An Interview with Elliott D. Bloom
By David Zierler
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ZIERLER: Okay. This is David Zierler, oral historian for the American Institute of Physics. It is October 29, 2020. I'm very happy to be here with Dr. Elliott D. Bloom. Elliott, thank you so much for joining me today.

BLOOM: You're welcome.

ZIERLER: Okay. So, to start, would you please tell me your most recent title and institutional affiliation?

BLOOM: Well, my most recent title is Professor Emeritus of Particle Physics and Astrophysics at SLAC, Stanford University.

ZIERLER: When did you go emeritus?

BLOOM: Oh, it's been almost 5 years.

ZIERLER: Have you remained connected with SLAC, or has this been a real retirement?

BLOOM: The first few years after I retired, I was rather active, but then funding for the programs I was involved with were greatly cut at SLAC for the most part. So, there were no young people there to work with on my astroparticle interests. As emeritus at Stanford, you can't be a PI. You can't even mentor young people formally—informally, of course, you can, but not formally. So, at that point I became less active, but I'm still active at SLAC. I have an office in the KIPAC building at SLAC. Staff are generally not allowed to be on the SLAC campus now because of the COVID-19 plague, but until that started, I was going in. I would go in a couple of days a week in the afternoons and putter around there, talk to people, and attend some meetings and seminars, and I attend faculty meetings on occasion or retreats on occasion. So, I'm still somewhat active, but my activities on astroparticle physics have waned over the past few years.

ZIERLER: Well, Elliott, let's take it back to the beginning. Tell me a little bit about your upbringing and your family background.

BLOOM: Well, I was born in New York City, and my parents were both immigrants to NYC as young people. My mom's father came before she did by some years, and then he went back to Poland, but he was already a U.S. citizen. Then he decided it was getting too bad in Poland in the '20s and came back to NYC with the family.

ZIERLER: Too bad for Jews?

BLOOM: Yeah. Well, it was generally difficult, okay, but yeah, particularly for Jews. So, he came back and then he brought the family over, his family which had… Let's see. My mom had

4 siblings that came, plus her mom. My grandfather started from scratch and worked his way up to the middle class as a butcher in the Lower East Side of Manhattan.

My dad had sort of a rather unique background. My dad was born in what was then Palestine in Jerusalem, under the Ottomans', and his parents were also born there. I believe his ancestors came there from Western Europe maybe in the late 19th century. His father came to the United States a few years after WWI ended. After he became established here, he sent for the family, and that was probably in early1920s. My father and his siblings—he had a number—and his mom took off for the United States, but then they got hit by a cholera epidemic in Turkey. All of them died except my father and one of his brothers, his older brother. They were sent back to Palestine. They lived in Jerusalem with, an aunt and uncle who were very poor, and they had kids of their own. They had to… at some point… kick my dad and uncle out and send them to an orphanage, which was very tough. My dad told me about some of the things that he had to do to survive.

In 1930, when my dad was 16 and my uncle was 17, they came to the US sponsored by a relative of my dad's father. My grandfather somehow was absent in talking to them and such and keeping contact, but at some point, he helped get them into the United States. On arrival my dad didn't have a high school education, and he had to go to night school while he was working full time to get his—what is it called nowadays? — high school diploma.

ZIERLER: A GED.

BLOOM: Yes, a GED. My dad did very well in business in LA over the longer term and died at the age of 93 a very wealthy man.

ZIERLER: So, your parents met in New York.

BLOOM: My parents met in New York, and my dad went into the piece goods business as a worker for a number of years. He did well there, but then my mom felt that he could do a lot better in California after WWII. My dad was drafted very near the end of the war but finally did not have to report. This was timing that was just fortuitous, but it had to do with how many children…you know, how well we were doing in the war and how many kids the draftees had in his draft board's region. There were a lot of potential draftees reporting to his draft board. At that point we lived in Brooklyn.

ZIERLER: Where in Brooklyn?

BLOOM: In Bensonhurst. We lived in an apartment in Bensonhurst.

ZIERLER: Do you remember Bensonhurst? Were you too young?

BLOOM: A little bit, yeah. I went to kindergarten and part of 1st grade there, so I vaguely remember those years, some of the incidents that happened. My parents didn't have a car, of course, but my dad was doing reasonably well. Then my mom urged him to go and try to do better in California. I think it was 1947.

He came out to California and got established on some level, and then he sent for my mom and my brother and I, I think I was six years old, so it must have been the spring of '47 that we arrived in LA (my 3-year-old brother, Stan, mom and myself) on the train, which I remember well. [Chuckles] It was interesting. Then we wound up living in Boyle Heights on the east side of LA in a lower middle-class neighborhood.

We occupied one-room of a two-room apartment with a bathroom and a kitchen, and the other room was rented to another renter. They had entry through our area. My parents separated their bed with a curtain, and my brother and I, as I remember, slept on cots in the same room, but on the other side of the curtain. I went to public school in Boyle Heights and had no problem doing well in school -- New York had very good schools. California, of course, at that time had one of the best public-school systems in the country.

ZIERLER: Elliott, when did you start to get interested in science?

BLOOM: Let’s see. Probably when I was about 12 or 13. I read *One, Two, Three…Infinity*.

ZIERLER: So many people talk about the influence of that book.

BLOOM: Yeah. I was fascinated by that. So that was at a time… Let’s see. It must have been 1952-3 or so, and there was a lot of interest in the country in science. Science had won the war for us. I also got into sports, and I was a gymnast at a young age. I started when I was in middle school, and I had a number of friends who I worked out with. We had a gymnastics coach, our gym teacher, Mr. Abe Lober, who was a great role model and was quite influential in my up binging in middle school. In those days, if you did something wrong in class, you got whipped. [Chuckles] You touched your toes, and he had a lanyard that he kept around his neck. You’d get a couple of lashes. So, he kept the class in good order. We had some bullies, but not very many. Besides interest in gymnastics, my friends and I had similar intellectual interests. Our interests were broad, and all the guys were pretty smart people. I thought I was the dumbest.

So that just developed through my middle school years, and in high school, we all went to the same high school, which was Fairfax High in west Los Angeles. My parents moved from Boyle Heights I think when I was probably around 8—they were already in LA a year or so—I must have been 8 when they moved to west LA. We actually experienced some anti-Semitic activity. There weren’t very many Jews living in that area at that time. I know my brother and I were personally attacked by some young neighborhood hoodlums, and our family had other incidents. But we just pushed on and it didn’t really affect me much as I remember. My parents were much more affected by these events than I was.

ZIERLER: Elliott, did you have a good physics teacher in high school?

BLOOM: No. I did not have a very good physics teacher. In principle, he should have been good. He was a past professor from somewhere, Dr. Gray, and he looked like a physicist as portrayed in the movies or something. [Laughs] No. In my high school years, there were a few things that made a big difference. My brother Paul was born when I was 15. My high school had an excellent chemistry teacher, Dr. Marvin Greenblatt, and we were regularly winning APS competitions nationwide, I believe that Greenblatt’s classes did very well in in those. Then I had an outstanding math teacher in the tenth grade who really made a difference, Dr. Hyatt. I initially wasn’t doing too well in algebra. I had to take algebra over again from middle school—junior high, we called it. In 10th grade I was taking algebra class and geometry at the same time.

I just did it. I got A's and all of a sudden, I learned more discipline. These teachers were fantastic, Dr. Hyatt. Dr. Greenstadt.

Then there were my friends, many of whom were on the gym team. Sports were very important. Also, I had an interesting and fun social life in the last two years in high school. I was a member of a social club, the "Barons of West Hollywood", and met many interesting girls and boys though activities in that club. I was a thespian in high school as well and appeared in a number of plays and student reviews.

I believe my music education was also important to my development. I took lessons on the trumpet. My parents wanted me to learn an instrument and started it in grammar school, which was then available. My grammar school loaned me a trumpet to begin with. As I got more serious, my Trumpet teacher, Mr. Nadel, got me into Peter Meremblum's Junior Symphony Orchestra in Hollywood, which was really quite a thing. During my two years in that orchestra, I was conducted by Stravinsky, Sir Adrian Boult, and fell in love with classical music.

ZIERLER: Wow!

BLOOM: Mememblum invited professionals that joined in with the kids.

ZIERLER: Did you ever consider pursuing a career in music?

BLOOM: I did briefly consider a career in acting. I was a thespian in high school, and I did a fair amount of acting in productions in college. But no, I really wanted to be a physicist. Physics was my thing! —I don't know what you'd call it—OCD or something. [Laughs] I had this really strong desire to understand and do physics. Later in high school they gave us the Iowa test. In those days, you know, one had to take the Iowa test, and the results were public. I mean, you got your results and you shared it with people. I did so well. My friends were flabbergasted. How could this nitwit do so well? [Laughs] Actually, my friendships lasted. I still have three high school friends that I regularly communicate with. Fairfax was a high-powered high school. 11 of my classmates went to Caltech in my graduating class from Fairfax, and we had a very high percentage of students that went to colleges overall. I also attended college. I had a choice. I was accepted at U.C. Berkeley, but my preference was Pomona College. I went to Pomona College and that was a great decision.

ZIERLER: You wanted to go to a smaller school.

BLOOM: I thought I'd do better in a smaller school, that I needed more personalized attention, and it turned out it was true. I really didn't have the necessary discipline yet, and I was way over committed my freshman year (two orchestras, freshman football team, thespian, too many units). My freshman year, I had a roommate who still is my best friend, John O. Beahrs. [Laughs] He lives in Portland, OR. He's a psychiatrist and retired. John was a professor at Oregon Health and Science University and is now emeritus there. He also worked at the Portland VA for many years. We are really just copacetic, you know. Came from very different backgrounds. He is Episcopalian, a fellow born and raised in Palo Alto. We just have a lot of things to share and have a great time together. We got in trouble at Pomona our sophomore year with pyrotechnics. [Chuckles]

ZIERLER: Elliott, did you declare the major in physics right away?

BLOOM: Yeah.

ZIERLER: So, you knew you wanted to pursue physics.

BLOOM: Yeah. I majored in physics. I minored in ancient art and archeology.

ZIERLER: Oh! That's an interesting dual curriculum.

BLOOM: Well, I liked art. I always enjoyed and I did well in the art classes. I liked the general education Pomona offered in those days in Western civilization, which was required. I had to take a language and, you know, a broad expanse of courses. For my ancient art and archeology minor, I took a number of classes and I had to go into the depths of the school library down in the basement and get an old book out on this ancient Greek temple that I had chosen to write my term paper on. It was an interesting experience. The math and the physics were easy by then for me. I was getting all very good grades, and--

ZIERLER: Elliott, at what point as an undergraduate did you start thinking that you might be more interested in theory or experimentation?

BLOOM: Well, I always had an interest in both, okay? But I had the great luck of having a professor, Dr. Ogier, who was an experimental physicist. The new Millikan physics building at Pomona College was completed when I was probably a freshman or sophomore. It was completed and there were unique opportunities there. There was a cyclotron in the basement, a little cyclotron, and he did research with that. So, I teamed up with Dr. Ogier and I took the cyclotron apart and I sprayed vacuum pump oil all over his pants. He was very patient. I machined proton beam sources for the cyclotron out of solid carbon. In the process, I learned how to be a competent machinist, and I just thought it was wonderful. Also, Millikan lab had a Bendix G-15 computer which I used to learn how to code and to analyze some of the data. It was all done with punch tape. You had to code in machine language and put the code in your punch tape. So, I really got a tremendous education on how to do experimental physics. When we got data, we actually bombarded protons on atoms, I'd try to analyze the data from observing the emitted products. I learned about the theory of what kind of emissions would come from that scattering and compared with our data. So, I was very well prepared as an undergraduate. I took a lot of physics, took a lot of mathematics. I also worked there the summer after my junior year. I got married early. I met my wife, Sue, when she was 13 and I was 17, and I couldn't really go out with her. She was the sister of one of my good friends. That's how I met her, and then I was really taken with her and apparently, she with me. Anyway, as time went on, after she turned 17, we started dating seriously, and we were married within six months. That was my best luck in my life. She had to get her parents' permission, and I was 21. So, she was 17 and I was 21. Sue worked to help support us, and we were married for my last half-year at Pomona. I applied for doctorial work in physics to Berkeley (on my honeymoon in Las Vegas) and then I applied to Caltech. I didn't have an interest in going far away and going to Harvard or Cornell or places like that. So, I went to Caltech. Some interesting things, like when I first arrived Caltech was just a big step up from Pomona, so I went there… I wanted to tell you this story. I went there to get a summer job, and I interviewed with Dr. Matthew Sands, Oh, geez. He was a famous guy. He became deputy director for the construction and early operation of the Stanford Linear Accelerator Center. He later went to UC Santa Cruz. He was, Vice Chancellor for science at UC Santa Cruz. He was very happy to accept me for a summer job almost instantaneously. Anyway, so I had a summer job there which was, again, different, totally different area working with a professor who didn't wind up being my thesis professor ultimately. I learned a lot from that experience.

ZIERLER: Elliott, was there a particular professor that you wanted to work with at Caltech?

BLOOM: Well, I just knew Caltech—in particle physics, which by then I knew I wanted to do—was one of the great places in the world, and they had an electron synchrotron there that I could do experiments on. They had Feynman. They had Gell-Mann. They had Zachariasen. They had Frautschi. They had this incredible group of theorists. They had great experimentalists who had been involved, actually, many of them, in the Manhattan Project.

ZIERLER: Who ended up being your graduate advisor at Caltech?

BLOOM: My advisor was R. L. Walker, Robert Walker, who when he was a graduate student actually worked on the Manhattan Project, which he told me stories about. But then I had another advisor who was German. He came from Germany as a post-doc to Caltech and he worked with Walker and then he effectively took on a number of Walker's students. His name was Clemens Heusch. Actually, he had five names, but we called him Clem. He spoke a lot of languages, a very educated person and cultured in many ways. A very hardworking guy. He gathered a group of students around him who later were called the Heusch puppies. So, I spent a lot of time with this group of exceptional young people who turned out to be professors at major universities and did very well in their careers. My wife was working during this time supporting us, but I had a stipend from the DOE at that point. It wasn't taxed, so we actually were living quite well. Financially It wasn't so difficult. My wife was earning a reasonable living, and I got this extra money that was tax-free. So, it enabled us to have a decent life. We didn't have children at that time. As I said, we were single. [Laughs] Walker would be a constant influence. I'd see him once a week at least.

ZIERLER: What was his research when you met him? What was he working on at that point?

BLOOM: He was working on gamma ray production of charged pions from hydrogen or deuterium (neutrons), and he used a big spectrometer which he had designed and built along with some of his graduate students, and Caltech staff. He also was involved in the maintenance of the Caltech synchrotron, which was a finicky beast. Clem was sort of new to that, but in his group, we built all our equipment from scratch. My experiment was built based on the first experiment that was built from scratch by all of us. Charles Prescott, who later became a professor also at SLAC and Stanford…

ZIERLER: Right.

BLOOM: …his thesis was first, and we all helped to build his experiment. Then mine was second, and then Leon Rochester was third and then Bruce Weinstein, …. They went down the line with different experiments. It was a somewhat modified apparatus that was put together for each experiment. Clem was made a professor after a number of years, assistant professor at Caltech. Then he was made associate professor. He didn't stay there; he went to UC Santa Cruz at some point after I left.

ZIERLER: What was Walker's style as a graduate advisor? Did he essentially hand you a problem to work on and that became your dissertation, or you were more independent in developing your research?

BLOOM: No. What he did was set me up with Clem, and then Clem decided what we were going to do (I guess in consultation with Walker). Charlie was photoproduction of etas. Mine was polarization of protons in pi-zero production at the second nucleon resonance. Clem would look into what was the possible interesting physics…and he talked to us about it. I had an interest in all of it. I thought it was all great. I never had thought I'd be able to do stuff like that! [Chuckles] Our small group was quite successful. There were some stressful times. In my case, the accelerator died. The electron beam source got poisoned and I had to learn about "electron guns". Walker came to help, and he was an expert, and I would be his assistant, his sous chef. We were working on the gun and got it going. It could have been a delay of a year for me alone to make that work, and Walker quickly saw the problem and made it work. Mainly due to Walker's expertise we made it work in a month. So, I got my PhD done in about 4.7 years, which at Caltech was almost the record for particle physics experimentalists. [Chuckles]

ZIERLER: Elliott, looking back, what were some of the essential conclusions of your dissertation research?

BLOOM: Well, it was hard to make conclusions. I did in my thesis discuss the theory of this process as far as I could take it. There were Regge poles involved and some things like that that happened to be popular at the time. But it was very hard to make a firm conclusion. I made a phenomenological model that sort of fit my data and other data, and Walker was happy with that. He thought that… He's the guy that finally approved and said, "Yeah, that looks pretty good." But in addition, he helped me a tremendous amount with writing the thesis because my writing ability was not the greatest and he would just give me great tips… That was an important experience on how to write something complicated. Clem didn't let us write our own papers. He would take it over, and then we'd go over it together afterward and publish. The author list on my thesis paper[1] had my name first, and the rest of the contributors in alphabetical order, okay, so he didn't take extra credit for having done that work. He was very cognizant of promoting his students, and Walker was, too. But actually, my first paper at Caltech was theory. I wrote a theoretical paper[2] with Boris Kayser, who you may know about.

ZIERLER: Sure.

BLOOM: He's a well-known theorist nowadays and he's retired from Fermi lab. Boris was head of NSF particle physics theory for many years. We were also friends for many years, not so much anymore as we're separated by a long distance. But we were good friends then.

ZIERLER: Elliott, who was on your committee? Who was on your thesis committee at Caltech?

BLOOM: Well, there was Walker and there was Clem Heusch and I think Robert Bacher was on my committee. My thesis results were published in Physical Review Letters[1].

In my third year at CIT, I worked with Boris on this bootstrap calculation from which I learned a lot about bootstrap theory at that time. The bootstrap method was a popular theoretical technique in particle physics. This paper had Boris's name first as he was certainly the senior guy. Then I also had ideas about what turned out to be quarks because I was living in Quarkville, and Gell-Mann was of course talking about the Eightfold Way. As Gell-Man told us in his advanced particle physics class, Quarks were mnemonics and they were too heavy to really be produced, but they're very useful mnemonics. But I took them more seriously—you know, concrete quarks made of concrete.

So, I had some ideas and then I talked to Feynman about them and he encouraged me for a while. But he asked me to go and do things that I had no idea what he was talking about, and he didn't really tell me, "Well, read this paper. Why don't you read this paper? Why don't you… This idea may fit in with that." He just said, "Oh, it's good. Why don't you go off and come back? The quantum numbers—try to relate to some quantum numbers…" and I was thinking about old-fashioned quantum numbers, not SU(3) and things like that. After a while he said, "That's a pile of crap." [Chuckles] But I had the experience of knowing him reasonably well, which was very interesting, and Gell-Mann also and other theorists there, Frautschi—really great physicists, you know? I learned a lot from Feynman about the scientific method and what you can believe, what you can't believe.

So, to this day, I tend to be a very strong skeptic about anything in science that I hear about until I see proof that's really strong, and that only failed me one time that I wasn't skeptical enough in my career because it got out of hand. There was too much hubris involved. But skepticism generally was the correct approach and it wound up that a lot of flashes in the pan went away because it's just easy to make mistakes. So, my education at Caltech was extremely valuable and hard.

A person I have to give credit to when I started there was Jean Swank. She was one of the first women graduate students in physics at Caltech. After graduation she went into astrophysics, and later in her career she was the P.I. for RXTE, which was a major NASA scientific X-ray telescope. She worked at Goddard Space Flight Center for much of her career, a very well-known astrophysicist. RXTE was an important satellite. She was very smart and disciplined. I was having trouble getting through some of these things, and she would show me how to on certain things. After a while I caught on and was able to do quite well. It took a while, but the amount of horsepower you had to put out compared to my undergraduate years was very significant. [Laughs]

ZIERLER: Elliott, what year did you arrive at SLAC?

BLOOM: I arrived in 1967. That was in about May. Again, Walker made that decision for me. I had a number of offers. For the first time in my life, I took an airplane ride with my advisor, Clem, to New York to the New York meeting of the APS in January'67, and that was before I got my degree, but it was clear it was going to happen very soon. I got introduced to Panofsky at that meeting as a possibility of going to SLAC. We also visited Cornell, and that was an interesting trip because we took Mohawk Airlines. There were a number of stops along the way, and taking off from one of the stops, one of the engines was going [imitates engine noise] and the airplane started to lose altitude, you know, and Clem and I looked at each other. We were wondering, and finally the thing caught, and the pilot managed to pull the airplane out. [Laughing] I gave a presentation at Cornell, also at Berkeley…

ZIERLER: But it was Walker who encouraged you to pursue SLAC.

BLOOM: Definitely. I also received offers from private industry, which was tempting. Bell Labs made me an offer.

ZIERLER: Right. That must have been tough to turn down Bell Labs.

BLOOM: Well, it was a lot more money for us and it was a great place, but Walker said, "SLAC is a new machine. Go there. You're going to do great physics." So, I listened to him and he was right. [Laughs] And my wife, Sue, loved it. She would have had a hard time at Cornell on the East Coast.

ZIERLER: Sure.

BLOOM: We visited SLAC in the early spring. I gave a talk there and it was just so green. It was beautiful, typical Palo Alto springtime, and Sue said, "We've got to live here." [Laughs] So of course that's the only person I listened to.

ZIERLER: That's right! What was the first project you joined in on when you got to SLAC?

BLOOM: Oh. I got hired by Dr. Richard Taylor. He came down to Caltech. He talked to me there. We hit it off very well.

ZIERLER: Now did Taylor and Walker… Was that the connection to Taylor, from Walker?

BLOOM: No. No. Dick knew Walker, but I don't think he ever worked with him. He was a lot younger than Walker, and they had a very different background. Dick was Canadian. He went to Stanford as a graduate student. I think he got his degree with Robert Mosley at Stanford on the HEPL linear electron accelerator. Pief Panofsky was involved. But no, Dick had a great respect for Caltech. I mean he really thought that Caltech was the cat's meow, and he tried to hire from there when he could, as I found out later on. He did have a close connection with Barry Barish and Charles Peck who were professors at CIT working with Taylor at SLAC and they both knew me.

ZIERLER: What was Taylor's research at that point? What was he working on?

BLOOM: Oh, he was working on building the spectrometers in End Station A at the end of the SLAC linear electron accelerator to do elastic and inelastic electron scattering and e^+/e^- scattering comparisons, and the head of the group was actually Pief. This was Group A at SLAC. Pief was the SLAC director and the group leader for Group A.

ZIERLER: And so, Panofsky was really involved in the day-to-day for Group A.

BLOOM: No.

ZIERLER: He wasn't.

BLOOM: Taylor ran it when I got there. I don't know what happened before. But in '67, the machine hadn't started up yet when I got there. It started up the summer after I arrived. (I arrived in May that spring.) It began late that summer, early fall. No, Taylor was running it day to day. I'm sure he talked to Panofsky. I didn't know about the details of that. I mean Panofsky was director. He had tons of things to do. Dick was really in charge of Group A. He made the hires. I met Panofsky and I had to give a seminar before I was hired, and I remember Joe Ballam. I don't know how I heard this, but Joe Ballam said when I was applying, there were apparently a number of applicants and then Joe said, "Look. We should give it to an American boy this time." [Laughs]

ZIERLER: That's great.

BLOOM: So, it must have been pretty close and sort of an affirmative action hire. [Laughs] So I went right to work, and Dick said, "Okay, you're going to work on deep inelastic scattering. Here are some of the problems and some of the things we've got to do," and Jerry Friedman from MIT worked with me.

Then I got involved in the electronics because I had a strong electronics background from my work at Caltech. Henry Kendall had built from scratch an electronic system to trigger the spectrometers and take data, and I built a trigger system using commercially available LeCroy modules, with some EG&G modules as well. I forget some of the details. So, I put together a totally separate trigger system, with Henry's approval [chuckles], and then he had a redundant trigger system, so we checked for possible trigger errors/bias. We had two independent sets of electronics in the trigger, so we made sure that we weren't having some systematic problems and we could check one against the other.

I also became involved in the online computing. We had an SDS 9300 online computer. It was the largest and fastest online computer available at that time. It was not a Dell or a… PDP kind of machine, which was quite common by that time, like a PDP-10 or something for the online. It was a much more online-oriented machine, and it had direct trigger aspects to it that you could program and many levels of priority in the trigger. The coding was done in FORTRAN, which had connections to some of the hardware features of the machine, so it was a very advanced machine for the time. It still filled half a room. So, I did work on the online as well and tried to put together more friendly online displays in this initial work. Then after the elastic ep scattering finished there was also a run on the positron-electron ratio in scattering, Caltech was involved in those with Barry Barish and Charlie Peck at that time. But then the inelastic scattering looked uninteresting to them and they withdrew. The theorists thought the same—except for one named James Bjorken (bj)…

ZIERLER: Of course.

BLOOM: But most of the theorists thought it was going to be very uninteresting and just show radiative tails of resonances in the cross sections. But bj was already talking about how interesting the inelastic scattering could be. However, he spoke in tongues, you know. Bjorken was using some pretty high-powered current algebra mathematics to get to the idea that there could be point-like structures inside the proton. I came from Caltech, so I was a quark guy. I just expected we'd find quarks because that was what Murray said. [Chuckles] Feynman also believed it would be a big cross section, actually, but then we were very much a minority of people. But it was my history at Caltech that influenced me.

So, when we started observing these big cross sections, there was a lot of excitement that we were maybe seeing point-like substructure, because if you see big cross sections at large scattering angle that is a natural interpretation. If it was just radiative correction tails of resonances you expect small ones like in the elastic ep scattering at large angle, it's zilch. As you go to large momentum transfer squared, q^2, to the proton and it elastically scatters, there's a very strong fall off in the cross section. It's very fast. But we were seeing things that were depending very slowly on q^2 in the deep inelastic, and this was before we knew about bj scaling. We were having problems with the radiative corrections, and at MIT Henry was working on that. Henry was the radiative correction guy. At SLAC we focused on the data analysis. I was deeply involved in the data analysis of just getting the data out before radiative corrections. I had a partner, Dr. Jurgen Drees, who was post-doc from Germany, working closely with me in Dick's group.

I remember in those days, you had to do something extra special to get your computing done optimally. You had to be at the computing center with your punch cards and you had to place them in the window. You'd wait until the computer operators collected the cards and ran them and then you looked at your output and then you figured out what mistakes you were making. Then you fixed it and then you had to go and put the corrected punch cards in to run all the tapes again. You had to be there to put your cards in and then get the output, change the cards, and put them in again. So Jurgen and I were there until three in the morning every day, you know. We were just working our butts off to get the stuff done, and Dick was very appreciative of this. He remembered this.

Also, I remember that I talked a lot to Dick about quarks. After the raw data was analyzed and radiatively corrected, Henry Kendall talked to bj, and Henry Kendall showed him the radiatively corrected data. Jurgen and I hadn't seen it yet, but Henry took it to bj and bj said, "Plot it like this, Henry." So, Henry plotted it… Well, actually the way it happened was Henry said "I got the data radiative corrected. What should I do with it, bj?" bj said, "Plot it like this. Plot it as ν/q^2 (where ν is the energy transfer to the proton) verses νW_2 (where W_2 is the inelastic form factor of the proton we were primarily sensitive to). Those are technical terms in the deep inelastic scattering world. So, Henry plotted the radiatively corrected data that way and all the data closely followed one curve for different scattering angles and incident electron energies, which in principle should give you different curves. If you plot it in these variables, from all the data we took you've got all the points lying very closely on this single curve, which was bj scaling, deep inelastic scaling.

Soon all members of our collaboration (MIT-SLAC) found out about this and it was clear to us that there were quarks (point like substructure of the proton) …once we saw that curve[3]. At some point in late summer 1968 Feynman came to SLAC to see the data. Most of the people were away at the Vienna conference where the bj scaling was announced in a talk by Pief, so most of the senior people were away, including bj.

ZIERLER: The fact that this attracted Dick Feynman suggests this was a really big deal.

BLOOM: Yeah, he was very interested, and I actually presented the data to him because nobody else was there that knew more about it than me. I was an expert on the data and so I presented it to him. We had a few of us at my talk, and then Feynman famously went off at the end of that day to his motel. He listened to my presentation. He went off that night to his hotel, his motel room, came back the next day, and wrote down the Parton model for us. "This is what's happening!"

ZIERLER: That's how it happened. That's where the Parton model comes from.

BLOOM: That's where it came from.

ZIERLER: Did he give you, Elliott, any indication in real time when you were presenting this to him? Did you see the wheels going off in his head?

BLOOM: Well, I saw wheels going off in his head, but I didn't know where they were going.

ZIERLER: Yeah. Yeah.

BLOOM: I had no idea. The way I thought of the quark model was different. I thought of it like we're scattering off a nucleus and quasi-elastic peaks, you know? If you electron scatter off of a carbon nucleus, you'll see a quasi-elastic peak in the cross section corresponding to the proton-neutron mass, the nucleon mass. So, it was an enhancement because there's a nucleon substructure there which you can pick out, and that's how I thought about quarks. They're little things in there and then you scatter off of them and have a quasi-elastic peak, but no. Feynman did the Parton model, his infinite momentum space, and it's a different approach to get the scaling to come out naturally.

Interestingly, I know Professor Sid Drell thought about it similar to the way I did because sometime later I was invited to give a talk at Caltech. They asked me to come down and give a talk. So, I wasn't going to present Feynman's way; I was going to give my (and Drell's) way. So, I talked to Drell about it and he said, "Oh yeah, that's very reasonable." That's what I presented in my colloquium talk at Caltech and Feynman went off. He just blew up. He was in the front row. "That's ridiculous! You can't… That isn't the way it is. It's like radar bouncing off bees. That's what it is. It's not…" Okay. So, I didn't do that again. [Laughs]

ZIERLER: Elliott, of course bj had access to the same presentation as Feynman, but he did not come up with the Parton model. What do you--

BLOOM: No, no, no. He was away.

ZIERLER: Oh, he was away. He wasn't there for this.

BLOOM: He was away. bj was not there when Feynman was there. Yeah, he was away. However, in any case bj thought about it very differently. He told us how to plot the data to find the scaling based on his mathematical reasoning. Feynman saw the scaling and explained it more intuitively with the Parton model.

ZIERLER: Right.

BLOOM: Okay? bj was much more formal in his approach. His mathematics was more formal. And he did not know the answer; he predicted scaling with no foreknowledge.

ZIERLER: Right. Right.

BLOOM: The deep inelastic scattering program continued for a number of years and we found out empirically… One of our graduate students, Guthrie Miller[5], found out that if you used s/q^2 (s is the $mass^2$ of the final hadronic state resulting from the inelastic scattering) instead ν/q^2 things worked better, okay? So, you got a better bj scaling. The curve was really tight with the data we had. I thought about this, and I had heard a discussion of duality from a guy from Europe who came out and gave a talk at SLAC and talked about the possibility of, as I remember, resonances being connected somehow to the scaling curve. But it didn't work for him for some reason, the data did not line up right. Then I also thought about this in the context of the work I did with Boris, I knew about finite energy sum rules. So, I wrote down a finite energy sum rule for the process which integrated the scaling curve to the resonances using our new scaling variable, s/q^2. I formally derived this, but you know, I was relatively a neophyte theorist, and my arguments were not complete.

I took it to a friend at SLAC, Fred Gilman. I talked to him about it, and there were errors in my work. He looked at it and said, "This looks like a really good idea." Later on, I found he was working on similar ideas, which I didn't realize at the time. Then we worked it through and then I showed it to bj and bj said, "Well, how about this?" There was some subtle problem with the limits, and so he made some suggestions. I made the derivation more formal, corrected those problems, and then we preprinted it and wrote the series of papers on the Bloom-Gilman sum rules[5]. That was probably my most well-known theoretical paper (and probably was a deciding element in my getting my early tenure at SLAC). Much later on, the subject came up with bj and he showed me his notebooks and he had it all. He had it in his notebooks! [Laughs]

ZIERLER: He did.

BLOOM: He just didn't think it was somehow provable or had enough behind it to actually put it forward. I don't know exactly why. Also, he didn't like to write papers. He is a very taciturn individual. [Laughs] Anyway, I thought that was something. When Jefferson Lab started up it turned into an industry at Jefferson Lab, as it turned out, showing that the Bloom-Gilman sum rule worked in great detail. So those were really great times.

While I was an assistant professor at SLAC, I spent a sabbatical year at Caltech in 1972-3 as a visiting assistant professor. At that time, we had two kids. My wife was pregnant when we went to SLAC. She wanted to have children as soon as I received my degree, and what she wants she gets. Our daughter, Wendy, was born in Stanford Hospital in the fall of 1967, and then four years later, our son, Matthew, was born, but he had a serious heart problem, Tetralogy of Fallot. While on sabbatical Matthew still was very ill from his heart condition. My wife was living in

Pasadena, but I had to travel to do experiments at SLAC. I was finding out what was it like to be a particle physics user. I was in nirvana being employed by SLAC, but I had to get a tenured job. I was an assistant professor at the time based upon my previous work, and I was worried about tenure. So, I went to Caltech and there was a possibility of getting tenure there, and I had some interesting interactions with Feynman and others. But basically, my wife hated it. I mean, she found it very difficult. Compared to Stanford and the SLAC environment, she found it unwelcoming. She found people were not nearly as warm and friendly. She just didn't have the kinds of support at Caltech that she had at SLAC—and I didn't either, frankly. It just was a different kind of faculty/staff culture.

ZIERLER: Elliott, what were your prospects for tenure there? What did you think your ability to get promoted at Caltech was?

BLOOM: You mean to make associate professor? I don't know. I think that my family's situation was so difficult, and I was away so much because of my user activities. I didn't have a lot of chance to develop independent work. I taught undergraduates at CIT and one of my best students, Tom Himmel went to SLAC as a graduate student, and ultimately became professor there. I just don't know if they wanted me to stay, but there were people there that didn't like me very much, and who were very vocal about it. So, I don't think it would have been very good if I did get an offer. But somehow, my year away got SLAC into action and they gave me early tenure. [Laughing] That was soon after we returned from my sabbatical.

ZIERLER: So, it was worth it.

BLOOM: Yeah. It was worth it. I didn't know what was going to happen when I started, but…

ZIERLER: Elliott, what did you do when you returned to SLAC?

BLOOM: As an associate professor, I became much more independent. I mean, I went off and started something new. Previously, I had worked closely with Taylor or Professor Joe Ballam (associate director for research) as a junior faculty. My work with Ballam was BC-42. That was an interesting bubble chamber experiment. When I was assistant professor, I was spokesman for BC-42 trying to see if we could knock quarks out of the proton and see them in a bubble chamber using an incident muon beam scattering from the liquid hydrogen in the rapid cycling bubble chamber. A good review of the goals of BC-42 and the open questions of the time can be found in Science News[6] (BC-42 was featured on the cover of the magazine). The short answer is that we did not see quarks knocked out or hadronic jets (in hindsight the incident muon beam energy was way to low), but our results were consistent with the quark model of proton substructure.

But when I said, "Okay, I'm going to do something completely on my own." Taylor was pissed at me. Joe was pissed at me.

After some thought about different directions, I started a Crystal Palace project which turned into the Crystal Ball. First, we called it the Crystal Palace and I got together with… Again, it started with friendships—Don Coyne and Charlie Peck, who I knew forever at Caltech and who I had tremendous respect for. So, Gerry O'Neill's group at Princeton, that was Don Coyne. He was the representative from Gerry O'Neill's group. Then there was Robert

Hofstadter on campus, who was very interested in sodium iodide and helped push this idea and was an expert. I mean, his group had a lot of experience with sodium iodide. Then we got Karl Strauch at Harvard to work with us.

ZIERLER: Right.

BLOOM: So, we organized this collaboration through a workshop, a PEP workshop, one of the first PEP workshops (PEP was a large e^+e^- storage ring that was built at SLAC in collaboration with LBL and completed in 1980). We got together and got the idea of building a multipurpose 4π detector out of sodium iodide with special capability for gamma rays. Our goal was to use this detector at SPEAR at SLAC first, and then maybe PEP. I pushed this quite hard at SLAC and had support from my collaborators and they were pushing it at their institutions. Ultimately, there was a shoot-out with an LBL group in front of the SLAC experiment selection committee. LBL had different design for a multipurpose detector called Mini Mag, and we won that competition. I was the spokesman for Crystal Ball. We won the shoot-out, and Bernard Sadoulet of UC Berkeley never forgave me for that. He just was very upset on losing that decision.

ZIERLER: Why? What was so upsetting to him, do you think?

BLOOM: Well, he lost. I mean, he was part of Mini Mag and they wanted to build it and they thought our stuff was inferior and we'd never do anything. They lost the competition when they attacked the ability of our Crystal Ball detector to accomplish important goals. My secret was I had people working with me who were very wise—Karl Strauch, Charlie Peck particularly—and they taught me you don't go after somebody else's experimental capability.

ZIERLER: Yeah.

BLOOM: You just push your own experiment, okay, because you don't know really what their experiment can do; you know what your experiment can do, and that's the way the committees decide. That's what we did, but the Mini Mag folks went after our experiment. Then I showed them how they were wrong. They attacked us. They didn't know what they were talking about completely, and I was able to put together a presentation to convince the committee that they were incorrect, which they were. Considering it now I don't know, Mini Mag might have worked better; I have no idea. But we did a good job, I think, and so we built the Crystal Ball.

How did we get to the design of the Crystal Ball? Well, Crystal Palace was too expensive, so we had to figure out a way out of it. So, I went to Caltech to talk to Charlie Peck. These are stories I actually told at Charlie's memorial symposium (May 11, 2017). Charlie passed away a few years ago, and I was invited to give a talk there and I told this story. I went to Caltech to meet with Charlie and we were brainstorming. We talked about what we could build if we had these geometric structures, these classic Greek solid geometric structures. We settled on an icosahedron. That would give us 20 sides and we could make something much more compact than the Crystal Palace. We'd cut out the middle and make a place for the SPEAR colliding beams to enter and exit. Charlie and I built a carboard model of this, which folded up and I carried with me on the airplane, okay? Then I took it to SLAC and said, "Oh, this is a Crystal Ball."

I had a fellow working with me, again who was a longtime friend, and it turns out he was Palestinian and very militant about the Israeli-Palestinian standoff, but quite a sweet fellow. We just had more in common than he realized, given my father's background. Fatin Bulos. He was a fixture at SLAC and got his degree from Stanford. Very, very smart. A very clever guy.

So, I showed him the model Crystal Ball concept, and then he went off and he thought about it. He said, "Well, you know, Buckminster Fuller has a geometry which probably would be a lot easier to build than what you're talking about because they build houses using it." He went off and did the work to show how you could make a Crystal Ball in a Buckminster Fuller kind of way, you know, geometry. That was convincing and that's what we worked on. Using Fatin's ideas you need a much smaller number of different shapes of crystal detector elements than in Peck's and my concept to make the Crystal Ball. In order to build such a device this was a big advantage as there was machining of all the pieces to high precision and such. It was quite a challenge.

At some point I had a group of people housed in an apartment in Cleveland helping the manufacturer, Harshaw Chemical (now out of business) make it. They couldn't do it themselves, and we had to put money into the company to build a properly controlled environment and SLAC had to machine to very high precision the aluminum crystal detector element shape templates so Harshaw could then manufacture the NaI(tl) crystals properly. Our group was also closely involved in the quality control of the finished crystals. My first graduate student, Mark Oreglia, didn't like being in Cleveland much, and he told me later—I think he announced it at in his talk at my retirement fest[7]. Mark Oreglia is professor at University of Chicago, and he said, "I don't know why the hell we were there." The reason he was there was because his degree would have taken two years longer if he wasn't there. [Laughter]

ZIERLER: Right. That's great!

BLOOM: I don't think he realizes it to this day how overstressed Harshaw was with this task. The reason we chose Harshaw? Well, they were the only choice. They were the biggest—not so big, but the biggest—and they had very close connections with Hofstadter who had built other detectors there using similar technology, but not as complex, and we had very strong connections with the Hofstadter group. But ultimately, we had to send a number of our staff and scientists to Cleveland. I think we spent a year there. I went to Cleveland periodically on visits to see how things were going and to try to help out when difficulties arose. It was like wind chill of 35 below, you know. [Laughter] It wasn't like the most fun place to live compared to Palo Alto. I can understand why Mark Oreglia didn't appreciate it. It was a tremendous effort on the part of a lot of people to make it work.

Anyway, so ultimately the Crystal Ball project[8] went very well. That detector has been used around the world ever since, and currently it is at the Mainz Microtron, in Mainz Germany. Our Crystal Ball collaboration had a world class physics program for 2 years at SPEAR at SLAC, and then 3 years at the DORIS storage ring in the DESY laboratory in Hamburg, Germany.

One of our major discoveries was the ground state of charmonium called the η_c[9]. Though we were the first to announce this discovery, the MARK II detector, also at SPEAR at that time, was fast on our heels[10]. Guess who? This discovery was the thesis of the Mark II collaboration Stanford graduate student that I thought was my best student at Caltech, Tom Himmel. So, he used the Mark II to also see the η_c and given the close timing of the discoveries we published two articles side by side in PRL. I was slow to publish, and the MARK II folks pushed me like crazy. They wanted us to publish first, because we had the first public result. We announced it before they had the result at the '78 Lepton Photon conference at Fermilab. I announced it in an invited talk; that was the greatest experience I had had giving a public talk. You get up there in front of the audience, and it was a big audience, and then you show this result and all of a sudden you hear a big gasp from the audience. I mean really! It was wild!

ZIERLER: Yeah, yeah.

BLOOM: I remember all of us were there before my talk, the senior members of the Crystal Ball collaboration. What should we do? What should we say? It was hot off the presses sort of. So that worked out well, and we also made some other great discoveries as well. We discovered glueballs. We had glueball candidates that are still viable candidates today. That was a passion of Don Coyne's, searching for glueballs. We did some really interesting work on axions which probably most people don't realize.

ZIERLER: I didn't know that. How did you get involved in axions?

BLOOM: Well, it turns out that the original axion theory of Helen Quinn from SLAC and Roberto Peccei from UCLA who recently passed away in June 2020. He was Vice Chancellor for Research and Professor at UCLA.

ZIERLER: Not Tom Appelquist?

BLOOM: No, Peccei at UCLA. He was a very nice guy, very smart. Anyway, the two of them came up with a strong U(1) symmetry that led to the Peccei-Quinn axion, which was designed to resolve the strong CP problem in particle physics.

ZIERLER: Ah, ah. Yes.

BLOOM: That was the first axion model which solved the strong CP problem. They did it inventing a U(1) chiral symmetry. Shortly afterwards, Wilczek and Weinberg pointed out that the breaking of $U(1)_{PQ}$ leads to a light, neutral, pseudoscalar boson - the axion, a°. They proposed a number of possible decay channels in which to search for this new particle. You don't have to have an accidental cancellation in the strong interaction Lagrangian to get rid of CP violation in the strong interaction. What you have is a Goldstone boson. It's an axion, and they had made very strong predictions. In particular, they predicted a certain branching ratio from the J/ψ into γ + axion that had a somewhat arbitrary constant involved that was not determinant with just the J/ψ branching ratio. But if you measure it at the upsilon, Υ into γ + axion, that constant was inverted: numerator went to denominator. So, you measure the two and you took the product of the two branching ratios. The theory predicted that the product had to be greater than a certain level, okay? So, you get my drift?

What we did with the Crystal Ball is measure both branching ratios[10]. The theory predicts the product of them has to give you an axion rate which is greater than a certain amount; otherwise, that model is wrong. So, we measured that. Kai Königsman was a champion of that set of analyses of Crystal Ball data. He's now a professor in Germany. It was a difficult analysis. It was a gamma plus nothing decay, but it was a gamma ray essentially at the beam energy.

Among many publications[8], we measured the $J/\psi \rightarrow \gamma$ + axion at SPEAR. As our SPEAR data collection was ending, I arranged, together with a lot of help from Panofsky, many other SLAC people and the people at DESY, to take the Crystal Ball to the DESY laboratory in Hamburg, Germany. Our detector was to be installed at the DORIS II storage ring, recently upgraded to run in the Upsilon energy range by Dr. Klaus Wille of DESY. So, after we did a few years at SPEAR and did some great work[8], then we decided we were going to go to DESY with the first SLAC experiment ever to do a user experiment in another lab. It was our equipment. It was our stuff, so Papa Pief said, "Well, okay," and he talked to me about the project. He said, "This long-distance travel excitement wears off quickly." I have experience already, right? It's going to be much more difficult than commuting from Caltech to SLAC. That was a breeze compared to doing an experiment in Germany.

Luckily, I won this prize from the Germans in 1982, the Humboldt Senior Scientist Award, and so I used the money from that prize to help support me and my family in Germany beyond my SLAC salary. Whatever I could get for daily support from per diem, it was not enough as it was very expensive to live with the family at DESY. We had to live there, okay, so it was not financially the easiest thing, but it was possible. It was a great experience getting the Crystal Ball to Hamburg. Sid Drell, Deputy Director of SLAC at that time, got a US air force C5-Galaxy to fly the Crystal Ball from Travis air force base in Fairfield, CA to Rhein-Main air force base near Frankfurt Germany. We had a special environment-controlled truck trailer that we built at SLAC to carry the Crystal Ball detector. The main detector material of the Crystal Ball, sodium iodide, is hydroscopic, so you had to protect it in case there was a breach. So, the whole trailer had an air dryer in it, a massive dryer that kept things safe, and lots of temp control and other electronics to monitor the environment inside the trailer and inside the detector, and it would run off electricity provided by the C5 which we had to hook into. Or, if we were on the road it would run off the trailer's gasoline generator. Then it had another backup as well consisting of bottled nitrogen that we could flow in case the dryer failed. We had to engineer the crap out of this. It was a tremendous experience.

Anyway, I got to know a lot about the Air Force, and we had great experiences with the Air Force folks. They were very excited to work with us. I didn't fly to Germany with the detector. Some of the team flew. I flew with my family commercial first, set up in DESY, and then we drove to Rhein-Main to be there when the plane arrived, and the trailer arrived there. My son, Matthew, was very excited about this and you know, the guards were there with their guns. You can't cross the red line, you know. My son crosses the red line. Agh! [Laughs] It was a great experience for all.

Then we had some trouble unloading because they didn't have the exact kind of tractor we needed to pull the trailer off the C5. The air force had to improvise. Then our DESY collaborators got us a Mack truck to haul the trailer to Hamburg because the Germans thought, "Well, it's an American trailer. We'll get it an American truck." The Mack truck was circa 20 years old; but it turns out our trailer had international couplings (by design). I mean the standard… We expected we were going to Germany. We prepared for that, but it worked with the Mack truck, except the Mack truck motor blew up about 50 miles out of Hamburg, on the autobahn yet. [Laughs] It was stuck on the side of the road, and so I had to drive down there again from Hamburg, and had to rustle up a standard modern truck which we then hooked onto our trailer and took it to DESY. We had our adventures at DESY installing it in its new environment. That was at the DORIS II storage ring, which was a great, great accelerator. We did some very good physics there, and that's where I made the big mistake of my career, which was the zeta "particle".

ZIERLER: Ah.

BLOOM: That was where sufficient skepticism wasn't shown. We tried to be very careful about that. We had this 5σ effect in two channels. It was a little over 5σ, so one channel was the γ + anything channel, or the inclusive γ channel, and the other in a γ + tau-tau decay channel, and the signal in the two channels combined to over a 5σ effect at the same particle mass in both channels, 8.3 GeV. We still were very concerned because we needed to reproduce this result with about the same amount of new data, and we didn't have the accelerator time to reproduce it. I went to the DESY directorate to get more accelerator time. If I was at SLAC, I would have gone to Sid Drell, who ran the SPEAR physics schedule, and said, "We have the signals, but we really need to make sure it's right. It could be a Higgs at low mass. It's a really big deal." Sid would have probably said okay take the extra accelerator time. We'd have kept it quiet, gone and run it, and it would reproduce or not (this had previously happened with other possible discoveries at SPEAR).

To get more time on the DORIS-II storage ring I went to the DESY director and he said, "Sorry. We can't give you more time unless you show your current results publicly." That was their rule. I don't know. It's not an unreasonable rule, and if there weren't any downside to making a mistake, it would be a perfectly fine rule. So, we showed it publicly at the… Let's see. It was one of the first major particle physics conferences in East Germany. It was Dresden. No. What's the other city? It wasn't Dresden. It was northerly, another big city that was seriously bombed. It was Leipzig. It was the big particle physics conference of the year, the Rochester Conference of 1984.

I traveled there with the director of DESY, Prof. Volker Soergeil, who drove me in his personal car, and that was a very interesting trip. It was an incredible experience, let me tell you. I'd never been to the Eastern Bloc before. This was in 1984 before the Wall came down. So, we drive across the border and it is armed guards and barbed wire and all this, and you go from West Germany to East Germany. It's like you're in a war zone.

ZIERLER: Yeah.

BLOOM: Suddenly in East Germany there are all these bombed out buildings, you know. You get on the Autobahn; it's Hitler's Autobahn still. I mean bricks pave the onramps. The autobahn is two lanes, you know, one way each way. We finally made it to Leipzig and to our hotel, and I was very paranoid about being spied on in my hotel room. So, we go to the conference and our collaboration presented the zeta preliminary results in a parallel session, okay, and the person that presented it was the post-doc who found the signal in γ + tau tau. That was the guy, Jochen Trotz, and I was sitting next to Karl Strauch (from Harvard) and we were listening. The young man gave a good talk. It was a short talk.

When the talk was over, all of a sudden, the East German press and TV just came rolling in. They were taking pictures of Karl and me. We're looking at each other. We were never told that this was going to happen. So, it became a worldwide event, and then it was presented, of course, in the plenary part as being this great discovery. I got an email from a past graduate student I worked with who was actually a Hofstadter student. He was in China and he sent me something from the *Chinese Daily* or whatever they call it showing these great results. The student wrote, "Who is your press agent?" I was really quite concerned that SLAC had made no formal announcement about this, and so we decided to make one. I talked to Professor Burt Richter, the SLAC director at that time, about this. We hadn't published the zeta because we had to show it reproduced first. So, all this was coming out based upon this unreliable initial data run that needed to be reproduced. So, I have here an article from the front page of the NYT[12]. After discussing the situation with Burt, SLAC made a press release, and so I soon got a call from the *New York Times*. [Goes to get the article]

ZIERLER: Ooooh! Very cool! Oh, that's great.

BLOOM: I made the quote of the day! Did you read what it says?

ZIERLER: I did, I did. Let the record say this is a *New York Times* article that Elliott is showing me.

BLOOM: The quotation of the day from me: "We really do not know what this beast is." So, what I meant to say by that is we really don't know that it's real or if it's a great discovery or what, okay? Anyway, that got lots of notoriety, which embarrassed the hell out of me. When we finally reran the experiment at DORIS-II and analyzed the data, which took months, there was a dip of almost exactly the same amount in the inclusive spectrum at the same place as we had the excess! (~5 σ dip) They canceled each other, and the signal was totally insignificant! Talk about statistics… That taught me a very important lesson about what you need to really show something is true, especially something that's important. I was totally mortified, so I arranged to talk at a number of conferences to announce the negative result, and I wrote up my talks. I announced it at SLAC first. (Burt was very unhappy that the zeta turned out to be not true.) As soon as we agreed that we were going to present this, I announced at SLAC that the zeta had gone away. I was invited to give a talk at Berkeley, Group A, at the home of Alvarez, and they didn't know about my SLAC talk. My talk at Berkeley was "it went away", and one of the more senior attendees said, "That thing went away?" I said, "Yeah, it went away." Then I gave talks in Europe, and maybe they were too long; I gave all the details to be as transparent as possible.

It's just a great lesson in humility, and of course there were people in our group who wanted it not to go away and people doing analysis—I won't say who. But they said, "The detector changed;" "This changed," and you saw they could make the bump start to come back. They changed the analysis and suddenly this dip started to fill in and I said, "No. [Laughs] All the analysis cuts on the data have to be the same. We can't do this; we can't tune the answer the way we want it. It's not there." That was quite a blow and changed… I think that was one of the big changes in my scientific career. I tried to always remember that, and it came in handy later on for sure, which I can tell you about when we get there.

ZIERLER: Elliott, do you see the work on axions as sort of your entre into particle astrophysics and cosmology?

BLOOM: I didn't think of it that way. I thought of it as particle physics. Now how did I get into astrophysics? Okay. Something happened. Things happened. My son passed away at the age of 16 from his heart condition. He needed a new valve. My uncle, at the age of 92, got a new valve and he survived; my son didn't. So, he died on the operating table after they replaced the valve. They couldn't restart his heart, so that was another big game-changer, life-changing event for us, my wife and myself and my daughter. That was about two years before I changed fields. But more of that later. First, my work on the SLAC B-factory, PEP-II.

I had done a lot of work on promoting a B-factory, as after the Crystal Ball I was casting about for a new interesting project in particle physics. During the Crystal Ball, I became friends with Alfred Fridman, who was a very well-respected French physicist in the CNRS, an incredibly smart guy, very creative and entertaining. He was considerably older than I was. We talked a lot about what should happen next, you know, after the Crystal Ball. (He was involved in the Crystal Ball when it went to Germany.) So, we started talking about, well, we should have a B-Factory at SLAC. There was this conference on a DESY B-factory concept in Heidelberg organized by Klaus Wille. He was a machine physicist at DESY, the guy who led the development, construction, and operation of DORIS II. He wanted to build a B Factory at DESY (which much to his disappointment never happened). But he had this great conference and there Fridman and I met Professor David Hitlin of CIT and Professor Klaus R. Schubert from the University of Dresden, and we formed sort of a collaboration. Charlie Peck of CIT was also interested. So, we started thinking about a B-Factory, and Fridman and I were thinking about using Hi Lum PEP and colliding beams as the basis of the design.

I actually had to do a fair amount of work with the PEP machine injection system when another experiment I was involved in, which you probably don't know… I was co-spokesman of the TPC/2γ experiment when Crystal Ball was still on going in DESY, and TPC/2γ was finally working well. A major part my work and that of my group's was to help upgrade PEP into Hi Lum PEP. We put all the PEP luminosity from 6 interaction regions into one interaction region, got all the interactions - all the luminosity of PEP into one interaction region to give the maximum event rate into the TPC/2γ detector. A major challenge for this project was the injection system of HiLum PEP, which was very problematic and difficult due to technical accelerator physics reasons, which I understood and people in my group understood. I was trained by Klaus Wille in how to make an injection system work while I was at DESY.

ZIERLER: Ah.

BLOOM: So, I learned a lot about machine physics at DESY and how their injection system, which is incredibly complicated, and just worked like a clock! [Snaps fingers] Unbelievable! I talked to Wille. "How did you do this? We have so much trouble at SLAC with our storage ring injection systems," and he gave me these tips about certain things you've got to do in an injection system to make sure it's going to work reliably and well.

Based on what I learned from Wille we tried to do what we could with the Hi Lum PEP design and construction, but we were limited because we couldn't change the storage ring and the injection system that much; we didn't have the budget or the time. We upgraded the injection line, but we couldn't do enough with it to dramatically improve its performance. We put in a lot of beam position monitors and did the things we could do that I had learned from Wille. For example, Dr. Gary Godfrey, a staff physicist in my group designed from scratch much more sensitive beam position monitors for the injection lines. We also enhanced our knowledge of the beam position and intensity just before and after the Hi Lum PEP injection septum. That helped, but it didn't help enough because of an additional complication. There was intense competition between injecting the Hi Lum PEP with electron and positron beams and running the SLAC Linear Collider (SLC) for physics collisions. The SLC needed the entire linear accelerator, and Hi Lum PEP had to take most of the linear accelerator for a short time to inject, and then go back to SLC, which always took time to recover. We couldn't make it work fast enough as the SLC was very high priority for SLAC at that time. We did get a fair amount of luminosity and good science out of that work, but the main thing for me is I learned what you really had to do to make a good injection system. [Chuckles]

So, what it came to finally… I was at a B-factory development meeting at UCLA along with Fridman when Dr. Pier Adone of LBL was there and suggested, "Why don't you make an asymmetric beam energy colliding beam B-Factory with two separate beam rings? You should do this he told me." I said, "Oh my god." I knew a lot about machine physics by then. Nobody had made an asymmetric beam energy accelerator work before, and I didn't have in my group the people needed for such a technically challenging project, and I didn't see the support in the laboratory to be able to make something like that happen successfully at that time.

We held a B-Factory meeting after that at SLAC and people presented different ideas/concepts for a B-Factory. Fridman and I presented our Hi Lum PEP colliding beam with one ring at higher energy…you know, symmetric with energy high enough to copiously produce B mesons. Adone talked about this other asymmetric beam energy, 2 ring B-Factory concept with center of mass energy in the Upsilon 4S energy region. Okay, so finally we decided, "Asymmetric is going to be hard as hell, but there is some indication from calculations that our machine physicists were doing it's possible to make this work." Being at the mass of the Upsilon 4S and having a moving center of mass system due to the asymmetric beam energies was a very big advantage in the search for CP violation in the B meson system, a major goal for the B-Factory.

So, after some time Professor Jonathan Dorfan (later to become SLAC director) finally got interested. It took a while. First Professor Gary Feldman (SLAC) was given the job by Burt, and Gary had a big group. That was Group C. That could do it. I wasn't even a formal group leader at the time! I had the Crystal Ball project, the TPC/2γ project. I was a project kind of character. Joe Ballam never forgave me. Okay. [Laughs] Ballam. But we decided, "This is a good thing for our group to get involved in. We know a lot about it in terms of injection."

So, when finally, Jonathan took over the project, now called PEP-II, after Feldman went to Harvard, I was made the head of the injection group by Jonathan. I had formal responsibility for the injection in the project, and it was a fairly big budget. I had 100 people working for me at one point, and we installed all new injection lines (2 separate, one for each ring, plus other major redesign to a large part of the old PEP injections system). We installed a whole new beam monitoring system. I had a terrible fight with people on the project about the size of the (dynamic) hole in the in the storage rings septum magnets. That's what Wille had emphasized in my training. "You have to have enough room for reliable injection! This is how much you need," and I argued, "You've got to… It's more expensive. The optics are more complicated. You have to correct for the larger injection aperture." I finally convinced enough people, and Jonathan and Dr. John Seeman, the chief accelerator scientist for PEP-II supported me on this, and we were given the necessary funding to do that.

ZIERLER: Elliott, what are the goals of the project? What are you looking to accomplish here?

BLOOM: To make an injection system that was going to be as good as Klaus Wille's was for DORIS II and it was. It made PEP-II, in my opinion, into the great machine that it was. That machine ultimately did over 10 times its projected luminosity. Initially, people thought it would never work. A lot of people thought that. I love projects like that. It was an engineering tour de force, and a lot of it was the injector worked extremely well[13]. If things went bad in the machine and the operators lost the beam, you got it back in a few minutes. That makes a big difference compared it taking an hour and a half to get the beam back after you've lost it for some reason fiddling around trying to get the orbits right and such. So, we were just overjoyed; it was a real factory. I believe that on all levels Jonathan made a machine like that, on all levels, and it worked very well. So, after the machine was built, they offered me the job of being the spokesman for BaBar, the one detector for the machine and I said, "No, I don't want to do that."

ZIERLER: [Laughs] You didn't want to touch BaBar, huh?

BLOOM: No, that wasn't it. I didn't want to be spokesman for a particle physics experiment of that size. I felt already with the Crystal Ball and Hi Lum PEP… I think my strengths are technical and scientific. I'm not a great politician; I'm too straightforward/direct the way I talk and act.

ZIERLER: And you were still involved with PEP-II at this point?

BLOOM: Oh yeah, I was. I had to finish-up PEP-II, my commitment. In parallel, I went and started in about 1989 teaching a class at Stanford to general undergraduates called Cosmic Horizons. I taught this class for a few years.

ZIERLER: Was this your first-time teaching, Elliott?

BLOOM: Oh no, no. I'd been teaching for years. I was a regular teacher. The SLAC faculty teaches on campus if they want to. When I was assistant professor, I was teaching already. So, I would teach a fair amount, and this particular class, I just developed it from scratch. I used a standard astronomy textbook for undergraduates, but I developed a lot more material, and the students did very well. I just learned a lot in doing it and I found out about the COBE satellite. That was the satellite that first discovered that the microwave background had these fluctuations ostensibly coming from the inflation period at the beginning of the universe, and also measured the microwave background to a fraction of a percent. COBE showed it was a black body, which was controversial at the time, and to high precision…and to me this was particle physics. I mean, this was talking about the beginning of the universe and was an incredible achievement. It won the Nobel Prize for J. Mather and G. Smoot that was well deserved. So, I got interested in particle astrophysics during the late 1980's. This is going to be a really new thing, particle physics from precision astrophysical experiments which you can't do in the laboratory.

So, in about late 1989 I talked to Dick Taylor about SLAC getting involved in particle astrophysics, and Dick was interested, but mainly in gravity. I was interested in gravity also, but he was interested in LIGO kind of gravity. Actually, he wanted to bring a prototype LIGO to SLAC. He wanted to start building a LIGO-like machine on the SLAC site, and I was not so interested in that. I saw LIGO as a "one trick pony" that would take many years to establish direct detection of gravity waves and might not work (LIGO and VIRGO announced the discovery of gravitational waves in September 2015. That was about 3 months before I retired.) I preferred experiments that are more multi-faceted, so they have some high-risk exciting elements, but they have a lot of bread and butter as well. The reason is for all my experiments, besides producing lots of different scientific results, I want to have as many students get degrees doing different science as possible. If you have lots of different physics that can get done, from my experience, it gives much more opportunity for student degrees. My Ph.D. project was not earth shattering in terms of the science. It was solid stuff, but it taught me a tremendous amount. That's how I trained my graduate students. So, they have to have good, solid theses to work on, that's how it worked for the Crystal Ball. It had lots of theses come out of it.

I wanted to do that for this new enterprise, and so in late 1989 Taylor and I put together a white paper for the consideration of the SLAC faculty. My emphasis was on particle astrophysics using observational x-rays; to use high energy astrophysics techniques to observe black holes, to see if we can understand black holes better, to test whether there really are black holes as predicted by General Relativity (GR). That's because at the time, you know, there was only very indirect experimental evidence that black holes were real physical objects.

ZIERLER: Right.

BLOOM: Black holes as described by GR had some weird features, a horizon, and an interior singularity. It seemed to me that "BHs" in nature were not properly described by GR because GR predicts a singularity associated with it. So, something deeper has to be going on there that we don't know about. The SLAC faculty approved Taylor and my proposal based on the new physics directions it promised for SLAC. The director, Burton Richter, was then motivated to lend some support. Dr. John O'Fallon our DoE monitor was also helpful in allowing our development to continue if it "did not get too big".

After searching around for a while in 1990 I teamed up with Dr. Kent Wood, who was a wonderful fellow at the Naval Research Lab, and a highly respected astrophysicist (who is now retired). Also, Professor Peter Michelson of the Stanford physics department was involved somewhat in this, not so much because he was involved in EGRET at the time and that took most of his time, but we talked about it. With the cooperation of the DoE, SLAC collaborated with Naval Research Lab to build a satellite x-ray telescope. That was the first astrophysics experiment at SLAC, And I was the SLAC PI. Kent was the overall PI and was like my thesis advisor. [Laughs] He was very well-known in the field, very famous astrophysicist, but young enough that we had a very easy relationship. Our collaboration promised very interesting science with a smallish group of collaborators again; one of those rare opportunities that luckily came my way in my 50s.

As Kent was well versed in this mode of getting a ride to space, we used the Air Force Test Program (not NASA) as the provider of a launch vehicle to get our proposed x-ray telescope to space (named USA for Unconventional Stellar Aspect). After your project is accepted the first thing the Air Force Test Program tells you is, "Okay, you're accepted for this. Make a model of your detector which is a weight model that gives exact weight and rough dimension. If you're not here when we launch, we're going to put that on the rocket because we have to have a proper weighting of the whole system. It's being calculated that way." Actually, one experiment didn't make our launch, so it does happen.

But we then built our parts of the experiment with my graduate students and my small group of staff and postdocs, which was now a formal SLAC group, the Particle Astrophysics group, group K. The funding came from SLAC (DOE), the air force, and NRL. We had responsibility for the very thin windows on the two multiwire gas proportional chambers that detected the x-rays, the x-ray collimator in front of the chambers, the mounting of the detectors on the air force spacecraft (called ARGOS). The multiwire chambers themselves and their electronics, including the onboard data recording was NRLs responsibility, as was the transmission of data from USA to the Naval Research Center. At that time, I also was taking graduate students from the Aero-Astro department on campus, and so I had an Aero-Astro student, John Hanson, who got his degree with me as well as an Aero-Astro professor. I was his thesis cosponsor with an Aero-Astro professor. Hanson was made the chief mechanical engineer for the USA space telescope.

In a very creative way, one of my technicians—clever guy, John Broder—built the x-ray copper collimator that we needed very differently than previous devices like this had been built, which was fast and cheap and saved weight. It worked extremely well. We required a very thin-walled detector to allow transmission of low energy x-rays, so you had very thin plastic Mylar layer that separated and sealed the gas volume from the collimator and so there was danger of leakage, or catastrophic failure, you had to worry about.

NRL was responsible for overall project, I mean, they were the lead lab because they were a defense laboratory with responsibility to the Air Force test program. They also had done this type of experiment before. In our checkout of the data acquisition system, we were responsible for offline data analysis and testing, we found tons of problems. I insisted on testing everything and I really believe in this idea that if you don't test it, it won't work. The NRL folks were surprising. They were mainly worried about schedule. They just wanted to get up there and be ready and I said, "No, we've got to test it all." Every time we tested, we found something wrong.

ZIERLER: What kind of stuff were you finding, Elliott? What were things that were giving you pause?

BLOOM: First, using an x- ray source on one chamber of the telescope, the front-end electronics didn't work properly. This required a serious redesign. We had two chambers, and each individual channel was tested separately, but we never got a chance to test the system that NRL built which merged the two channels and that failed in orbit. [Laughs] So, we had to use individual chamber information; we lost some efficiency by doing that. There were two separate detectors. Each had its own electronic system, and then they were merged. The merging mixed up the two and made it incomprehensible by the time it got to the ground. We couldn't use the two multiwire gas chamber detectors at once. That one item we didn't test.

Figure 1. USA Collaboration members attempting to observe ARGOS launch at Vandenberg air force base, January 1999.

We went into orbit on schedule pretty much, only about 1 year late due to spacecraft delays. A lot of the USA collaboration attended the launch attempts. Here's a picture (see figure 1) of us at about three in the morning, our group with NRL and SLAC people.

The guy second from the right in the front row is our technician, John Broder. Let's see. Kent Wood is in the last row on the left. I'm in the last row second from the right with my wife who is third from the right. Gil Fritz, the head of the NRL division sponsoring USA (Kent's boss) is in the second row up at the right end. Paul Hertz, a well-known astrophysicist at NRL is on the front row left most. Paul later became the director of the Astrophysics division for NASA science. There are a number of my Stanford physics graduate students present, one with spouse, but John Hanson did not make it. Interestingly, the man in the second row second from the right is Paul Kunz, a staff physicist in my group at that time. Paul is the man who brought the world wide web to the US helping to establish SLAC as the first worldwide website in the country in December 1991.

We traveled to where military launces took place in California at Vandenberg air force base, in Lompoc, CA. We stayed in a motel in a nearby town of Santa Clarita, and we got up early in the morning to view the launch. The ARGO satellite, as for many military satellites, was to be launched into a near polar low earth orbit, so you had to launch in a tight time window, and it was like 3 - 3:30 in the morning. So, we'd go to bed very early and we'd get up at two in the morning, and then we'd go to the base to watch and it wouldn't work. [Laughs] Every time! It turned out that AROS had the most launch tries of any satellite up to that time, 11 tries before they finally launched successfully.

I finally saw the launch on VHS tape that they sent me. [Laughs] So after about four or five times in person, we went home, and then for another four or five times they showed it on the web. (I got up early to try to see it live.) Then, they gave up doing that and then they finally sent a VHS tape -- We saw it, and it was a successful launch and a successful orbit.

After a short delay for other short experiments to complete (one was a test of a space motor that actually blew up and set the satellite into sun safe mode until mission ops could recover control! This event may have damaged our detector.) We started taking data. That's when we found out that the two proportional chamber channels were totally mixed up and we couldn't get any results out, and so we had to use one detector or the other. Then one of the detectors sprang a leak soon after we started taking data (caused by the motor explosion?)! [Laughs] Since we only had one detector left the electronics problem wasn't a problem anymore! [Laughs] So, we did a couple of years of data taking with that one detector, and I think really for me this was a wonderful success. We published lots of papers. Students—I had 6 students who received their Ph.D. thesis using USA data, and so we trained a lot of young astrophysicists. This group was about a third of the 19 students total that received Ph.D. s working with me in both particle physics and astrophysics, which isn't extreme for professors who are full-time teachers, but for an experimentalist professor at SLAC it's a lot of students.

ZIERLER: Elliott, when did you get involved in the Fermi LAT Collaboration?

BLOOM: The LAT? You mean LAT?

ZIERLER: Yeah.

BLOOM: Okay. So… Well first, just let me tell you about a very interesting paper that used USA. We were trying to measure if black holes had a surface or not. The black holes of GR do not have a surface; they have an event horizon. There are theories of black hole like objects that do have a surface. These theoretical objects are all bigger in radius than a GR BH, but just a bit bigger. This paper[14] that was based on the thesis project of one of my students, Derik Tournear, gives limits to the radius of a stellar object with a surface that could fake a GR like BH with the same mass.

ZIERLER: Mm-hmm [yes].

BLOOM: So, we went after this to try to show that using timing data and spectral data in USA and RXTE—RXTE was up at the same time, had a lot of data on one of the stellar objects we were looking at, which was Cyg X-1. We could show, using a recently published theory by astrophysicists, that you had certain patterns in x-ray intensity and timing if the object being viewed had a surface, okay, if there's a surface to this object. One can have a 10-solar mass "black hole" that was not a classical GR black hole, but it would have this surface, and the radius of the surface would be bigger than a GR black hole event horizon of that mass. We made a series of measurements and we set limits on the radius of a putative "black hole" stellar object at a given mass that theoretically had a surface. We showed that a number of types of theoretical "black hole" like objects with a surface were excluded.

There was another theory—which again, these are unconventional theories—that really a black hole is a dark energy star of some kind. George Chapline of LLL invented that theory a long time ago. We couldn't exclude that theory because… George is a clever theorist. The surface of his dark energy star—which has a very strange surface, but it's still a surface—is like a Fermi (10^{-13} cm) bigger than the classical black hole event horizon radius for a given mass black hole. So, we couldn't resolve a Fermi away, but we could resolve 15% bigger than a GR black hole. I thought that was a fabulous measurement. That's why I did USA…[laughs]…to test if physical "black holes" could be explained some other way than by GR.

In early 1992, Peter Michelson came to see me. He had given a talk at SLAC about EGRET and was thinking about some new ideas for the next gamma ray telescope. There was a permanent staff member at SLAC named Bill Atwood who I hadn't talked to about this yet, but I think that Michelson had. Bill had very creative ideas about detectors. Michelson came to me and said, "Well, how would you build a new gamma ray space telescope to replace EGRET?" Okay. So, I sketched a new detector on the black board that would have a cesium iodide calorimeter. It would have silicon strip detector tracker, and I forget what I put for the anti-coincident shield. I don't remember if I had one or not, extra scintillator counters or just the silicon strips. That's what I sketched out for him, but Bill Atwood had already talked to him and Bill Atwood probably had a similar idea. I mean, it was a straightforward idea that a particle physics experimentalist would have at that time for a space-based detector to replace EGRET. Also, Guido Barbiellini had published—which we didn't know about, but we found out later—an article with a design like that, but only using silicon strips, and the Japanese also had published a similar idea. Given the technology at the time, to me it was a straightforward attempt at a conceptual design, you know, but the problem is to make it work. [Chuckles] The details matter very much!

Okay, so I said, "Wow! Yeah, it would be good to collaborate on that." Dick Taylor was very encouraging. He thought it was a good idea. I talked to him about it, and Burt loved it, Burt Richter. So, Peter and I talked to Burt Richter about it and Burt was very positive. He thought it was a great new project and was the kind of thing SLAC should do. He wasn't so crazy about what I was doing with my little USA experiment that only cost SLAC a couple of million or something. He wanted a project costing tens of millions of dollars, and GLAST, the detector was a few hundred million. (The project was initially called GLAST, the name selected in a meeting in my SLAC office by our smallish group at SLAC/Stanford. GLAST stands for Gamma-ray Large Area Space Telescope. GLAST had resonance with Glasnost, very much in the politics of the time. After launch GLAST was renamed by NASA as the Fermi Large Area Telescope, or Fermi-LAT.)

ZIERLER: He saw this as a promising future for SLAC.

BLOOM: Yeah, Burt saw a promising future. We all saw it as great science. So, he said, "Yeah, why don't you go ahead with it?" though it was not legal. I mean, we were not an astrophysics lab and we had no formal DOE funding. Support came at the discretion of the director. When I started astrophysics at SLAC, the DOE monitor, John O'Fallon, came to me and said, "Elliott, what are you doing here?" and I said, "Well, we're building this to go after gravity, and to better understand black holes. It's not too much money; it's only a few hundred K a year." He said, "Well, yeah. Keep it small. That will be okay." So that's how it got started, sort of subversive, because in those days, the SLAC director had some discretion. *Never* could have started USA or GLAST now. In today's accounting, everything is project oriented. You can get smaller money, which is LDRD, but to get that kind of money for that length of time to develop a big project is very, very difficult. So, the world of US science has changed in a dramatic way…

ZIERLER: Right.

BLOOM: …in terms of innovating in fundamental science. We were very lucky. I feel blessed that I lived in that time, which allowed more freedom to innovate. Crystal Ball was the same. It was pulled out of somebody's ear, okay? And so was SPEAR! SPEAR was pulled out of the laboratory hide because Burt and Pief wanted to do it. Could never build a SPEAR today in a DOE lab!

ZIERLER: Right. Right. That's an important point.

BLOOM: Yes. So, we got started, and Bill Atwood had really been a spark plug for this. I mean to me he's a spark plug. We talked about it with Bill; he had these ideas. He didn't like that I had some of the same idea independently, so after a while I stopped talking about it, okay? But what Bill Atwood did was take these ideas and make them much more real, and he worked very hard to make it happen. He formally joined my group, Group K. He could do what he wanted. He was a permanent staff member. So, he joined my group and he started working full-time on the GLAST design using advanced Monte Carlo techniques and other members of the group and I worked with him. I had on Group K's plate, at the time, USA and also PEP-II was not completed, so our group was working on three big things. We were getting them done as roughly scheduled, and initially all the funding came from DOE. The work was mainly in my group K for GLAST because Peter had little funding for GLAST at that time. We had Burt behind it, and so I got SLAC funding to support our efforts. We did some work in silicon strip development, and then we got Professor Tune Kamae from the University of Tokyo, Japan. He joined our Collaboration and he had access to silicon strip detectors through industry in Japan, and we started developing our silicon strip tracker design. In my group we built protype silicon strip tracker trays (built structures to hold the strips)—that same technician, John Broder—and figured out ways to do wire bonding with Santa Cruz people. Santa Cruz joined the Collaboration, and later Professor Robert Johnson led the silicon strip tracker part of GLAST.

Soon Peter won some funding from NASA. He put in R&D proposals to NASA. I think the following year we got NASA money from that R&D program that they offer to start developing new missions, and then it grew from there. It took from '92 until it became a real project in 2000, after a formal proposal from the GLAST collaboration made in response to an RFP from NASA was approved. There was a lot of politics. It was very different than working with the Air Force Space Test Program. USA was much more informal. While building USA we were talking to the contractors, Hughes Aircraft. We talked to their engineers. We were working closely together with the spacecraft constructors from early on. My students had a fantastic education, but with NASA it was much more formal, a lot of paperwork and politics was required. We had shadow NASA people working with many of the GLAST Collaboration project management and engineers. The project management was at SLAC. So okay, when it became more of a formal NASA project things got a lot more complicated and bureaucratic.

Initially I was part of the development of the science. I was on the science team. NASA had some official board/committee to define the science requirements for GLAST; I was part of that. Also, we set up the ground rules for science operations, things like open public data, things like we're not going to point the LAT. We won't approve observing proposals and then you're going get your data by having GLAST point to your favorite source like a telescope for a specified time. Like let's say Chandra and you're going to point Chandra at a particular object because Chandra has a field of view that is very small. LAT has a field of view that is almost ½ the whole sky. It's stupid to point GLAST to get data from a single point object in the sky. The

Fermi LAT (GLAST) scans the entire sky, you know, every two orbits, or 180 minutes. What our committee decided is that ultimately an observer outside the Fermi-LAT collaboration can have open access to all the Fermi LAT data, and we put all the data out quickly. To fund outside observers' efforts to pursue some piece of science they submit a proposal to NASA, and it's kept confidential. Others don't know what they're planning to do. The proposal was kept secret from the Fermi LAT Collaboration, etc... If the proposal wins in a competitive review, funding is provided by NASA to the outside observer to carry out their science.

The Fermi-LAT Collaboration science was much more programmatic and funded by SLAC and NASA in the US and other agencies in our non-US collaborator's countries (France, Italy, Japan, Sweden). Our Collaboration had a year to get the Fermi LAT up and working in space after launch and do some team science. After the first year in orbit, the guest observer program began funded by NASA and some foreign agencies. So those kinds of things, those rules were set by this committee, which was an unconventional way to organize and run a mission for NASA.

Then, of course, GLAST weighed a lot. That's how NASA decided how long it's going to take to complete and how much it's going to cost, and they couldn't believe that we'd build it for what we said we would. Okay, we overran a bit. Why? Because they insisted, we put a reentry motor on the Fermi satellite.

ZIERLER: Ah!

BLOOM: A reentry motor on the satellite because even though we'd done studies to show that the LAT would have a totally minimal earth footprint after reentry, it still had some stuff that would hit the earth. The amount of stuff rules changed sort of halfway into the project, so they said, "No, we have to be able to control the reentry, so it goes in the ocean". That changed a lot of details that cost more money, and it also impacted the GLAST design. We had to take two layers (because NASA is so conservative) of our calorimeter off the back to save weight. We came in underweight. We could have kept them.

NASA folks were pleasantly surprised how SLAC operated and how quickly the project progressed. They're used to dealing with contractors earning money from this as a cost-plus contract. SLAC earned no profit. We had a project. We wanted to do exciting science as soon as we could, and we were anxious to get the project done and done right. Later in the construction phase, I was the I&T (integration and test) manager for GLAST at SLAC, so I ran the group that tested all the pieces. I was responsible for assembling the entire GLAST, the testing all the pieces, and the final completed GLAST.

I had a student, Alicia Kavelaars, who was a graduate student from the Stanford Aero-Astro department who received her Ph. D. with me, and she built the software which we used in the I&T testing. NASA wanted paper and we built this software system which we taught our technicians to use and NASA finally accepted, which gave them records of everything that was done in the way that they wanted. They finally gave up on having us do everything with paper. And there were a lot of other innovations that we did. We had to build a high tech dry clean room from scratch which would hold all the pieces of GLAST and the final assembly during the construction and other subsidiary elements. As the cesium iodide was mildly hydroscopic it had to be kept very dry and clean, okay? It was a clean room to NASA standard for space hardware, but it also had to keep things dry. We designed that. Our Crystal Ball experience was useful; we built dry rooms, a number of them.

ZIERLER: Right.

BLOOM: So, we knew how to do it without any problem. I interviewed with a NASA engineer who wanted to talk to me about my plans for I&T. He was the I&T manager for the last Hubble repair mission, and he said to me after I told him about what we had to do, "If I were you, I'd be very, very worried." [Laughs]

ZIERLER: Ah! What was he getting at?

BLOOM: GLAST was very complicated and had many moving parts and lots of…tons…almost a million electronic channels. It's just not a typical kind of space telescope. It was a typical particle physics detector, a small one, only two meters on a side, you know, and 1.5 m deep or so; but, it was very complicated as far as what NASA folks had experienced for a telescope, electronically, number of channels, how much it weighed, all these things, okay? We did it and we were on time. They weren't. They had problems. But we did have some issues. Where do you think?

First of all, we had 80 m^2 or so of silicon strips—the biggest silicon strip detector ever built at that time—in space! So, nobody had ever put anything remotely like that in space, and on the ground, it was the largest silicon strip detector ever built at that time. Since then, it's been surpassed at the LHC. Critics said, "You're going to die with this. You'll never do it. It's going to cost a fortune." So, Tune Kamae and Hamamatsu Inc. along with Harmut Sadrozinski of UCSC developed bigger and bigger silicon strip detector wafers. We started with little 2"x 2" that had to be connected via wire bonding to make one long (2" x 2 meter) detector. Too many detectors, and to noisy. We just increased the size of that building block detector to have the area which was, about 10 times the original, and the complexity dropped tremendously as did the cost per square centimeter, and this was our plan.

We got the silicon on time and we were building it. We figured out how to mount it. That was a lot of work my group did, figure out how to mount the silicon strips properly so it would survive the shake of launch and all that kind of stuff. We shook it. We took prototype tracker tray assemblies to Lockheed Research on Page Mill Avenue locally. They helped us, and we tested our mechanical pieces in shake and thermal vacuum. They failed miserably and then we figured out how to fix them. By the time we had to build a real one, we had a pretty good idea what to do.

Then a real problem was the cables for the tracker. Robert Johnson had designed four-layer cables of very compact design -- I mean the space we had for the cables was impossible, and so we had these state-of-the-art many-layer cables, but they had to be space qualified. Space qualified means you cut open a random sample and there are no air bubbles in it. The filler holding the wires has to be completely uniform.

So, we went to this company. They said they could do it. They went bankrupt trying to do it, and at the end, we were hanging on delivery of cables to make our schedule. What saved us was the testing schedule of the satellite was being delayed by the aero-space company that NASA had hired to build it because they had some high priority dark projects that they had to slip in before us. Ultimately, we got our stuff built, and a second company solved the cable problem.

Professor Persis Drell was then Associate Director (she became SLAC's director and is now the provost of Stanford University) and she set up a taskforce of very talented people, including Tom Himmel the same student who co-discovered the η_c who was my great undergraduate student at Caltech. He was on this taskforce helping us get these details taken care of, and he was very gracious in doing this since this wasn't his experiment. But Persis asked him to do it for the laboratory, so we got it done. The GLAST space environment testing was done at NRL, one of our collaborators, as that lab had a full complement of space test equipment that was able to accommodate GLAST. We then trucked GLAST to the NASA spacecraft vendor in Phoenix AZ to be mounted to the spacecraft and then to be tested as an entire assembly, including another much smaller experiment on the spacecraft called the Gamma Ray Burst monitor.

Then finally we were ready, but the aero-space company couldn't do the final thermal vacuum test as their only test chamber was tied up with another dark project. So, we had to do our final Thermal Vac testing at Naval Research Lab. That was always our backup because they have a full testing facility. That was not part of the NASA plan, but it was in my I&T plan as a backup. So, we went and did this full satellite thermal vac testing at NRL and kept on schedule, and so that minimized the time. It was June 11, 2008 when we launched. It took eight years to build it and get to orbit, and that was very fast for a NASA project.

ZIERLER: Elliott, what were your goals at that point, at the moment of launch? What were you hoping to accomplish?

BLOOM: My goal was to search for dark matter using the LAT. That was my *job*, okay? DOE had invested, I don't know, $50, $60, $80 million. I don't really know. Burt kept it a secret. There are official numbers, but we knew those are low. There was more that came out of SLAC's base budget for people who worked on GLAST. After this large investment, my goal was aligned with the goals of the DOE HEP division to find out more about dark matter. I also had other goals. I wanted Fermi LAT students to get training in particle astrophysics, I had them work on pulsars, the gamma ray diffuse background, active galactic nuclei (AGN), and other more astrophysical topics. I had done x-ray work with USA and with RXTE. I knew a lot about the astrophysics of those objects, and I knew there were many of these. By observing things like that GLAST could do very well and make a major contribution to astrophysics, which it did.

But my personal goal was dark matter. and so, after launch, we worked, my group – myself and essentially all my graduate students who received Ph.D. theses featuring different aspects of dark matter searches. There are lots of ways to look for dark matter with Fermi by indirect detection, and one of my best students was Alex Drlica-Wagner who is now an assistant professor at University of Chicago and a Schramm fellow at Fermilab. His thesis was on searching for gamma ray emissions from dark matter decays originating in dwarf galaxies. So, if you observe dwarf galaxies, they're dark in every known wavelength. The only way you can figure out that they are there is by stars associated with these galaxies, only a few stars that move around in a way that tells you there has to be a much bigger thing there, which is all dark matter. They're very highly dark matter dominated with relatively few stars, sometimes 20 stars, that you can see, sometimes a few hundred. It depends on the which dwarf galaxy, and he did a great job on that for his thesis. No dark matter signal was detected, only upper limits, and these limits are strong enough to give theorists pause on the validity of important classes of WIMP models.

ZIERLER: Elliott, given that we're still looking for dark matter, what do you see ten-plus years out some of the contributions to this endeavor and some of the limitations in the project?

BLOOM: Of Fermi? Well, there have been some interesting ideas recently of different ways to look at dark matter with Fermi, but they're not as sensitive as this dwarf galaxy technique. So, I think for Fermi, dark matter has pretty much played out, or will very soon, because we have 12 years of data, and to double that, it's not going to happen.

ZIERLER: Right.

BLOOM: So, I think unless somebody might have a brilliant new idea, and I've been racking my brain, I can't think of other things to look at. We've looked very extensively. You know, we must have 50 papers or more on different ways of looking at dark matter and only have limits at this time. I want to tell you the story, though, of the putative Higgs line in the inclusive gamma ray spectrum. Do you know about that?

ZIERLER: No.

BLOOM: Okay. Fermi LAT data is public, and people use it without sometimes the most experience in analyzing our data. So, a number of particle physicists decided they were going to analyze the data, go look for a gamma ray line from the Milky Way galactic center, and sure enough, they found a line. They found what they thought was a significant signal for a WIMP annihilation into 2 monochromatic gamma rays (line). This was really big news, but *I knew* that it was not a significant signal, that they had made a typical blunder. I talked to them about it politely. The lead on this paper was a very smart and very nice fellow, and he turned into quite a good Fermi LAT observer with time and practice.

One of my students, Yvonne Edmonds, who happens to be a black woman—she was, I think, the first black woman PhD in particle physics that SLAC ever graduated, and she is very good. Her thesis was to look for gamma ray lines in the inclusive spectrum and she didn't find any with the data we had at the time - just limits, and she did a very careful job. Sometime after Yvonne completed her thesis this putative line came out. There was another graduate student, Andrea Albert, who came and worked with us, also a woman, and she was a graduate student at Ohio State, which was a member of the Fermi LAT collaboration. For her thesis, she worked very closely with us trying to understand this new putative line from Dark Matter that had been reported, and we showed in a paper published in 2014[15] that this line was very unlikely for a number of reasons. You can read the paper to find out all the technical reasons. So, we said, "It's not a line. It's not real."

Most UC Santa Cruz people refused to sign this paper, okay, because they so much wanted that line. Peter Michelson, the PI of Fermi LAT would not sign it to begin with. I talked to him and said, "Peter, you have to sign this paper. You're the PI/spokesman, and it is an official paper passed through our vetting system. It's gone through review. We are publishing it. You have to sign it." He signed it, but it went out without Steve Ritz, without Bill Atwood's name on it, [without] all the prominent UCSC people because they didn't believe us. They thought it was really dark matter. I knew what they were thinking. I'd been through it with the Crystal Ball fake line, been there, done that. Okay.

So that segment of the collaboration that thought the line was real pushed for a Fermi pointing at the galactic center. So, we did away with, for months, the All-Sky Survey to focus our data collection on the galactic center looking for this line and to reproduce or to increase the signal. Okay. We did that and it went away. That was the ultimate proof that it was not real.

ZIERLER: Yeah.

BLOOM: And we all published a paper that it went away based on that, but we have our paper that came out first that showed that it wasn't real for a number of reasons. A lot of it had to do with a number of tries that people make. For example, you don't know what energy the line has a priori, and other issues. You know, you try enough; you can't use simple Gaussian statistics to tell you what the odds are that you're right. You have to correct somehow for the number of tries you make, and that wasn't done correctly by the authors who did the original work claiming a line. So that was one of the reasons we published in the first paper that the line was not real. There were a number of reasons we had. I'm very proud of that paper, and Andrea Albert for her great contributions to that paper. I mean, I was not popular, let me tell you, in the collaboration, and this young woman was also very unpopular because she just was doing her job. She didn't have a desire somehow to find this whatever it is great discovery. She wanted the right answer. Her job is to get the right answer. That's the way she did it. She's terrific. She's now at Los Alamos as a senior scientist. She's very good.

ZIERLER: Elliott, I want to ask you. As you're coming up to 5 years after your retirement, did you start to think of yourself really primarily as a particle astrophysicist or as a particle physicist who went into astrophysics and was interested in cosmology?

BLOOM: I really think of myself as a particle astrophysicist. I had a deep interest in particle physics. I thought that astrophysics was a laboratory one could use. It's sort of like you build accelerators to study elementary particles in a laboratory setting. I did machine physics also because you need to build machines to do particle physics. I built detectors because you have to have a detector. So, I viewed astrophysics as a tool. If you became good at it, you could do interesting and new particle physics. These ideas were becoming popular at the time I started in this direction. Other people, for example Professor Michael Turner thought this way at the University of Chicago. Precision astrophysics—you could do particle physics using astrophysical techniques and measurements to find out about the origins of the universe and very high energy particle physics, and that's the way I thought about it. I never changed my mind about this. I had to learn a lot of astrophysics along the way like I had to learn a lot of machine physics. Astrophysics is not my primary driver. It's a different field, really, than particle physics. What drives astrophysicists is something other than what drives particle physicists. What they call a discovery is if something goes off in the night that they didn't know about before.

ZIERLER: Right.

BLOOM: They have a different perspective. They love to see these new things that happen in the sky as observers. They're observers. Astronomers in general—they're more astronomers. Then of course the theorists, people like Professor Roger Blandford at SLAC/Stanford Physics, who is a fantastic theorist and understands a lot about astrophysics, tries to get at the root of it. But he understands how complex it is. You know, it's not really simple when you're talking about an AGN. I mean, how are we going to understand an AGN in terms of some finite theory which is so simple? I studied these theories. I can't believe it, okay? I might say it's a lot like climate change theory, very complex and non-linear, but I don't want to get into that. Yet they get a great pleasure out of it; it's fine. I like building instruments that are really new that find new things, and I like trying to understand the mind of God. That's my great interest in life. I go after the more fundamental things, and I use these others disciplines more as a tool, but I appreciate them. I get pleasure out of seeing the results.

ZIERLER: Well, Elliott, to sort of round out our discussion, for sort of my final question I want to ask a broadly retrospective question. Your transition in this regard—it's very representative. So many people in particle physics became interested in astrophysics and cosmology. Because you were involved in so many fundamental areas of discovery, really a golden age of ground-based particle physics, if you will, to what extent were your motivations to move into questions in astrophysics really driven by the idea that this was where the most fundamental opportunity for discovery was at that moment in time? To what extent was that part of it? To what extent was it there wasn't that much of a grand plan; it was just the things that were most compelling to you at a particular moment, or the best opportunities that you saw at a particular moment?

BLOOM: Okay. I saw the future of particle physics as being prisoner to very large collaborations, a cast of thousands. Well, BaBar was a not too big collaboration, but I had already done B physics when it was a new thing; I made my contribution. My name is on the BaBar CP violation paper. We knew that was going to happen and we made it work. My thoughts at the time were it's the LHC, these enormous experiments and collaborations, and I don't want to run one of those and I didn't want to run BaBar. [Chuckles] Contributing as one of the thousands of people didn't excite me. I thought they could all do a good job. I didn't see how I'd be something special in that environment. I saw astrophysics because of COBE's (NASA mission, Cosmic Background Explorer) promise of really doing precision particle physics in a whole new environment of the broader universe.

ZIERLER: Right.

BLOOM: …which could pay off in ways that we couldn't anticipate, and I think that idea was right. I think COBE was very successful. It was only the first step, and now we have Planck and future and ground-based telescopes looking for incredible gravitational waves from the Big Bang. So I think my idea was right. So I wanted to try, but again, I wanted to do experiments that had some breadth of possibilities.

ZIERLER: Which has always been true in your career. That's a constant.

BLOOM: Yeah. I got deeply into it through USA and GLAST. I mean I saw things that I hadn't spent the time to learn about when doing accelerator-based particle physics. The cosmological constant, Dark Matter, inflation—I didn't read about things like that much in my accelerator particle physics days. Due to these new perspectives and broader knowledge, I believe that dark energy is, the cosmological constant. It's just as mysterious as *g* (Newton's gravitational constant). Probably what the majority of particle physicists are not thinking. I thought I just sort of fell into it. Also, given my world situation and some of the terrible things that happened to my family with my son and things, I think I was in a mood for a change, trying something new that looked interesting.

ZIERLER: Yeah. Yeah.

BLOOM: But I didn't want to cut with particle physics because I always felt that was my first love. It still is, actually.

ZIERLER: That's right.

BLOOM: I keep track of what's going on in particle physics, more than I do in astrophysics right now.

ZIERLER: Well, Elliott, on that note, that's such a beautiful summation of not only your career and how you've always been interested in the most fundamental thing in front of you, but it emphasizes the idea that scientists are people and that very real, human challenges go into shaping what science happens and who does it. So it's a really wonderful perspective, and I want to thank you so much for spending this time with me. I really appreciate it.

BLOOM: Oh, thank you very much, I appreciate your patience. [End of recording]

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Appendix: Included below in figure 2 is important parts of my academic history put together by my last graduate student, Professor Alex Drlica-Wagner, that he presented in his talk at the Symposium marking my retirement from SLAC/Stanford University.

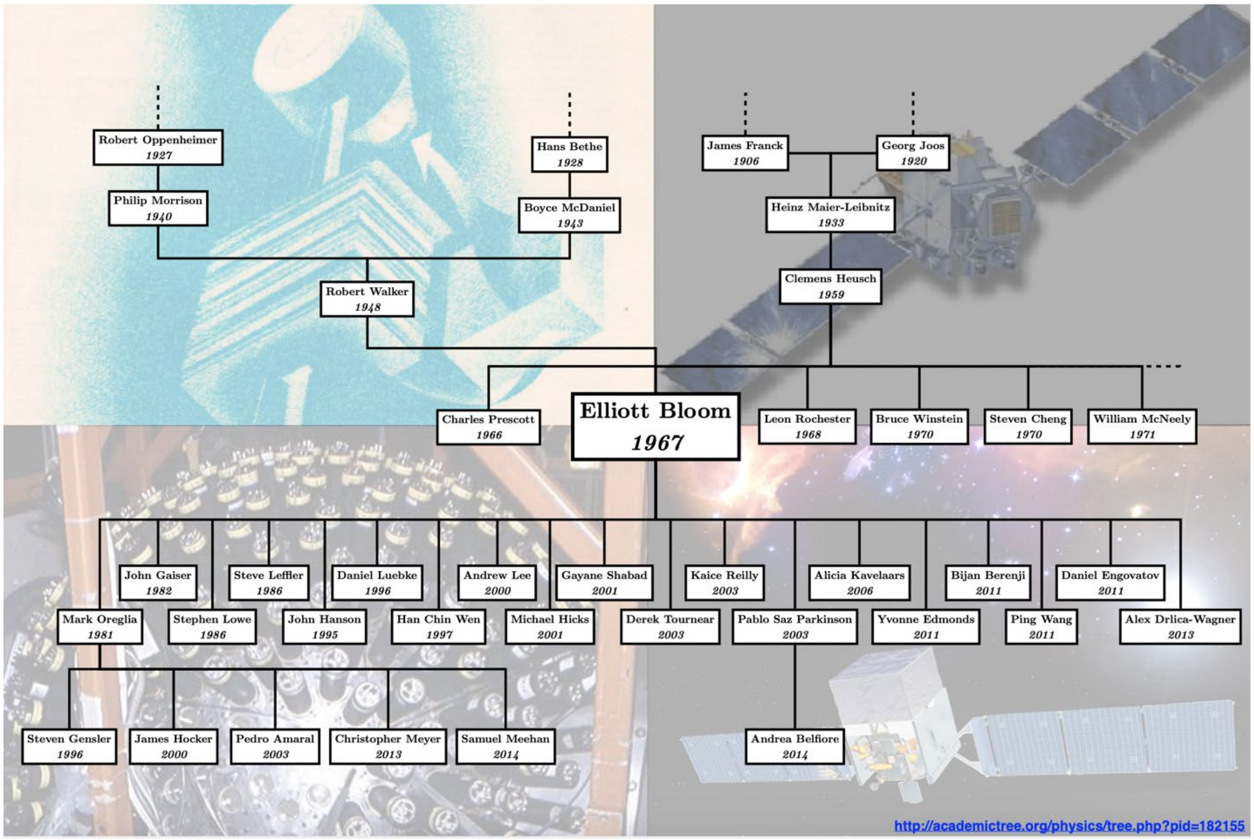

Figure 2. Elliott Bloom's Academic history showing in the background 4 experiments in which I had a leadership role, BC-42, Crystal Ball, USA x-ray telescope on the ARGUS spacecraft, and Fermi LAT. Superimposed is the academic tree from which I came, many of my fellow graduate students at CIT, and my graduate students and their graduate students.