faster. So, it's a very good baseline data. Of course, the problem with that is you get this stuff funded by…basically, promising more than it really can deliver, and that's true of all of our proposals. I mean, I've found the same problem with SBIR proposals. But, [when] the human genome more or less finished, [they said], "Oh, we're going to solve all these biological problems." Yeah, it's essential to doing that. I think it's a very important advance; […] it's been great for evolution in certain areas, but as far as really understanding biology and learning how to treat disease and all…it hasn't done nearly as much as its proponents said it would ten years ago.

GRAYSON: Well, I think it's revealed the problems that really need to be [worked on].

VESTAL: Yeah, biologically. Biology is very complicated. [laughter]

GRAYSON: Yes.

VESTAL: You start somewhere, and the DNA is a good start. There's an awful lot of work [yet] to [be done] and there's...there's no shortage of good research to do. I think people will do it. The problem that I have with the research establishment in the U.S. right now—and it started way back when [I was] in the university—is the graduate student. We're not getting very many American graduate students in science. And most of the universities, even, you know, when I [was at] Houston, [if] it wasn't for the foreign students that came in, our graduate program had to shut down, because we didn't have enough students to justify it. But we got a lot of really quite good students from China, and Korea, and Asia, Middle East, Europe, every place. And that made it go, and of course, a lot of those people have stayed in the U.S. If you look around at any meeting—look at the mass spectrometry meeting, now—I would guess half of the participants were born somewhere else. And many of them live somewhere else, too. Now, that's an international thing. [But] a lot of these people have gone back to their home countries, and that's great because, earlier on, they came and [most] of them stayed, which was good for us. I must say isn't good for their country. But, that has improved, it's just that I would like to

see more Americans given the message that this is important to do rather than, going to Wall Street and making a lot of money.

GRAYSON: But that's the draw in this country is making money.

VESTAL: Well, I know. It may be starting to shift back. I hope so, because with these obscene bonuses and stuff gets put into perspective, I think people might realize that there are other ways, other things, to do that are more satisfying. […] Someone said do I want to do this, you know, mess around with stock? No, it's dull. Unless you're really good about money, it's not very interesting.

GRAYSON: [Yeah].

VESTAL: And of course, the money becomes a way of keeping score. After you've made your first few million, how much difference does it make if you make another ten? Or I'm making more than this other guy, and therefore [I'm better].

GRAYSON: I think it's a little kindergarten thing. I've got more or better than you got, and that's [the goal].

VESTAL: Some of these people are smart enough and aware enough to start giving it back, people like [William Henry "Bill"] Gates and [Warren E.] Buffett and so forth. I think that's good. It's the other people who see it as just they can have more of whatever. Screw everybody else. But I think the science establishment in general is pretty reasonable. I like scientists [a lot] better than I do [people] on Wall Street. That is really the most amazing thing about being a scientist is you meet people from all over the world, and you establish friendships almost instantly <**T: 15 min**> with people that you have no previous contact with just based on knowing them through the science. And that's probably been one of the most pleasant things about being a scientist I can think [of]. I have to say except for my—from a few friends who are living, not very many left, but from my high school days—most of my friends I know through science. My really good friends are [all] people I met, not necessarily, exactly [in] the same discipline, but people I met [as the result of being] involved in science. And so, it is an important part of my life […].

GRAYSON: So, you have been both sides of the management experience. You've been on the management side and you've been on the, on the being managed side, right.

VESTAL: Yeah. Well, yeah, I suppose. I have been president of the company, and I've been the vice president of a company, and the director of research and a few other things. I've always considered myself a lousy manager, and I think that's probably accurate, although I think I'm a better leader than I am a manager. Perhaps maybe I, maybe I let my prejudice that really good managers are in fact good leaders, and they don't have to do a lot of other things, if they can motivate people to do something, because they really want to do it…so, my management style is not to have a lot of meetings; probably fewer meetings than one should have, but just talk to people in one-on-one or in small groups and just kind of let it happen as time goes along.

GRAYSON: Not micromanaging or over-managing, so to speak.

VESTAL: No, hopefully not. But just be available to talk to people and if you want something done, you know who to go to and say, can you do this? This is what we need to do. And you know that works, I think. […] There's all these theories now about management and about process. And one of the things that I ran into at AB that I'd never been involved in before was they'd have these huge meetings of all the middle managers up, to talk about how can we do things better. And at one point, AB went through a period—which was not surprising, because they had been growing at a tremendous rate with great profits—as long as the genome…as Tony White described it, "we started the World War and we were the major manufacturer of the weapons." And so, they did great on the DNA sequencing. They had great business, good profits. But, you know, that eventually flattened out. The Human Genome Project was really finished, and of course, other technologies come in as well. So that business, sort of, reached a pinnacle and didn't grow as fast, and [there] was more competition, and so, they had these meetings to say, "Okay, how can we get…," because their goal had been 20 percent growth and 20 percent EBIT [Earnings Before Interest, Taxes]…

GRAYSON: Um-hmm.

VESTAL: And they were doing that for several years, and of course that's very good from a corporate point of view, but then they flattened off to 10 percent or something, or maybe even less. I think they both went to zero and their earnings were still positive, but they were reduced. And so, the reaction was…the HR people, they get all the managers together, and do these exercises and have these meetings. First of all, listed, you know, the five things [we] need to do better, better job of service. We need to do a better job of selling. We need to be more responsive to our customers. We need to invoke the gurus in the fields. And I think there was one other, I don't remember now.

GRAYSON: How do you remember these?

VESTAL: They'd do these exercises, where you paste things up on the board and you had these little groups. You'd go off and discuss things and come back and then report it. Standard stuff that these people do. And I suppose it might have been useful. I think it's mostly a waste of time [and money. The] amount of money involved [is considerable]. You have a hundred and fifty people spending two or three days. Highest paid people in the company. […] I was on the guru committee which is with Jack Richards and a few other people. We sat down for a day. We figured we answered all the questions. We knew what we needed to do. Same thing we'd always [done]; we knew the people in the field who were influential and said, these are the people you need to talk to. Develop relationships with some of these people, and you do that, then what else is there to do?

GRAYSON: Yeah. You have a network and you know the people who [need to be part of it].

VESTAL: But that wasn't good enough. They wanted a more <**T: 20 min**> [comprehensive view]. But one of these days, I remember, had gone on and on, and they had this - each table was supposed to stand up and make a comment or a question, and I stood up and I got my table to [agree to] let me [represent it]. I said all this process stuff is great. You know, I think there are areas we can improve in terms of how [well] we do things, but it seemed to me that the real crux of the matter […] if we really want to get back to growth and better profits, is really not process, but substance. What are we going to do? What products are we going to develop? And which directions are we going to go? And they kept [saying]. "Oh, no, we're not going to talk about that. That's not for this session." They had all the people in the room. It would have been a quite fruitful discussion, I think. There may not have been agreement, but at least if you did something like that, I could see some value in it. If you really don't know you've got all this technology, and [you know all of this stuff that] could be done. We talked about doing more in proteomics but never could get a lot of traction on that. We did some, but it was never really made into, "We're going to replace genomics with proteomics and put that effort into doing it." I would have liked to have seen it, but it didn't happen. And I understand why it didn't happen, but there wasn't that process going on […]. And I think all big companies fall into the same [situation], so you end up with lots and lots of […] meetings, and you bring people together, and most of the meeting ends up with a few people educating the other people about certain things, but the people that are being educated are the ones that are making the decisions, not the ones that are doing the education. So, they try to gather enough facts and then they'd say okay, we'll do this. Well, maybe that works. I think it may be a good model for some businesses. I don't think it's a good model for science.

GRAYSON: Yeah, one of the other interviews that I did, we talked about the fact that right after the Second World War, a lot of the managers in companies that had serious scientific experience and/or the engineering experience in doing process in a plant. Not distillation, but a refinery and all this kind of stuff.

VESTAL: A lot of people came up through the ranks.

GRAYSON: Yeah. So, they knew what it was like to have to do this stuff.

VESTAL: Exactly.

GRAYSON: Nowadays these guys that come in as managers, they come in without really ever having that kind of experience. They go with all the MBA stuff about how you manipulate the system to make it look like you're making a buck.

VESTAL: No. I think that's very much true, is that management classes changed. And people that came from the business, to people who came from outside [the business] with these credentials that they were professional managers and knew how to do this stuff.... And to an extent they do. They know how to make money, for themselves at least; sometimes they make money for the company even, but I really don't think a lot of understanding of the business on the underlying that you can do that very well. Some people maybe can, but if they really listen to the right people, they may actually be successful at doing it. But, I agree that during the time that I was first involved with this, all of the top managers of these companies were people who came from the [science side]. Take a company like BioRad for example, the guy who started that—he and his wife—he's now semi-retired, but he's on the board, but they built that into a big company based on their ideas. And I think it's kind of run out of gas now because they're semi-retired. And it maybe the same way with their kids. I don't know if their kids are as innovative as they were.

GRAYSON: Did you ever have any problems with getting a paper published or anything getting in literature […]?

VESTAL: Not any significant problems. I've usually been pretty careful about the way I write things. You get some negative comments, but I would say with very rare exceptions that the comments I got have been fair and reasonable. But, you know, there was something that needed to be improved or made more, clear or corrected. I can only think of one or two cases, where…and I've been a little on the other side of it a couple times when I read something that I said this just isn't…this can't be so.

And I did enough work to write a comment on the paper.[16] I don't do that very often, but a couple times. The H double minus thing was the one that I got involved in, because I knew…I understood the experiment, and Bill [William] Aberth <**T: 25 min**> who had actually

[16] M.L. Vestal, "Comment on 'Existence of H^{2-}, a Relatively Long-lived Doubly Charged Negative Atomic Hydrogen Atom'," *Journal of Chemical Physics* 65(10) (1976): 4331.

built the apparatus that it was done on, and I agreed.[17] He wrote a comment as well, which said exactly the same thing independently, that it was an artifact that they observed. And the guy who was doing it was so gung ho to be able to show something really earthshaking. Didn't really want to do the key experiments to show that it was an artifact. And these things happen occasionally, but scientists in general, I think, are pretty honest and straightforward. I know there are people who worry about somebody cuts them down [in order to get] priority on something, but I really haven't run into that person. I've had great…you know, occasionally been in competition with guys, but it's always been […] fun. I never really felt any real animosity or any cutthroat behavior. Or maybe some [that] you're not aware of.

As far as papers and all, I always felt that the reviews were really quite fair, for the most part. […] I've had a few that the reviewer obviously didn't understand it, and that I've always just written a rebuttal and said, "Okay. Here's what you don't understand." Maybe [had to give] a reference or two. When I've done that, it's always [that] the editor's just gone ahead and done it or the reviewers…

GRAYSON: Yeah. Well, I mean, the editor does have the opportunity to step in.

VESTAL: Yeah. I have not published a large number of papers in my career compared to other people that I know; because I never have really been all that driven. Even though I like to write, I sometimes hated to stop and write papers.

GRAYSON: Oh, well, I understand that.

VESTAL: So, I could have published more papers than I did, but I think I published enough. And as I say, I don't know of any paper that I published that I wouldn't stand behind now.

GRAYSON: Do you feel that you were able to work in teams with people in […] both in industrial and academic work?

VESTAL: Oh, yeah. I've always been […] early in my career, I was a graduate student working with postdocs. But I had more experience than they did, but we got along really well…Calvin Blakely, for example, [and I] never had a problem. Some people, I think, feel like they don't get enough credit for the work they do, and often that's true. I think the professor very often gets more credit than he deserves. And I certainly put myself in that category as well. In that you [can] tell everybody who did the work, […] yet, once you're established if your name's on it, most people refer to it as your work.

[17] William Aberth, "Comment on 'Existence of H^{2-}, a Relatively Long-lived Doubly Charged Negative Atomic Hydrogen Atom'," *Journal of Chemical Physics* 65(10) (1976): 4329.

GRAYSON: Yeah.

VESTAL: You know, I wouldn't put my name on something that I didn't contribute to, but you have to say sometimes my contribution was only 5 percent and somebody else did most of it. Ah, but that gets lost in the system and it's hard to overcome that. And particularly if people are not themselves forthcoming and so forth, and this is a problem that Calvin had to some extent. Calvin's a very bright guy, very capable guy, and did excellent work. But he was not the kind of guy that'd get out there and blow his horn. He would give a talk, but he didn't really give a very good talk […]. He just was a quiet sort of person, and so he didn't get the recognition he deserved; and to some extent I think he blamed me a little bit for it, because I got recognition that he should have got, and it was true, absolutely true, but I didn't know what to do about it. I mean, I did what I could. Every chance I had, I pointed out how important his contribution was, but sometimes people don't hear that. And I do that think generally the system is such that the senior guy gets more credit than he deserves, and the really good people do go out of their way to make sure [other] people understand the credit that other people should get, but it never quite makes it, I have to admit.

GRAYSON: But, when you look at the literature…I know I frequently look at the last name on a paper and figure this is the group leader or what not, the driving force behind it. And you kind of think, these other people are students or subordinates working for him, and so on, so forth, so you don't really realize exactly how the credit is distributed amongst all those people. […] Well, we talked a little bit about research management. I guess, obviously, <**T: 30 min**>, one of the reasons Applied Biosystems didn't work out is because you had a different view of what direction to go…

VESTAL: Well, in PerSeptive, Noubar was a great leader and entrepreneur. He was a lousy manager, but the company fared very well because he did provide direction and leadership. As far as the nitty-gritty of managing and dealing with problems and all, he was even worse at it than I am. [laughter] But that was not as bad as somebody who is really a good manager that really doesn't understand what's going on. So I've seen all kinds, and what is always amazing to me is some really bad managers that really do very poorly in terms of their job, end up persisting very well. And that's true in AB Sciex at the moment. One person…everybody agrees is incompetent but yet survives.

GRAYSON: Somehow they manage to [survive], yeah. I don't know [how that continues to happen].

VESTAL: There's something about that that I don't understand, and need to. But I do think the rise of the professional manager at the expense of some people who really know the content

of what they're working on has been a bad thing from the point of view of what's happened in our companies and our country. You know, the professional management classes, whose goal is to manipulate money and not to do anything else, is a negative.

GRAYSON: Yeah. I don't whether that's going to change. Things do swing back and forth, but it just seems […] we keep getting more of the same.

VESTAL: Yeah, I don't know. I'm old enough; I don't have to worry about that. Right now, I've got the perfect management situation. [laughter] All I do is please my wife.

GRAYSON: Now, she's an important part of your team. You mentioned her name a couple of times […].

VESTAL: Yeah. She was a graduate student at the University of Houston while I was a professor there. And she actually was sort of in my group for a while. […] Up at Utah, I had done these measures of scattering of helium [ions from helium and argon plus] from argon and so forth, and had some really interesting results, and she came around, you know, looking for something to do, and she has a very strong mathematical background from Taiwan. So, I said maybe you'd like to do some quantum calculations, because we've got all these quantum oscillations. And so she worked with me a while, and then I sent her off to Bob [Robert L.] Matcha and his postdoc who was really a bright guy, who is now a professor at Houston, and they helped her get cranked up to do these calculations. So she did her thesis on this.

GRAYSON: So, who was this guy, Bob's last name…?

VESTAL: Bob Matcha, M-a-t-c-h-a. And Monty [B. Montgomery Pettitt] I believe […] who was a student, a graduate student, when I first went there, and was really bright. He went away somewhere else. He got his Ph.D. with Matcha and then went and got his postdoc […], came back as an assistant professor, but now he's got one of these endowed chairs or something. So she started out as a theoretician […]. Around the time she got her Ph.D. was when we got married. And she was looking for a job, and we had this company we had just started, so she basically ended up working for the company. She had a good enough chemistry background [in chromatography] and so forth, as well, but she […] was an organic chemist [and the opportunities in] theoretical chemistry [were limited]. She was in Vestec all the way through. When Applied Biosystems or PerSeptive decided it [was not] good to have both of us working for the company, she left and eventually worked for another company in the area there for a few years, and then retired a few years ago. And then, when I started this company, I said oh, you're too young to be retired. So, she's been with me after the business end of things ever since. She looks after the money, so I don't worry about that.

GRAYSON: Well, that's nice. So, seems your kids are really bright or not? Two Ph.D.'s and pretty innovative people.

VESTAL: We don't have any children.

GRAYSON: You don't have any children?

VESTAL: No. Not between us, no. I have children from my previous marriage.

GRAYSON: Oh, I see. Not clear on that one. Well, you mentioned you don't have a lot of publications, but you do have a ton of patents for a relatively […]. What did I see, fifty or sixty odd patents <**T: 35 min**> […].

VESTAL: […] I really didn't know until I started making that list, and I started listing all of them. I think I ended up with ninety-nine [publications]. I'm not sure I found all of them […].

GRAYSON: Yeah, okay. What is your opinion of that whole business of patenting things…what do you derive from that? […] I mean, I think most people think you get a patent, and then people come out and make this stuff, and you get money from it […].

VESTAL: Well, sometimes it works that way. Yeah, [thermospray] worked that way, except the University and Research Corporation got most of the money, but that's how the University worked is, they took 85 percent and gave the inventors 15 percent. And the 85 percent was split between the University and Research Corporation that paid for doing […] the patent. And if there were three of us inventors, we each got 5 percent. But all three were involved with the company, so I think that we paid the University a thousand dollars a unit or something like that.

And we collected twenty-five hundred [dollars] for the company, because we put a lot of additional IP, et cetera, in. I can remember we didn't really [want] to charge for it because we were building. We figured that's about what we made, realistically, on building one ourselves. They wanted to build them and wanted to pass up profit; we might as well do that, so I was negotiating with Hewlett Packard, and went out to visit them. We worked with the technical advisor on how to mate it with their instrument, sitting in this room with the bosses, Karl Schwartz and Larry Cattran, and a bunch of engineers. And one of the engineers was really a good guy, did a great job of designing a [lot] of [their] stuff. I don't know if he's still there or not, but he was obviously given the job of…I explained what we had in the way of the IP and so forth, and said we'd thought about it, […and a royalty of about] twenty-five hundred dollars a unit was reasonable. And so, this guy was given the job of arguing why that was way too much,

because they had to do this and that, and he was going along and he had a good presentation. He was going full stride, and Carl Schwartz was sitting in the back of the room. He was the boss, kind of leaning back in his chair, looked up and said, "That sounds about right to me." The guy stopped mid sentence. [laughter]

GRAYSON: That was it…

VESTAL: That was the end of the negotiations.

GRAYSON: Didn't have to do anything else.

VESTAL: But in general, patents, I would say, are more defensive than anything else. I don't know that I've ever collected a dime on any other patent, as far as I can recall. The company may have. We had this patent on delayed extraction, which frankly, I don't think is valid. […] Wiley [and] McLaren [had] done the basic [work, but] the company wanted to file something, so, I don't know, three or four of those patents are on delayed extraction because they kept a different versions of them. Of course, [James] Reilly at Indiana had also filed a patent. The guys who really discovered this did not file a patent, and they were probably right that they really didn't have anything [patentable]. We came up with a superfluous…you know, [it] may or may not have been patentable. Anyway, the patents were awarded, and Reilly's patents were awarded. He added our claims to his [patent], because he had enough disclosure that even though they weren't in his original version, you could cite disclosure in them […]. So, they end up sued. Bruker and AB were suing each other for a while and they'd end up settling. I don't know somebody paid somebody some money, I guess. And they agreed that they would [license] it […]; Bruker would take charge of licensing people, and then split the royalties on the future licenses, so somebody made a little money out of the deal, but probably they might have got back what they spent on the patents. They didn't make a lot of money.

GRAYSON: So, do you have to retain a patent lawyer for all this stuff?

VESTAL: Oh, yeah. I mean, I'm an expert at writing patents. I usually write most of it—my first draft—and then the attorney does a little work on it […]. What the attorneys are really good at drafting is the claim. So, a lot of technical stuff one has to worry about. I've usually done quite a lot of it myself.

GRAYSON: It seems like an arduous process.

VESTAL: Yeah. But if you've got a good guy who really understands that it doesn't take that long. So, we've got thirteen patents or patent applications in the new company, most of which are not listed here. Some of them have just been awarded within the last few months. I don't really expect to get any money out of these. It just, sort of, stakes out an area, which means that it's a bargaining chip […]. <**T: 40 min**> At some point, if there's really blatant infringement, then [you have some protection].

GRAYSON: Then you get into a legal [suit].

VESTAL: You really have to be prepared to spend a lot of money to defend a patent. Getting a patent is easy; defending it is much harder. I ran across this with thermospray […]. We agreed that anybody that we sold interfaces to, we would do that provided they signed the license agreement. Then if they wanted to take the design and build it themselves, that was fine. We'd help them and whatever. So, we licensed everybody except VG, and VG had this guy who had been doing it for years. He's not a patent attorney, but he functions as in-house patent attorney. I think he's still there. His name won't come to me right at the moment. But their standard position was, "We don't believe that we infringe on any valid claims of your patent," and that's the position that they usually take. […] So, we had everybody licensed but them, and I talked to my attorney should we think about suing them? He said, "Well, if you're prepared to put a hundred thousand dollars on the table right now, we could do it, but you have to realize it's going to take a lot more than that before it's done." I said, "Well, it's not worth even a small fraction of that because, first of all, they weren't selling that many." They didn't do it very well. And there's just no reason to spend any money on it. We did collect royalties, because we had the patent. I don't know how much money it was; it wasn't a huge amount. But, in general, I think patents are more bargaining chips than defensive things and you know, if you've got a patent on one area, and somebody else has got a patent on the other, then you usually work out a plan, work out a compromise where nobody sues anybody, because when you start getting these suits going, you know people making money are the lawyers…

GRAYSON: Oh, yeah […].

VESTAL: And occasionally, you know, AB sued Waters on this quadrupole coupling in the Q-TOF. And they won that, and they got, I don't know four million dollars or something.

GRAYSON: I think I remember that. At the time, we had an instrument from…MicroMass or VG or whatever it was.

VESTAL: Yeah. Well, MicroMass became part of Waters.

GRAYSON: Yeah. The instrument performed miserably and they sent engineers over and all this [to fix it]. Basically, they had to replace it, but they couldn't replace it because in the meantime, this suit had been settled, so to replace it would have been violating the legal thing; so they kind of replaced it, by replacing parts. But basically, by the time all the parts had been put in, they replaced the instrument. It was a little subterfuge…

VESTAL: But that's generally what I'm aware of, where there really was significant amount of money changed hands, because usually they get settled by people realizing that the best thing to do is just [work it out between you].

GRAYSON: Well, I think the naïve concept is, "I get a patent and [I'm going to] get rich." Well, no, it didn't work that way. [laughter]

VESTAL: Sometimes it happens.

GRAYSON: Sometimes yeah. Once in a while.

VESTAL: That's part of what happened with SELDI [Surface Enhance Laser Desorption Ionization]. You remember the SELDI story now.

GRAYSON: No.

VESTAL: Well, Bill Hutchens was a young professor or young assistant professor, associate professor at Baylor [University]. […] Anyhow, he was probably one of our early customers for MALDI and was actually very helpful in the early days, because he worked at Baylor in a group that did proteins and stuff, but he was quite knowledgeable about proteins. We kind of used him as an applications lab, and Randy Nelson, who worked with me, we worked over at his lab [a lot], and they published a number of papers on applications. And it helps sell the instruments.

But he got this idea of…well, he had two ideas, SELDI and SEND [Surface Enhanced Neat Desorption]. I guess he was convinced that his idea was worth a billion dollars. And he said that. So he went around…he would never tell me at the time that he had been consulting for me, but we really didn't pay him that much. We supported his instrument, but he was officially a consultant, and he went to all the big companies like Beckman and so forth, trying to peddle his idea SELDI. And it's really bizarre behavior. It turned out that nobody could reproduce the results. So it became a big fiasco. Although he did eventually get it going with this company, CipherGen [Biosystems] and <**T: 45 min**> sold some instruments.

GRAYSON: So, what's that acronym [SELDI] stand for?

VESTAL: Ah, surface enhanced laser desorption/ionization.

GRAYSON: Okay. Surface enhanced…

VESTAL: Laser desorption/ionization.

GRAYSON: Okay. Surface enhanced…

VESTAL: It's MALDI, but the principle is that you activate the surface with something that absorbs particular molecules and not others, so you can enrich the sample. And it does work to some degree, but it has a lot of issues and problems. It's really MALDI, because the surface does not enhance the laser desorption; it just enhances the sample preparation a little bit. Anyway, but that's a case where a guy went totally berserk because he went off and did all sorts of crazy things as a result of getting that. So I mean it took him a while, but he got a patent through Baylor, and he acquired the rights to it, and he was riding high for a while. He really went from being…seemed pretty reasonable to be just totally off the wall.

GRAYSON: Well, I think some people think that their idea is so unique and so valuable and what not, that they're very special and it's their pet thing. And they get really wrapped up in it.

VESTAL: Yeah. Well, that was certainly his case. As I say he did actually make quite a bit of money out of it, one way or the other. You know, people paid him off. But it was a bizarre episode, and generally I haven't seen that. I don't expect to make a lot of money out of a patent.

GRAYSON: So what do you consider to be your most important publication, if you had to name one or two?

VESTAL: I don't know.

GRAYSON: I mean, didn't you say you didn't publish a whole lot. You've got a pretty good record here.

VESTAL: Yeah. What I've been doing recently, what's really started, the paper that Peter Juhasz and I did together on the theory of time-of-flight and MALDI and so forth.[18]

GRAYSON: That's a recent paper.

VESTAL: Yeah. That was…I don't know, 1997 or something like that. I don't remember. It's probably later than that. So, no, that's probably…it's 1999. No, no, that's not it.

GRAYSON: Applications Delayed Extraction…

VESTAL: No. No, no.

GRAYSON: Okay. Extraction…

VESTAL: I think it's this one right here, [1998].

GRAYSON: Okay.

VESTAL: It's got a hundred-fifty equations in it.

GRAYSON: Uh-oh. [laughter]

VESTAL: Well, I really just tried to put it down in one place, all the equations in some detail that really applied. I expanded on that and drew some stuff too. I guess that's what I'm getting the award [ASMS 2010 Contribution in Mass Spectrometry Award] for. You were aware I'm getting the award.

GRAYSON: Yeah. This is one of the ASMS awards, which one, because I can't remember?

VESTAL: It's the ASMS Award for Distinguished Contributions. […] I had been formally told about it, and I think they haven't actually…a number of people have come up to me at this

[18] M.L. Vestal and P. Juhasz, "Resolution and Mass Accuracy in Matrix-assisted Laser Desorption Ionization Time-of-Flight," *Journal of the American Society for Mass Spectrometry* 9(9) (1998): 892-911.

meeting. And said they'd heard that. I said, "Oh, I didn't think it'd been announced yet." And I ought to talk to Gary [Glish, President of ASMS at the time the award was given].or somebody and said that was true. So, I guess the formal announcement will come out one of these days. The rationale for that was work on developing modern MALDI/TOF mass spectrometry. I forget how it's worded.

GRAYSON: Well, the TOF/TOF instrument concept is that…

VESTAL: That was my idea…

GRAYSON: That was your idea.

VESTAL: Yeah. I have several patents on that. I have other, better patents on it now, than the earlier ones, but. I suppose that from an instrumental point of view, that might be the [reason]. Not the papers, because we didn't publish that much early on, but as far as an instrument development, I think that probably was important to them. I don't know. I think the most important ones I haven't done yet.

GRAYSON: Yeah. Well, from what you said this morning, I can…

VESTAL: I've always felt that way. [laughter]

GRAYSON: What's coming up is better than, anything that's been done so far.

VESTAL: Right. This is why you do it.

GRAYSON: Right. Why bother? You're certainly not going to do something already done or somebody else has already done. So, I just want to go over some individual personal interactions that you've had. We talked about Rosenstock. I guess, is there anything in particular that you wanted to add to your interactions with Rosenstock?

VESTAL: Yeah. We only worked together fairly briefly, just a couple years. But he certainly had a great, very strong effect on getting me started in this business. I mean we remained friends and so will I <**T: 50 min**>. So, he's at least, one of my favorite people. And also, Wahrhaftig probably had more of an influence on me than anybody in terms of really both

providing me with his wisdom and knowledge, but also pointing me in the right direction in some areas, where I needed some guidance.

GRAYSON: He was your mentor in graduate school.

VESTAL: Yeah, one of them. He and Jean Futrell together. I'd say Austin was primarily the mentor, and Jean was primarily the facilitator, I'd say. But, they both were important […]. I'd say I had a little disagreement with Jean in the course of my leaving. But, we're still good friends. I'm eating lunch with him tomorrow, and even though he goes to great pains to look younger, he's actually a lot older than I am. [laughter]

GRAYSON: Yeah. I noticed that. He's been doing that for a long time […].

VESTAL: He never ages.

GRAYSON: Yeah. He looks pretty much the same as he did a long time ago. You mentioned Fred Lampe in passing, but I don't think we spent too much time talking about him.

VESTAL: Well, Fred consulted with us in the company, in Baltimore on radiation chemistry and stuff. He was a great guy. I worked with him at the company. He came down…well, he was at Penn State at that time. He came down regularly and helped out with all kinds of projects. He was really versatile guy. […] I have great respect for him. I nearly went to his place to do a Ph.D. I could have done that. And he continued on consulting even after I left. In fact, Al Yergey was certainly my replacement at the company there.

GRAYSON: Oh, okay.

VESTAL: Al would say that's not so, but it is really. We really didn't overlap. He came just as I was leaving and he did take over a lot of work I'd been doing. […] In fact, I was the one basically, that hired him. But we've interacted on lots of things since then.

GRAYSON: Okay. What's up with Hank Fales these days? I haven't heard much of Bill Milne lately […].

VESTAL: He's around somewhere. I [asked Hank] about him. [Milne's] retired…but he was retired, and then he was doing something else. Hank told me what he was doing and I've

forgotten, but he is still alive and well and percolating around that area somewhere. I haven't seen him in years either.

GRAYSON: He's an important part of Fales's operation there at [NIH].

VESTAL: Yeah, yeah. They were a real team for a long time. And he split off and went under this EI [electron impact]…stuck with the EI data and worked with Steve [Steven R.] Heller and…

GRAYSON: Oh, yeah, Steve Heller.

VESTAL: And all that stuff. So, he moves from being part of Hank's life, and moving over to that activity. And that's what he did the last several years of his career.

GRAYSON: Now, this name [Jennifer M.] Campbell here.

VESTAL: Jennifer Campbell. She was somebody we hired …I guess, it was while we were still at PerSeptive or anyway, around that time. We were first really developing the TOF/TOF. We decided we needed to add a young postdoctoral or somebody to work on this project. And we interviewed several people and she was actually just finishing her doctorate degree at [University of] British Columbia with Don Douglas. Of the people we interviewed, she was by far the most impressive, and even though she didn't have any postdoctoral [experience], we basically hired her as a postdoc. It was [kind of her] first postdoc, and she was very much involved in the TOF/TOF work, and all the way through. In fact, the team that did TOF/TOF was really me, her, and Kevin Hayden who works with me now.

GRAYSON: Kevin Hayden. How do you spell that?

VESTAL: Yeah, he's my right-hand guy […]. So, if you looked there's several publications and things, and some patents involving [her].

GRAYSON: Now, she moved on to other activities then?

VESTAL: Yeah. She's with BG Medicine at this point, which is where Steve Martin went after he left. He has since moved on to Novartis, but she's still there as well, had a child a few months ago. She's still active. But there's nobody left anymore [in the old group]. But, it's

true; in that project the three of us worked very closely together for a long time. But it was very complementary. She was good at doing things with the machine and dealing with software people to get things done. Kevin was the guy who looked after the hardware and made it work and fixed things and came up with a lot of good ideas and a lot of innovation <**T: 55 min**>. He stayed on. He's still doing it. He's one of my most valued employees or collaborators. And he has no degree; he's taken a few college courses in his spare time. He feels he doesn't know enough, but he knows a hell of a lot about a lot of things. A lot of things he does and otherwise, I would have to do, because nobody else in the company knows [how to do them].

GRAYSON: So, how did you come about hiring this fellow? Normally you're looking for people that have some kind of formal education.

VESTAL: Well, you know it's really kind of unusual, but that I was talking about him with somebody down at the AB booth earlier, in fact, the guy who originally hired him. He'd worked in the semiconductor business as kind of a technician—you know, things and ultra high vacuums, et cetera. And he moved to being a service engineer with VG and did that for two or three years. In the end, the New England area…he traveled all over the world for them, I think. He's a very good service engineer.

So, […] we hired him originally as a service engineer at PerSeptive, but he wanted to get into research, so he ended up working with Peter Juhasz on a project putting a YAG laser onto an existing instrument, I think […]. But he really wanted to do R&D and, by some combination of circumstances, he ended up working with me on the TOF/TOF, I guess. And we worked together ever since. I realized this guy can really [perform]. I don't care how many degrees he's got…

GRAYSON: He has or doesn't have.

VESTAL: Yeah. He learned at the SIMION Program very well for…

GRAYSON: Oh, good.

VESTAL: …ion optics calculations. He's labored through all these theoretical papers I've done, and he really understands [it], how to use it, and he can do the calculations and design the instrument as well. And he can make them. He put them together and made them work. And work with the machinists. His specialty is not electronics, but he does understand electronics and servicing and troubleshooting and so forth. He doesn't design the electronics per se. So he kind of fills that gap and then we have electronics guys that do the basic design; I would say everybody in the company is important, but he's the one I could least do without. I'd really feel a bit lost if I didn't have him, because it's a hard position. You know, I don't care…you can go

to any university. I couldn't find anybody as good as he is. Any doctoral student or somebody that really could step in and do his job right away. I mean, they could learn to do it.

GRAYSON: Well, he better not hear about this. He might get a big head.

VESTAL: No, he already knows it.

GRAYSON: But he doesn't believe it, so it's all right. He's very modest. He always feels like he's not as good as he should be. And things take a little longer than…he feels very bad because it took so long. It was easy if somebody else would have done it. What about John Fenn? Since his…

VESTAL: Oh, I've got a very friendly relationship with John forever. I mean way back before electrospray. I knew him when he was doing molecular beams. I've always had a great respect and admiration for John. I was really impressed by the work he did on electrospray and what he's done with it since. He's a real gentleman […].

GRAYSON: Yeah. When I went up to interview him, he was "the guy" by this time. Of course he won the Nobel Prize [in Chemistry in 2002] and all that stuff, and then he met me at the hotel. Of course he had a driver; I don't think he drives very well.

VESTAL: Well, […] his first wife got killed in an accident in New Zealand, and I don't know exactly what the circumstances were, but I don't think he's driven much since then, as far as I know.

GRAYSON: Yeah […]. He hopped out of […] the car and, you know, greeted me warmly and had me sit in the front seat where he was. Here's a guy that could lord it over us all.

VESTAL: Oh, no, no, no. He hasn't changed a bit.

GRAYSON: No. It's just amazing […] what a down-to-earth kind of guy he is.

VESTAL: He is. You know he and I used to joke early on. I told him the story about when I read this review and somebody said, "[Why do we need electrospray? We already have thermospray]." And I told him, it wasn't me that said that! But no, as I say, I've never felt anything but warmth for John and from John. [laughter] I think he's starting to show a little bit

of sign…last year or two he slowed down noticeably from where he was. <**T: 60 min**> But we're all going to do that one of these days.

GRAYSON: Oh, yeah. I'd finally like to wrap this up with some […] general philosophical questions or topics or what not. So, how do you feel now that […] you've had this great career? How do you feel about the fact that you ended up being a scientist instead of going into some other [career such as] writing, you know?

VESTAL: I thought about that occasionally. I guess it almost seems like it was inevitable. I can't really say I ever made this conscious choice to be a scientist. It just the way my life went. And I think it was the right one. I've never regretted any move I've made. I almost never looked for a job. I took the part-time jobs when I was going to college. I mean, I went from Purdue, and before I finished the degree to working with a small company in Lafayette, which then moved to Baltimore. And I stayed with that until we had this disagreement with the money man, and started another company. And then I went from that back to the university and got my degree and went to another university and then started the other company. And, you know, it hasn't been sitting back and saying, well, should I do this or that. It's just been…just, kind of, went that way and it still does. And I don't think I could really have been happy doing anything, I guess. It's hard to say, you know. You know, the old saying you come to a fork, take it. I guess I did, but I wasn't all that conscious of it—having more than one alternative.

GRAYSON: Well, it seems like as you've aged, instead of looking for a job when you were changing, you didn't so much as look for a job as look for starting another company.

VESTAL: Well, in the most recent time I've been looking to go ahead and do what I thought I wanted to do. I had some ideas that I wanted to pursue. That was very much the case with starting this company. I felt that…and I convinced my wife of it, because originally she was not at all enthusiastic about my forming another company. But I've got these ideas that I really would like to see developed and I don't see any other way to make it happen, other than to go do it, because nobody's going to put up the money just based on my guesswork, even though I may have some reputation. And I don't want to go out and raise money from somebody, because they're going to control what we do, and so I said it was really something I want to do. We can afford to do it, so why not do it? So, that's what we did. Anyway, I suppose I didn't have to do that, but it's kind of the way the machine works, you know. I just follow my nose.

GRAYSON: But most people don't decide to start a company when they're seventy.

VESTAL: Well, I guess, most people don't have that opportunity, right.

GRAYSON: Well, or aren't so inclined.

VESTAL: Well, yeah. I don't know why we do what we do. You sit around and analyze it too much…. This is one problem I had with Henry Rosenstock, and I liked Henry a lot. He was a very good theoretician, but he was one of these people that instead of going off and doing something, he would think about it in great detail, and think of all the reasons why one path wouldn't work or the other path wouldn't work. And he would agonize over it. And my idea has always been, well, if this looks like a reasonable way to do it, let's go try it, and if it works, great. If it doesn't, we'll usually find out why it doesn't work and…

GRAYSON: Try something different.

VESTAL: We'll, yeah. We'll go on.

GRAYSON: That'll educate us.

VESTAL: We'll change the direction that's necessary. That's the way I've always worked in the sense that you figure out that this looks reasonable. Let's see what happens and do it. And okay, well, we overlooked something and I'll fix it. That's about what we've been doing in the company. We started out to do a TOF/TOF with a set of specifications of what we felt we needed to do, that was high resolution precursor selection and multiplexing, and I knew in general how to do that, but you know a lot of the detailed stuff [we're doing] now is different from what we started out [doing; because we've] learned […] a lot in the process. If it'd been easy, we'd have done it in a year, [or] somebody else would have already done it, I suppose. But anyway I have to say I've thought about this a couple of times, and I don't really think I ever made a conscious decision to do hardly anything scientifically. It just, sort of…

GRAYSON: Happened.

VESTAL: It just happened, yeah. Kind of, followed my nose, I guess.

GRAYSON: So, with regard to a young person going into science today, do you have any advice, words of wisdom?

VESTAL: Don't do it for the money. Do it because that's what you love doing. That applies to anything, not just science. [laughter] <**T: 65 min**>. If people really enjoy what they're doing; they will be successful, and they will be able to make a living at it. […] If what you do is

of no interest to anyone else, then okay, you may have a little problem. But if you really can find a way to do what you really enjoy doing, I think you'll be okay. You know, if you look at from that point, rather than what kind of fame and fortune and stuff can I get by doing this, which I suppose some people do that, [maybe] a lot of people do that. It depends on your background, obviously, as well. If I'd been born in the ghetto somewhere, I mean, my attitude might have been different, but I grew up in a family that was comfortable, but not by any means rich. [We] never had to worry about where [our] next meal was coming from. On the other hand, my father was pretty frugal. He didn't throw money around. He'd been through the Depression. You know as I say, he didn't go to high school because his father said he couldn't, and that certainly affected his life.

GRAYSON: Oh, yeah.

VESTAL: It had some sort of subliminal effect on my life at least. I was going to get an education.

GRAYSON: And during your career, the presence of women in science has been significantly increased.

VESTAL: Oh, yeah.

GRAYSON: So, do you have any words or any thoughts on that?

VESTAL: Well, I think presence of women or the role of women in society in general, has […] changed radically. I think that's all for the good. I mean, in fact, the role of minorities in…we no longer have male WASP [white Anglo-Saxon Protestant] culture entirely. Although we may still dominate more than's good for us. […] I can remember in mass spectrometry when we called Catherine Fenselau, who we're honoring today—the Nymphet of Mass Spectrometry. Well, actually, that was her name.

GRAYSON: Oh, that was her name.

VESTAL: I called her the Matriarch of Mass Spectrometry. She said, "No. I'd rather be called the Nymphet of Mass Spectrometry." [laughter] Okay. But, you know, when I first started there was her and this gal, Pat [Patricia] Haug, and I think they were the only two females. Pat was a student or postdoc of Joe Franklin's. And I think they were the only two women in the mass spec meeting. And for the first ten [years], a few more came in but it was predominantly white male.

At the time of the New Orleans meeting in 1962, which was the second one I went to, we had one black member in the Society, and he was denied entrance into the hotel or something on Canal Street there, one of the hotels. And as a result of whatever happened…I never really knew what [it] was, [but there was one] the black member, and he was treated badly, and the Society voted to never go back to New Orleans, until they changed their policies. But, there was [one] black member at that time, but I don't think there were any females […].

GRAYSON: There was this woman by the name of Sibyl [M.] Rock, who worked for CEC. […] Her name's on some papers back in the [1940s].

VESTAL: I don't remember ever meeting her at the meeting. I mean, she might have been there. Because they didn't have an exhibition in those days, so the only people [who] came were the hardcore scientists who were doing [the research].

GRAYSON: I thought you were going to say the hardcore drinkers.

VESTAL: Well, they were that too. [laughter] But, you know there weren't all that many students that came.

GRAYSON: Oh, yeah.

VESTAL: There were some. Of course there weren't that many students.

GRAYSON: There weren't that many students because the whole idea of educating people in mass spectrometry is a [recent development].

VESTAL: Yeah. A lot of the group of people from the chemical companies, and still there weren't that many people from academia at all for that matter. A lot of them were people like Al [Alfred O. C.] Nier, who was involved in other things. But, I would say the Society [ASTM E-14, predecessor of ASMS] was probably half—at least at that time—people that were working in industry. […] Maybe more than that. I hadn't really thought about it, because some of them did move to academia, like, Fred [McLafferty], Frank [Field]. I mean, all those guys started out at Exxon and moved […] into academia. And so, it became more academic as time went on, but not much.

GRAYSON: With regard to the mentoring. You have been the subject of being mentored and you've also mentored in your career. Do you have any words of wisdom on having been mentored?

VESTAL: Well, I already told you where I benefited from. I don't think I was a very good professor or mentor to tell you the truth. I was more interested in <**T: 70 min**> what can we do, and I probably didn't give people much direction. But as you know, I have students…my most successful student is Hee Yong Kim at NIH. […] She gives me a lot of credit, but I don't think I deserve it. She's just a very capable, and very persistent [person]. She just went ahead and did it, and she's still doing it. She thinks I helped her a little bit. I mean, I helped her by finally agreeing to let her be a student. I tried my best to chase her off, but she wouldn't do it, so I had to accept her.

GRAYSON: So, you didn't think […] she was a good match for…

VESTAL: Well, yeah. When she came to see me, she had a master's degree in pharmacy.

GRAYSON: Oh, yeah. That's right […].

VESTAL: I told you about it. Anyway, she knew organic chemistry, but I wasn't…she was in the analytical program. And she said she wanted to do mass spectrometry with me. I said, "Well, I don't know. I guess, let me see your background." Because […] we do these things with all these big instruments, and she's this big. You couldn't get away with that these days. My first reaction was it just didn't fit with her persona and background. And I said, "Well, you really ought to go talk to the other professors and make sure what you really would like to do." And so, she went away and a couple days [later] she came back and said, "I talked to the other professors. I still want to work with you." And I said, "Well, I'm not sure whether I have any money to support you or not." I did have some support for her, but I was doing some other grants. And she said, "Well, it doesn't matter. I still want to work with you." So, she came back a week later and I put her on and paid her […]. Best decision I ever made. But looking back, I can say that I had some good students, and I suppose they got something from working with me, but I can't really recall that I [did] much in the way of mentoring them or guiding them or directing them. I did direct them to some extent. I interacted with them, in doing all this, "What you're saying doesn't make any sense. Do some more work on it." I had one or two really bad students that I had some problems with, but mostly they were pretty good students. Whether they succeeded or failed was more their work than mine I think […]. Since then in the company it isn't really an issue, I don't think. I suppose there was a certain aspect of leadership to it, in terms of providing [direction to people]. I suppose in a sense I mentored Kevin, in that he showed a lot of promise, and it was well worth doing because he made my life a lot easier.

GRAYSON: So, what do you think about the state of competition in science these days? Is it over-competitive, under-competitive, about right?

VESTAL: Oh, I don't know. I think in the instrument scientific business, I think, it's fine. Competition is healthy. I think people are going to compete without [there] being […] a lot of anger or animosity in it. People in different companies are friendly to each other, but there is competition and that's good. As far as in scientific research is concerned, I've never really felt it very much. I suppose someone would say they're more aware of it than others. Who's going to publish first when people are working on the same kinds of things? My own personal experience… it doesn't [matter] much. There's never been an issue as far as I'm concerned. In some cases I've been working on something other people are working on, but I never felt like I was racing against somebody else to do something. I think there are some areas where I'm sure that happens, but it's just not something I've experienced.

GRAYSON: What do you think are the most significant changes that have happened in the field since you got into it?

VESTAL: In what, mass spectrometry?

GRAYSON: Mass spectrometry.

VESTAL: Everything. Electrospray and MALDI. I mean probably, the ionization techniques, are what revolutionized mass spectrometry. Mass spectrometry was really plateauing before those were developed. We were doing all these things. I was doing thermospray and [there was FAB (fast atom bombardment)] people were doing the best they could. […] In the end, big magnetic instruments had been done. They worked great. Quadrupoles had been done. They worked pretty well. And for EI and CI, which was what everybody did, […] there were lots of applications developing. But you could see it. […] You were doing all the molecules you could do. <**T: 75 min**> You could try to do biological molecules, but it was tough, and not many people were really doing it. People like Klaus Biemann were derivitizing and doing MSMS and getting results, but they couldn't convince any of the biological colleagues it was something they wanted to do. It was too hard. And once we got these new ionization techniques and what followed; it sparked a revolution. It went from a few hundred people in the Mass Spectrometry Society to a few thousand. Growing by an order of magnitude. It's all due to biological applications.

GRAYSON: Yeah, yeah. For better or for worse.

VESTAL: Yeah. It's different. But I think biology is the future. I really do think that, and not just for mass spectrometry but science in general. I think it's the most important thing we can do.

GRAYSON: That's definitely a complicated problem.

VESTAL: Well, yeah. It's sufficiently complicated that the more people you can get convinced to work on it, the better you're going to be, because there's plenty of work to be done. And I certainly would like to see more money put into that than, say, sending some people to Mars or something, which I think is asinine. I don't think it'll happen first of all. And second of all, to spend money on it at this point in time, it seems to me, is absurd, because we have plenty more pressing problems to deal with in terms of the environment, and biology, et cetera, and our resources only go so far. And going to Mars is a stunt. There's no scientific [value]. I think the space exploration program with the probes and the unmanned stuff has been very successful and it makes sense, but, you know, the problem with putting a man on these things is not scientific. It's political, and it's a waste of time and money. Even the lunar program…I have to say at the time, I got a big buzz out of it as everybody did. But when you really look back and say, okay, what did we really learn that we wouldn't have learned just by sending a ship up there and taking samples and sending them back, and then we wouldn't have been risking lives in doing it. We learned a lot. It was a tremendous program, and they did a great job in take those people up and bringing them back alive, but people now are talking about doing it routinely and I think that's crazy because, the probability of doing it successfully is not all that high.

GRAYSON: No.

VESTAL: […] We managed to avoid a few disasters. […] A lot of those guys went to school at the same time I did at Purdue.

GRAYSON: Yeah.

VESTAL: [Virgil Ivan] Gus Grissom was a student at Purdue. Neil Armstrong was a student at Purdue at the same time I was there. And I think I may have even shared a class or two with them. I didn't know them. Those guys, military guys hung out together.

GRAYSON: Yeah. So, from your perspective, what do you see as the future of mass spectrometry?

VESTAL: Biology.

GRAYSON: Biology.

VESTAL: MALDI!

GRAYSON: MALDI. [laughter]

VESTAL: MALDI TOF/TOF.

GRAYSON: Now, this isn't supposed to be a sales [pitch]!

VESTAL: I know. But that is […] really my feeling. And it's not widely shared, I don't think. But, electrospray has been wonderful and continues to be wonderful. It seems a lot of problems have been solved that wouldn't have been solved, and it'll continue to be [extremely useful]. I really do think it's […] kind of reaching its zenith […]. And MALDI is here.

GRAYSON: So, why do you see electrospray as reaching a limit?

VESTAL: Well, because I don't see how you can go beyond certain things. [People have developed] some very fancy, very powerful machines, but where are you going to go from here? You've got some inherent limitations. To get high sensitivity you've got to work at lower and lower flow rates, so that means you've got lower capacity and longer separation times. And to get high performance, you've got to take more time to do it. With MALDI, it's much more reliable, […] more large-bore columns, more capacity, and the MS and MSMS is inherently a hundred thousand to ten thousand times faster, and more sensitive and so forth. And we know how to do all that now. […] The problem was back, twenty years ago when the techniques—no, twenty-two years ago—when they first burst on the horizon, doing electrospray was easy. You just bolted it onto an instrument you had and off you went.

The instruments have improved and so has the electrospray, but it was incremental and huge results came out. With MALDI, [there wasn't any] instrument. […] Bob [Robert J.] Cotter did an experiment on the Bendix but it […] set things back rather than advanced them, because he felt like he did delayed extraction and he did sort of, but it was on this Bendix machine <**T: 80 min**>. The results were miserable. And Bruker had a machine that they [had built for] something else, and so they […] fairly quickly modified it. […] Finnigan doped the reflector machine that they had, but there wasn't any established instruments, and it took us a while to get the instruments up to the point where they competed. You know […], the first

instruments we built at PerSeptive only got up to ten thousand resolution with this humongous big machine. We really thought that was really great. That was after doing extraction. You could actually do that before doing extraction, you're talking about two or three thousand resolution maximum and poor sensitivity and terrible signal-to-noise. Delayed extraction really was the first, the first jump. But then, from that we've gone a ways. Now, we can make any resolving power or mass accuracy we want. [laughter] Anyway, that's my personal feeling and you know it's not widely shared.

GRAYSON: Obviously.

VESTAL: Well, see in five years whether or not it will.

GRAYSON: So, basically what you're saying is the ability to tackle more difficult problems is greater with MALDI than it is with electrospray.

VESTAL: I believe so. I mean, I can see a route to doing the whole proteome in less than twenty-four hours with much higher dynamic ranges and so forth than you can do it now. You […] can do that with electrospray to some degree, but what's been tried, [and it] took about four months. Just the way it is. So, I have to prove it to it, but haven't done it. But that gives me something to do in my waning years. Well, we've got it pretty well launched, and I think…we probably are going to make a deal with somebody that'll actually do the other end of that…

GRAYSON: So, when do you think an instrument will come on the market from this effort that you're [currently involved in]?

VESTAL: Next year.

GRAYSON: Next year.

VESTAL: At the latest, probably. Not this year, but it could be. Could be later this year, but we may announce something before the end of the year, but we probably won't be doing any [before then].

GRAYSON: So, by next Pittcon?

VESTAL: Yeah. There'll be instruments on the floor, I believe. In somebody's exhibit. It won't be a Virgin Instrument, but we'll see whose it is.

GRAYSON: Well, unless you have any other comments that you want to say…

VESTAL: I think I said more than I know.

GRAYSON: Then I think we can call this a successful exercise […].

VESTAL: I'm not sure I even want to read the transcript. [laughter]

GRAYSON: Well, you will, and have an opportunity to look at it.

VESTAL: Well, I enjoyed [reading Fred's transcript].

GRAYSON: […] I think other people will probably enjoy reading yours. How many people knew you were thinking about [championing MALDI-TOF]?

VESTAL: A few comments I made, that I might want to edit out. I'm not sure.

GRAYSON: Well, that's why you get a chance [to review the transcript].

VESTAL: Maybe the comments about one or two people.

GRAYSON: You'll get a chance to look at it […].

[END OF AUDIO, FILE 1.2]

[END OF INTERVIEW]

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