AIP Interview

ZIERLER: Okay. This is David Zierler, oral historian for the American Institute of Physics. It is my great pleasure to be here over the phone with Dr. James D. Bjorken, known in the physics world as BJ, and it's a great pleasure to be with you today, Dr. Bjorken. How are you doing?

BJORKEN: I'm fine, but please call me BJ.

ZIERLER: Okay, I'll call you BJ. All right, so BJ, let's start with your early childhood in the Chicago area. Tell me about your birthplace and a little bit about your family.

BJORKEN: Some of this is in my memoir, and I'd rather not repeat what is already there.

ZIERLER: That's fine.

BJORKEN: I was born in Chicago, but grew up in Park Ridge. It is a suburb near O'Hare airport, and is also the home town of Hillary Clinton. In those days it was very conservative politically, as were my parents. I attended the public schools, which were quite good. I had an aptitude for science, as did my father. He emigrated from the backwoods of Sweden, as described in my memoir. But I did not get attracted to physics until I entered MIT.

ZIERLER: What was your father's occupation?

BJORKEN: My dad worked in a shop that repaired industrial electric motors and generators. I always felt that it was in my blood that, when the original Fermilab magnets kept burning up during the construction phase, I would be strongly attracted to that lab. My father would have known what to do.

ZIERLER: And what kind of primary school did you go to? Did you go to public school, private school?

BJORKEN: I only went to public schools.

ZIERLER: And did you show in high school, did you have a specific aptitude for math and science?

BJORKEN: Yes, the aptitude was there. But I was mostly interested in math and in chemistry. Physics was not taught as well.

ZIERLER: Now, when you were thinking about--

BJORKEN: This is just an aside. I am involved in the creation of an informal bio of Sid Drell, my mentor. I was surprised to learn that the same thing happened to him. He was not turned on to physics in high school. It was only after he arrived at Princeton that he picked it up.

ZIERLER: Now, when you were thinking about college, the two choices turned out to be the University of Chicago and MIT. And I found it interesting, because it seems that your parents encouraged you to leave the nest. Is that right?

BJORKEN: The reason they gave me was that it was too close to home. But I suspect that the real reason was that U of C was then run by ultra-liberal Robert Hutchins. Since my parents were conservative Republicans, they didn't want me anywhere near that bright pink environment.

ZIERLER: Uh-huh, uh-huh. And MIT did not have this reputation, I take it?

BJORKEN: Of course not. It was a place for nerds. In those days, MIT was not as socially conscious as it is nowadays.

ZIERLER: Now, given the fact that you weren't so turned onto physics in high school, what was your planned course of study when you got to MIT? Were you going to focus on math or chemistry?

BJORKEN: I was open-minded. But, without question, math and chemistry would have been higher up than physics when I entered.

ZIERLER: So, what was it that turned you onto physics when you got to MIT?

BJORKEN: That's in my memoir---even in the titles. It mostly had to do with the legendary Hans Mueller, who taught us freshman physics.

ZIERLER: Right.

BJORKEN: It was a beautiful course---a mixture of deductive logic and reliance on experiment. The deductive-logic side was very appealing to my instincts for mathematics. And the experimental side matched my instincts regarding chemistry.

ZIERLER: What was the course that Hans Mueller was teaching?

BJORKEN: Physics 801: the entrance course that everyone had to take.

ZIERLER: And what was it about Hans--

BJORKEN: He had been teaching it for years and years.

ZIERLER: And what was it about Professor Mueller that was so inspiring to you?

BJORKEN: The contents of the course were beautifully organized. And he was very theatrical and put on a good show.

ZIERLER: Uh-huh. And you write that Professor Mueller was in some ways "Einstein-ian." What did you mean by that?

BJORKEN: Oh, he had this big, white bushy mane of hair.

ZIERLER: (laughs) Oh, so he kinda looked like Einstein?

BJORKEN: His physical appearance was reminiscent of Einstein. And he had a very thick Swiss-German accent as well.

ZIERLER: Yeah.

BJORKEN: [In faux German accent] "Vy do we do physics? Because physics is fun!" Despite some groans from the audience, for me this has stuck in my mind to this day. It contains a lot of truth. And his follow-up was equally memorable. It was addressed to the hot-shot students from elite schools, already conversant with Einstein, the Bomb, and all the things that made physics at that time sexy. I had done some homework on calculus prior to heading off to MIT. But I was still way behind the hotshots. Anyway, Hans began by addressing the Big Questions: “Vot is space? Vot is space?” After a pregnant pause while he paced back and forth behind the lecture table, he answered his own question. “Space is vot ve measure mit a ruler.” “Vot is time? Time is vot ve measure vit a clock.” Then he got down to business, such as apples falling out of trees.

ZIERLER: It doesn't get more simple than that.

BJORKEN: It’s no joke. It’s absolutely true. As far as I am concerned, it is the right way to think about space and time. It is a fundamental, operational definition, no matter how one approaches gravitational theory.

ZIERLER: Now, you write of Sidney Drell as a sort of a father figure. How did you develop that relationship with him?

BJORKEN: Sid taught a lot of undergraduate physics courses. I attended more than one. And, of course, they were very well taught. In addition, he and his close colleague Fred Zachariasen ran an evening seminar for undergraduate students out of Sid’s house. One of the student attendees would be assigned to do a show-and-tell in Sid’s living room, via a small blackboard. There were roughly a dozen of us who would attend. I got to know Sid's family very early on. I've known Persis Drell since she was in the crib.

ZIERLER: Yeah.

BJORKEN: So, a close relationship between Sid and me started very early. Meanwhile, I became noticeable to Sid. Most of this is in the memoir. But, in brief, Sid noticed me because I didn’t pay much attention in class to his presentations. The relevant classes were problem sessions which supplemented the major lectures, not given by Sid. While Sid was holding forth, I was busy doing homework for other classes. I needed to get my homework done on the weekdays, because on the weekends, I was off to the mountains with the MIT Outing club-

ZIERLER: Can you--

BJORKEN: Sid noticed my indifference. He also noticed that I did fine on the exams. And down through the years he enjoyed telling others about our first encounters.

ZIERLER: So he saw that you had a natural talent in physics?

BJORKEN: This was out of the ordinary. So he noticed.

ZIERLER: Right, right. Can you talk a little bit about the research that you did senior year with Al Wattenberg and Bernie Feld?

BJORKEN: MIT had a 300 MeV electron synchrotron lab. I got a summer job working there as a peon. As I recall, I built a simple solenoidal focusing system for the injection line. Burt Richter was down there doing his thesis. And I hooked up with Al Wattenberg and Bernie Feld---wonderful people. Bernie, a theorist, was interested in what was called the theta-tau puzzle. It had to do with the decay of K mesons labeled theta and tau. The theta decayed into two pions, while the tau decayed into three. The problem was that the analysis of the final states indicated that the theta was even parity and the tau was odd parity. But otherwise the theta and tau behaved as if they were the same particle. If that were the case, parity conservation would have to be violated. And in those days, that was a no-no. Bernie's interest in the problem was stimulated by another resident, Dave Ritson. Dave had gotten a nuclear emulsion sample of stopping K's at the Brookhaven synchrotron, and Bernie became the local theoretical guru. Dave invited me over to have a look through the microscope at the tracks of the stopping K's. After five minutes of acute eyestrain, I vowed to have nothing to do with experimental physics.

ZIERLER: Yeah.

BJORKEN: I knew nothing about this thing called parity. That is grad school material, and I was a mere undergraduate. I was an innocent fool, in the right place at the right time, and should have asked the obvious question. It was T. D. Lee and C. N. Yang who asked it. That stimulated a major experimental program. A year or two later, I was walking across the Stanford quad on the way to the physics building when Sid Drell came running up: "BJ, there's some really interesting news! Parity is violated!" My reply was one word: "Good!"

ZIERLER: Yeah.

BJORKEN: Right from the start, it was an exercise in humility

ZIERLER: Now, you defined your…You knew that you didn't want to do experiments, but can you talk a little bit about what specifically attracted you to theoretical physics? What was so interesting to you about theoretical physics?

BJORKEN: I think it was just natural aptitude. It's just who I am.

ZIERLER: Now, the choice between Harvard and Stanford when you were getting ready to graduate from MIT, you write about a formative conversation you had with Fred Zachariasen. And he said, "Don't become a Schwingerian, because his legend at Harvard was already set." What did he mean by that?

BJORKEN: It was not long after Tomonaga, Feynman, Schwinger, and Dyson had done their monumental work on quantum electrodynamics. Schwinger's reputation in particular was deservingly very high. But his scientific style was very formal, and difficult to understand. I had personal evidence of this, via a friend---Russell Hobbie—who made a pilgrimage to Harvard to attend Schwinger's lectures on ordinary quantum mechanics. Russ was very organized, and produced excellent notes of the lectures. I may still have a copy somewhere in my files. But understanding what Schwinger was talking about was really tough. So I could resonate with Fred's argument. It is probably why I still remember it. It was after one of Sid's seminars, when Fred provided me with a lift back to the MIT dorms.

ZIERLER: Yeah.

BJORKEN: I suspect he was instructed to do that, and that he agreed with Sid to do it. They were both headed off to Stanford, and I am sure they wanted to encourage me to go out there with them. But this is just a guess.

ZIERLER: Now, you refer to this as an "exodus out west." What made it an exodus? Were a lot of people leaving at the same time and what was so attractive about going out west?

BJORKEN: The exodus included Sid and Fred, Burt Richter, Henry Kendall, Charlie Schwartz, and—a little later---Dave Ritson. For any of the MIT people involved with the electron synchrotron, the higher energy, higher intensity beams were very attractive. And by that time Bob Hofstadter was exploiting them to measure the size and shape of nuclei. For me, all of this was a factor too. But, in addition, there were for me the nearby mountains. I really wanted some nearby mountains at that point in my life.

ZIERLER: Right, right. What were some of the cultural differences between MIT physics and Stanford physics? Was there a different way that physics was done at Stanford that you could detect?

BJORKEN: There was more than a cultural difference. While I was there in grad school, a cultural divide occurred. On one side were newcomers like us, plus Pief Panofsky, who had arrived a few years earlier from Berkeley. On the other side was the Old Guard: Leonard Schiff, Felix Bloch, Walter Meyerhof, et al. When SLAC was proposed, the Old Guard opposed it, because it would disrupt the cozy, homey atmosphere of the department as it existed. Instead, it would be overwhelmed by a faceless bureaucracy based in Washington, DC. They also worried that it would drain away students. But, as it turned out, they overreacted. I do remember vividly a one-on-one conversation with Felix Bloch. “You don’t realize how bureaucratic SLAC will be. The Washington bureaucracy will smother physics at Stanford."

ZIERLER: Yeah, yeah.

BJORKEN: His comment was very prescient, because in my opinion that is what has actually happened in the long run, especially after 9/11. SLAC nowadays is much more in line with Felix’s warning.

ZIERLER: Yeah, right.

BJORKEN: Back then we had Pief Panofsky as director. He sheltered the lab from the Washington bureaucracy with a lot of hard work. Russians were able to visit, with a minimum of red tape. He had to fight very hard to keep SLAC an open lab---which it no longer is.

ZIERLER: Can you talk a little bit about your course of study at Stanford? How much time was divided between coursework and lab work and independent study?

BJORKEN: That’s a good question. Coursework occupied most of my time in the first year or two of grad school. My main focus was to learn quantum field theory. Don Yennie taught it. And it was a brilliant, wonderful course. Within our grad student culture, Don was a hero. He was the secret behind Hofstadter’s success in his electron-scattering experiments. Don did all the difficult calculations necessary to calculate the electron final-state wave functions. It was state of the art in setting up the calculation, as well as state of the art in using the computer to confront the data. To this day I do not think he got his fair share of the credit for that program's great success.

ZIERLER: Can you talk a little--

BJORKEN: Yennie's course was the highlight. There were of course other very good courses as well. But they are much more forgettable.

ZIERLER: Can you talk a little bit about what made the Feynman diagram so special?

BJORKEN: Julian Schwinger answered that question in a single sentence. In a history-conference talk, he commented that "Feynman brought computation to the masses."

ZIERLER: What did he mean by that?

BJORKEN: If you compute QED processes with Schwinger's methods, it is nothing but pain and suffering.

ZIERLER: (laughs)

BJORKEN: Schwinger used an updated version of what is called old-fashioned perturbation theory. It is based on the Hamiltonian formalism, and much more tedious that the use of Feynman diagrams. Those diagrams, as originally presented by Feynman, were heuristic and not rigorously derived. He of course knew that they were right, but nobody else did until Dyson created the synthesis between Schwinger's methods and Feynman's.

ZIERLER: Yeah. Can you talk about how you went about-- what was the process of you determining what your dissertation topic was going to be? Was this something that you came up with on your own? Was this something that one of your professors suggested to you? How did that work?

BJORKEN: It was half and half. The first part was assigned by Sid Drell. The second part was my own research. Both Sid and Burt Richter were influenced at MIT by Vicki Weisskopf. He was the grand old man at MIT, revered by everyone. And he was a pioneer in quantum electrodynamics. Like many others at that time in history, he felt that the theory was provisional and sure to break down at small distance scales. All this rubbed off on Sid and Burt. On arrival at Stanford, Sid's program was to examine which experiments were sensitive to short-distance modifications of the theory. Burt was eager to do an experimental test. The experiment turned out to be wide-angle electron-positron pair production. Doing the theory for the experiment was set up by Sid and was very tedious work. It was done in triplicate by Sid, me, and Steve Frautschi, a fellow graduate-student victim of Sid's program. The other half of my thesis was analyzing the analytic properties of scattering amplitudes, as described by Feynman diagrams. I invented a method for the general problem which used an analogy between Feynman diagrams and electrical circuits. I had learned graph theory for electrical circuits at MIT. The electrical-engineering students were given a huge dose of this material from Professor Ernst Guillemin. It was all the buzz in the dormitories, and it drew me in as a bystander. But the work that I did never got into the science journals, because I got scooped in the meantime by none other than the great Lev Landau.

ZIERLER: Were you aware of Landau's research, or you only found out about this after the fact?

BJORKEN: I found out only when Landau presented it. And I didn’t have an “oh shit” reaction at all. Landau was a great teacher, revered for that in Russia. I tried to use Landau’s text in quantum mechanics when I taught it at Stanford as a postdoc. But it was very hard. Landau’s style was brief and telegraphic. Landau omitted intermediate steps in an argument because they were obvious to him. But they were less than obvious to ordinary mortals like me. And his paper on Feynman diagrams was likewise brief and telegraphic. I much preferred my own version.

ZIERLER: I'll test your memory now. Do you remember the title of your dissertation?

BJORKEN: The title is long: “Experimental Tests of Quantum Electrodynamics and Spectral Representations of Green’s Functions in Perturbation Theory.”

ZIERLER: And at the time when you were writing, what did you feel was your contribution with this research and what field of physics did you feel like you were most contributing to with your dissertation?

BJORKEN: I didn't worry about such things. Documenting the work with Sid and Steve which supported Burt Richter’s experiment was clearly appropriate for the thesis. What I did with Feynman diagrams found its way into the public domain via Chapter 18 of our books. But it was never a formal paper in the literature.

ZIERLER: And who was on your committee?

BJORKEN: Other than Sid and Fred Zachariasen, I don’t remember. In those days, the oral exam was very serious. The questions could range over all of physics, which required a lot of serious preparation. Nowadays the oral is a formality---the questions are only about the thesis material. But then it was three hours of terror. Anyway, in my exam, Fred asked me a question. I don’t remember what it was, but I do remember that Fred for sure did not know the answer to it. He was having fun with me. So I just dug in and tried to calculate it on the spot. I calculated on and on and on. When I finally finished, I ended up with the wrong answer. This was after an hour or two of my efforts. I shrugged, and said “I tried, but I failed. I’m so sorry.” After an uncomfortable silence from the committee, it was broken by Sid asking me an easy question.

ZIERLER: Uh-huh.

BJORKEN: So there was Fred’s question and one softball question. And that was the exam.

ZIERLER: But you still passed.

BJORKEN: Oh yes, they passed me. They couldn’t find where I went wrong. So how could they flunk me?

ZIERLER: Now, I want ask you, since you--

BJORKEN: This story was actually documented. In those days the grad students kept a secret notebook documenting their experiences on the orals (e.g. who asked what questions?). I doubt that that notebook still exists. Nowadays the Stanford oral exam is nowhere as fearful as it was in the old days.

ZIERLER: So I wanted to ask you, since you were really present at the creation, if you could talk a little bit about, what where some of the really big goals that were set out in terms of the creation of SLAC? What was the creation of SLAC designed to achieve and accomplish?

BJORKEN: After the success of the linac on campus, building a higher energy version of it was a natural next step. But Project M (M for Monster) was a great leap forward. It was to be the first "geographical" accelerator, so big you could easily see it from an earth satellite.

ZIERLER: Yeah. What were the technological developments that made SLAC possible?

BJORKEN: The basic technology dated back to World War II and was in use in the linac on campus. The technology for SLAC was for sure more advanced. But I am not an expert on that.

ZIERLER: Now, after your dissertation defense, you decided to stay on as a post doc at Stanford. Was that a unique arrangement? Did most dissertation graduates move on, or did most people stay on at Stanford?

BJORKEN: No, most postdocs move around from place to place. But Sid and the Stanford people wanted to hang on to me. They put me on leave for three years in a row (my "*wanderjahren*"). All that time I was still a member of the Stanford physics department. I did return in the summertime, especially because the textbooks were being written in those years. During the academic season, I was a very successful procrastinator, much to the annoyance of Sid. So yes, it was an unusual arrangement. But it was a quite attractive one for me.

ZIERLER: I wonder if they were thinking of you as a kind of a scout? Like you would go out to these other places in the world, and then bring back what you learned.

BJORKEN: No, they wanted to hang onto me at the tenure level. I think that was probably much more on their mind.

ZIERLER: Uh-huh, uh-huh. And so it was during this time that you spent some time at CERN?

BJORKEN: Right.

ZIERLER: What was the connection there? How did you end up at CERN?

BJORKEN: Mountains were always in the mix. But the physics there was very, very good. And Geneva and environs is a wonderful area.

ZIERLER: What projects were you working on at CERN?

BJORKEN: Frankly, I just don't remember. I suspect that Sid had a sabbatical year at CERN at the same time. But I am not even sure about that. I am quite sure that we did not do any research together. But he may have wanted action on textbook-writing. If so, he was unsuccessful. I am a very good procrastinator.

ZIERLER: Right, right. So this textbook, this multi-volume textbook, what was your involvement with that? What was the need for this textbook at this time?

BJORKEN: The textbook used in Don Yennie's class was Schweber, Bethe, and de Hoffman. In those days, it was widely used, but also was dated. There was, I believe, a rather widespread feeling that one could do much better. Leonard Schiff was closely linked with McGraw Hill. Leonard probably pressured Sid to consider writing one. And then Sid asked me to collaborate.

ZIERLER: Now, separately, you spent some time in Copenhagen at the Niels Bohr Institute. What were the things that you were--

BJORKEN: Copenhagen was the last of the three *wanderjahren* years I spent away from Stanford.

ZIERLER: And do you remember what you were doing there?

BJORKEN: Enjoying Copenhagen. It is a wonderful city. I got a taste of it a year earlier, thanks to a grad-student friend, Abe Goldberg, who was a postdoc there. He informed me that the Bohr Institute was arranging a spring-vacation journey to Russia. This was in the midst of the Cold War, but thanks to a close personal friendship between Niels Bohr and Igor Tamm, it turned out to be possible. Abe suggested that I join in, which I did. So I got a first taste of the Copenhagen ambiance as well as a first look at Moscow, Leningrad, and the excellent physics communities therein. I already knew that via a Stanford colleague, Marshall Baker. He managed to visit Russia very early on, and brought back stories of how excellent the physics was there. The best and brightest of them were from the Landau school. I got to meet them (briefly) on the Easter trip, including Sakharov.

ZIERLER: Now, during your trip to Russia, did you have a sense if the KGB was interested in your presence in Russia?

BJORKEN: Of course. We were very carefully watched. Some years later, I was invited to visit for three months at ITEP in Moscow. I brought the family along. We were accommodated within an apartment complex run by ITEP for employees and visitors. It was very comfortable, but clearly the apartment was bugged. My stepson tried very hard to find the bugs, but he failed.

ZIERLER: In your collaboration with the Soviet physicists, did you feel like your work transcended the Cold War? That you were just looking to partner with physicists in a way that had nothing to do with politics? Or was it inescapable that you were on one side of this Cold War and they were on the other?

BJORKEN: It was the physics that was transcendent. In fact, I deliberately stayed away from matters political throughout my many visits to Russia---even when the internal protest movements were occurring. The Russian physicists suffered from the isolation from the West. It took a long time for the science journals to make their way to the physics institutes. And they had trouble distinguishing the clutter therein from the material that was making real impact on the western scientists' thinking. I could help set them straight: "Oh, don't worry about that paper---everyone knows that he doesn't know what he's talking about."

ZIERLER: Uh-huh, uh-huh.

BJORKEN: I met Sakharov a few times---typically once per visit. The session would be physics shop talk only, with two other “listeners” in the office in addition to Sakharov. Still later, Sid started making frequent trips to Moscow. The opposite occurred: long sessions in the Sakharov kitchen, with nuclear arms control and the like the principal subject matter. I did almost nothing in that direction.

ZIERLER: Was your sense in the Soviet Union that physics was being done at a very advanced level? Was it farther ahead than the Americans?

BJORKEN: They were very much up to speed. The leadership---Okun, Ioffe, and Gribov in particular--stand out. In some ways they were for sure ahead. In other ways they were a bit behind.

ZIERLER: And were there things that you learned from your Soviet counterparts?

BJORKEN: Of course.

ZIERLER: Like what? What are some examples?

BJORKEN: The answer is definitely a yes. But it will take some reflection to come up with the best examples.

ZIERLER: Yeah. Okay, so in 1964, you're back at Stanford, and it looks like you have a choice at this point. Are you going to join SLAC or are you going to stay with the physics department? And so in this choice, what was it that the physics department was offering you? Were they offering you an assistant professorship or something else?

BJORKEN: Both at SLAC and in the department I was offered a tenure position. But there was that conflict between the department and SLAC in that regard. Pief Panofsky, SLAC director-to-be, felt strongly that SLAC senior physicists should have an academic position as Stanford professors. His argument was that this was needed in order to attract top-notch physicists to the lab. The university administration agreed to this. But, as I already mentioned, the old guard within the physics department (Bloch, Schiff, and Meyerhof in particular) was horrified. It would compromise their autonomy and lead to the aforementioned dominance of Washington bureaucracy of Stanford physics programs. Opting for SLAC was a difficult decision for Sid, because going there might jeopardize his contact with students and with his teaching. It was less painful a decision for me. I don’t think his decision influenced mine, or vice versa. They occurred at about the same time. And, as best as I can recall, we did not interact directly regarding that issue.

ZIERLER: So this concern that there, this concern that there would be a lack of autonomy. Was that a legitimate concern? Did it play out like that?

BJORKEN: In the short term, the answer was no. In fact, a decade or two later there was a year when the physics department was shorthanded, because too many of their faculty took a sabbatical. So they came to SLAC for help in teaching courses. And, as I recall, there was no oversupply of SLAC people rushing to their aid. They were very busy with their experiments. But nowadays the bureaucracy has a bigger impact. And I became interested in the prospects for inelastic electron scattering experiments right from the start.

ZIERLER: Now, since you were there really right at the beginning, how much were you involved in building both the lab itself and the workforce?

BJORKEN: Not at all.

ZIERLER: So what were you involved with in the lab at the beginning? What were your projects?

BJORKEN: In the very early days, the SLAC theory group consisted of Pierre Noyes, Sam Berman, Sid, and me. We all concentrated on helping to define a robust experimental program for SLAC. Pierre concentrated on prospects for neutrino experiments, and Sid invented the "Drell Process" for exploitation of the photon beams. Sam was, as best I can recall, in the middle of all of that.

ZIERLER: And how did you get involved with the cosmic ray group in Utah?

BJORKEN: A friend of mine, George Williams, part of Felix Bloch's group, failed to obtain tenure at Stanford. He ended up in the condensed-matter group at the University of Utah. Since he knew I liked mountains, he invited me out for visits. Once there, I connected with Jack Keuffel's cosmic ray group and bonded very quickly.

ZIERLER: And what was the work that they were doing out there? What was their big project at that time?

BJORKEN: They had a huge experiment deep underground, beneath the Park City ski area. They were looking at high energy muons created by cosmic ray neutrinos. It was a trendy subject in those days. The biggest detector, created by Fred Reines' group, was located in South Africa. But it was relatively crude. Jack's detector was a real spectrometer, able to measure the momentum of the muons up to a TeV. The event trigger consisted of four tanks filled with water, with detectors looking at the Cerenkov light emitted by the muons. Between the tanks were banks of tracking detectors. They were acoustic detectors. Each element was a twenty foot sewer pipe with a wire down the center carrying high voltage. A muon passing through would create a spark. The sound of the spark would be detected by microphones at the end of the pipe. The arrival times would locate where in the pipe the spark originated. Between the water tanks were slabs of magnetized iron, which bent the muons. The whole thing measured something like 40' x 20' x 20' in size. The maximum detectable momentum of 1 TeV was a huge number for those days. And inelastic neutrino scattering is a very close cousin of inelastic electron scattering. So my deep-inelastic thinking was directly applicable to what Jack was doing. Consequently, I got very interested in the experiment and in visiting Utah. I vividly remember visiting the experiment itself. To access the detector, one did not descend an elevator. Instead one went in horizontally from a nearby valley through a long 3' x 3' tunnel. The vehicle was a two-person push-pull handcart. I was told to be sure to keep my head down, because on the ceiling of the tunnel there was a fat, unshielded, copper bus bar that carried the high voltage to the experiment.

ZIERLER: (laughs) Wow.

BJORKEN: So please, BJ, don't raise your head. After a lot of push and pull, we arrived in the cavern housing the detector. When the lights went on, it was like being in the middle of a science fiction movie. Awesome! I remember that a sneeze created a remarkable background event, because all those microphones detected it. So please, bj, shut up. A big billboard alongside the detector showed the muon tracks from real events. A sneeze lit everything up.

ZIERLER: Right, seriously.

BJORKEN: It was all very memorable.

ZIERLER: Yeah.

BJORKEN: I spent a winter quarter there in that period. Equally memorable was the skiing. If it had snowed during the night, my phone would ring. Be ready in a half hour. Jack and a close friend, Gale Dick, and maybe one or two others, would arrive at my door and transport me up to Alta to ski the fresh powder. Alta was and still is legendary for its excellent snow and special ambience. On the way up the chairlift with Jack, I would get a tutorial on cosmic ray experimental physics. On the way down, the subject would change: how to ski deep powder. Those were good days.

ZIERLER: Once again, once again the theme of mountains comes up for you.

BJORKEN: Yes, over and over again.

ZIERLER: Over and over again.

BJORKEN: Yes, down through the years, it has been a significant part of my life.

ZIERLER: Now, at SLAC you refer to yourself as a "Group A groupie." What does it mean to be a Group A groupie?

BJORKEN: “Group A” defined the experimental collaboration. While I was not a member, I was thinking about the experiment a lot, as well as being a close friend of some of the members.

ZIERLER: And you write about how Feynman would, he took an interest in SLAC, and he would visit SLAC every once in a while. Can you talk about your interactions with Feynman?

BJORKEN: Feynman did not visit very much. It was only that one visit that I recall. And that visit is well documented.

ZIERLER: Can you talk about the development of light cone quantization, formalism, that was developed by your students, John Kogut and Dave Soper?

BJORKEN: Yes, they did a beautiful job on it, all by themselves. Actually, the idea goes back to Dirac. But it was not recognized by the community. The motivation behind the Kogut-Soper work was based on current-algebra sum rules. The program was initiated by Murray Gell-Mann. It was a trendy subject in the 1960’s, with many contributors, including Sergio Fubini, Steve Adler, Feynman, and yours truly. Several of these sum rules were best expressed in light-cone variables. I had some familiarity with the light-cone language. But the others (especially Kogut and Soper) did better with it than I did. And of course, Feynman made that language central to the parton description of hadron constituents.

ZIERLER: Ah-ha, ah-ha. Now, in the 1970s, you make more trips to the Soviet Union. Were these trips different in character from your original visit earlier on? Or is this really a continuation of that earlier collaboration?

BJORKEN: My Russian experiences began with the trip in the 1960's from Copenhagen. The Russians sensed that I was receptive to visits to Russia. So almost every year I would get an invitation to a conference being held in Moscow. The conferences were sponsored by the Academy of Sciences, which had a reasonably friendly relationship with the regime. So the conference invitation sufficed to get me into the country. But once there it would be okay for me to visit sundry institutes, especially those which had less friendly relationships with the regime. I would get a day at the Lebedev Institute, with a post-seminar one-on-one meeting (not counting the watchers in the room) with Sakharov. The visit usually included a trip to Gribov's institute ("Gatchina") outside of St. Petersburg (Leningrad). Gribov liked it up there, because the political restraints there were considerably looser than in Moscow. The quality of the physics in Gatchina and in ITEP (Moscow) was extremely high. Their seminars were open-ended, informal, and very similar to those at SLAC. In fact, when the Russians finally were able to travel and got to SLAC, they felt very much at home. And they did not say that about most places they visited in the US.

ZIERLER: Now, was it you who invited your Russian counterparts to SLAC? Were you involved in that?

BJORKEN: No, I think it was Sid who did the work. Administratively, I have always been a nobody.

ZIERLER: Yeah. (laughs) I wonder, can you recount the story of the phone call you received in 1974 when Burt Richter called you with the news of what SPEAR had discovered?

BJORKEN: The story is well documented. Burt Richter phoned me while I was having dinner. "We have discovered a very narrow resonance...." I went back to dinner in a daze. It was roast beef with a sharp horseradish sauce. After I wolfed down the horseradish with no effect whatsoever, my wife Joanie said, "You had better go to the lab now."

ZIERLER: Yeah. (laughs) You had a very understanding wife.

BJORKEN: By then, she understood my nerd-hood very, very well.

ZIERLER: And what exactly was so tumultuous about what was discovered? Can you explain that?

BJORKEN: It was the narrowness of the psi resonance. It was very spectacular, and obviously was of fundamental importance. Actually, Helen Quinn and I had written a memo some time prior to the discovery, urging Burt to devote some running time to scan for narrow resonances. We included a catalog of physics examples of what they might find. Alas, charmonium was not included. Stupid!! Stupid!!

ZIERLER: I wonder if you could explain why, as you assessed that in both standard quantum chromodynamics and in the (Maverick Lipkin) version, you saw that quark and gluon confinement was a big deal, but that you concentrated in the how of their confinement, and not the why. I wonder if you could explain that?

BJORKEN: If, in some high energy collision, a quark gets hit very hard, it may leave the collision environment at nearly the speed of light. But confinement demands that it cannot do so alone. So something has to accompany it, and screen its color. The dynamical mechanism which describes this is known as the "inside-outside cascade". Over the phone is not the place to go into details. But the most important feature is that it takes quite a long time for the color of the quark to get screened. The understanding of this requires a good understanding of the spacetime geometry of the collision. This problem, distinct from the confinement problem, was worked out not only by me, but also independently by Lenny Susskind and John Kogut.

ZIERLER: Can you explain, what was so monumental about Steve Weinberg's theory of leptons paper, and where did your work fit in with that?

BJORKEN: I was a reactionary critic. But of course it was a great landmark contribution, which went a long way in unifying the weak and electromagnetic interactions.

ZIERLER: So how exactly, how did Weinberg unify those subjects?

BJORKEN: Actually, he did not completely unify things. The title of his paper is "A Theory of Leptons." The whole story has several steps, including what is called the GIM mechanism. It is well documented, and need not be elaborated over the phone. But Steve's paper set things in motion, and of course is a classic.

ZIERLER: How did the concept of rapidity change over the course of the 1970s?

BJORKEN: The rapidity concept had its origins in cosmic ray physics, where it was known as log tan theta. Feynman provided a more precise version and of course popularized its use. I picked up on it and wrote a paper called "A Plumber's View of Quantum Chromodynamics."

ZIERLER: What does it mean for a plumber to have a view?

BJORKEN: The variables log tan theta and phi have the topology of a cylinder (Theta and phi are polar and azimuthal angles, respectively). The particles produced in a typical, generic high energy collision populates this cylinder rather uniformly. But if there are high transverse-momentum jets, they create their own final-state cylinders. It turns out that these can be attached to the generic cylinder at right angles. In a multi-jet final state, the overall topology is that of a plumber's tree. (I still like this paper, and wish that it had taken hold among the expert practitioners.)

ZIERLER: Now, in 1979, you make the decision to move to Fermilab. What was the, what were the decisions that led you to make that move?

BJORKEN: Leon Lederman, the new director, really wanted me to join the lab. Part of the inducement of going there was that Leon did not want to live in the director's house, which is a glorious old estate occupying the northwest section---perhaps a square mile--of the laboratory grounds. It dates back to the prohibition days, when some kind of mafia warlord owned it. Anyway, Site 29 was the natural habitat for the Director, and indeed Bob Wilson occupied it. It has its own stable, has a huge front yard, complete with a pond, and is surrounded by forest. Quite splendid! Nevertheless, it was available to me and family if I were to join Fermilab. These happened to be my midlife-crisis years, so I was vulnerable. But I had to convince Joanie, my wife, who was a lifelong Californian. She got convinced, and so off we went. Unfortunately, about a third of the way through the decade that I was there, Joanie died of salivary gland cancer. My kids were of high school age, and thereafter parenting became a central responsibility of daily life. At the end of the decade (the 1980's), Leon stepped down as director, and my kids headed off to college. It was clear that it was time for me to exit Site 29. The only question was whether the move would be across the street into Batavia, or across the country to SLAC and California. I opted to go back to California.

ZIERLER: Now, when you started at Fermilab, your title was associate director for physics. And so I'm curious, how much of your job was bureaucratic and how much of it was actual physics work?

BJORKEN: It wasn't at all bureaucratic. Leon knew better and so did I. It was a formal title. With one exception, he let me do my thing. The exception was that he connected me up with the program office, responsible for dealing with new proposals for Fermilab experiments. I spent most of my time in that milieu, communicating with the proponents regarding their program goals and the realities of working at the lab. This meant being rather directly involved with the lifeblood of the lab. That was fine with me, especially because there was very little line responsibility associated with that role.

ZIERLER: What were some of the major cultural differences between Fermilab and SLAC?

BJORKEN: The Fermilab sociology was interesting. The region around Fermilab at that time was not exactly a cultural desert. But it was out there in the suburbs by itself. So the social life of the lab tended to be centered within the lab itself. It had a splendid restaurant on site, which was a great gathering point. And there were a lot of evening social events which attracted the visitors as well as the staff. My wife Joanie ran the guest office, and we of course were in the middle of these activities ourselves. On-site after-hours social activity was not at all necessary---or present--at Stanford, where that kind of thing is everywhere in the immediate neighborhood of the campus. So SLAC more or less closed down after working hours, while Fermilab kept going until the wee hours.

ZIERLER: Yeah. Where did you feel like more cutting-edge physics was being done? At Fermilab or at SLAC?

BJORKEN: In the 80's it was a golden age for both labs.

ZIERLER: And so what were some of the big projects at Fermilab that you were involved in?

BJORKEN: The first one for me was a neutrino experiment led by Luke Mo, Al Abashian, and Tom Nunamaker in the neutrino area. They were measuring neutrino-electron scattering and looking for a follow-up experiment. We got interested in axion searches. But it turned out that the experiment we were considering was best done at SLAC, not Fermilab. So it turned out that my Fermilab involvement with them ended up being involvement in SLAC Experiment 137. I think you already have documentation about that. The other experiment I was involved with came much later, and was influenced by my activities regarding the SSC supercollider. We called it Minimax [and it is discussed later in this interview].

ZIERLER: Can you talk about your work with the e137 project? Was that at SLAC or was that at Fermilab?

BJORKEN: I was at Fermilab commuting to SLAC. Before long, I moved out of Fermilab and back to SLAC. Shortly thereafter, I was commuting from SLAC to Fermilab to do an experiment there. Really stupid!.

ZIERLER: And what was the E-137 experiment?

BJORKEN: E137 was an axion search. Frank Wilczek, among others, proposed the things. They are neutral spinless bosons, expected to be of low mass, with a decay channel into a photon pair, or possibly an electron-positron pair. SLAC could produce them. They would penetrate the hill within which SLAC dumps its high-power electron beam. Beyond the 200 meters of the hill was 200 meters of valley, where the axion might decay while flying through the air. Beyond that valley was a hill at the east edge of the SLAC site, where we put an updated version of Luke et. al's Fermilab detector to catch the products of the decay. When we made the proposal, there was a rather good window of opportunity for a discovery. During preparation of E137, much of that window got closed by people who looked at astrophysical phenomena and other things. However, it turns out that certain kinds of dark matter could be sensitive to the E137 data. So there has been a rebirth two or three decades after E137 was run. I have been a small part of that rebirth. [But that part of the story comes later in this transcript.]

ZIERLER: Can you talk a little bit about the big debate surrounding high energy physics in the 1980s? What was that debate?

BJORKEN: There was a big debate on what kind of major HEP project should be the follow-on to Fermilab. The future for Europe was clearly to put a proton ring into the huge LEP tunnel. Doing things within the Fermilab site did not look at all competitive. The answer for Leon and many others in the HEP community was the SSC project. While the national consensus was to go ahead with it, I was a contrarian. I argued that it you built an upgrade at Fermilab and did it fast, you could get on the air well before CERN could build their LHC. There were other contrarians within the Fermilab community, and we put together an alternative called the "dedicated collider" (DC). I invested quite a lot of political work in that---more than is usual for me. Leon of course was not amused. But he was respectful.

ZIERLER: Why do you think that was? I mean, if the idea was that we really needed to compete with CERN, shouldn't there have been a groundswell of political support to complete the dedicated collider?

BJORKEN: I think that the public climate for the support of science was better then than it is now. The SSC initiative almost succeeded. There are books out there which analyze the nature of the failure. Part of it can be blamed on the physics community, and part of it was just plain politics, which itself is never very simple. I personally do not want to entertain that question with any seriousness.

ZIERLER: What was your dream for the dedica-- I mean, not yours personally, but the scientific dream. What was it that the dedicated collider was supposed to be able to accomplish?

BJORKEN: I think the DC might well have discovered the Higgs boson. I would have to go back to the proposal to lay out the physics agenda for you. But a Higgs search was for sure part of the program. Although the DC was not a Great Leap Forward, it was probably good enough to find the Higgs.

ZIERLER: Now, the fact that the dedicated collider was never actually completed, did that relegate the United States to secondary status with regard to CERN? Or was the United States able to make up for this in other ways?

BJORKEN: The Dedicated Collider proposal never got off the ground. It was the SSC which did get off the ground, but crashed and burned. That obviously hurt the US program a great deal.

ZIERLER: And continues to this day. I mean, that legacy remains with us, is that fair to say?

BJORKEN: You are the historian, I prefer to let you answer that one.

ZIERLER: Okay. Can you talk about your entrée into studying the properties of the central plateau region of phase space?

BJORKEN: Actually, we already discussed that (my plumbing paper). Unfortunately, the idea went nowhere, and that disappoints me to this day. The experts of course do very well with sophisticated computer-based programs.

ZIERLER: And when was it when you developed an interest in spectroscopy and decay properties of hadrons containing heavy quarks?

BJORKEN: That occurred in the late eighties at Fermilab.

ZIERLER: Can you talk about your worked related to the CKM matrix?

BJORKEN: That was in the same period.

ZIERLER: And then in--

BJORKEN: In those days the descriptive language used to describe CKM mixing was rather clumsy, involving several equations and/or plots. I came up with a pictorial version involving nothing more than drawing a triangle on a piece of paper. I remember having to urge people to just “draw the unitarity triangle.” Isi Dunietz and I wrote it up, not realizing that the idea was already in the literature. Ling Lie Chau had invented it, but her work had gained no traction at all.

ZIERLER: Now, in 1989, you returned to SLAC. When you got to SLAC, did you start brand-new projects, or did you bring with you ongoing projects from Fermilab?

BJORKEN: I don't know the answer to that. Nothing specific comes to mind.

ZIERLER: Now, can you talk about the approval that you were granted by John Peoples for the T864 experiment?

BJORKEN: Brenda Kirk (Tom Kirk’s wife) can tell you a great story about that. Do you know them?

ZIERLER: Yeah.

BJORKEN: Do you know Brenda as well?

ZIERLER: By name.

BJORKEN: Brenda is a force of nature---a wonderful woman. I have had a close personal relationship with both Tom and Brenda for many, many years. After my wife Joanie died in 1983, Brenda took over her old job in the Fermilab guest office. So both Brenda and Tom were very involved in the inner workings of Fermilab. And they were both very close friends of John Peoples, who became director after Leon Lederman moved on. The T864 Fermilab experiment evolved from my involvement with the SSC program. After the DC died and the SSC construction began, I publicly concluded that I was mistaken, and that the SSC represented the future of high energy physics in the US. And I joined the planning sessions for the experimental program at the SSC. Before long I created a draft proposal of my own for a full-acceptance detector ("FAD") – a kind of electronic bubble chamber that could see complete events. It attracted a very small, but adventurous experimental group, with some novel ideas for the FAD experimental program. After the SSC was cancelled, we said to each other "Well, maybe we can do some of this at Fermilab." So our little group stuck together ("Minimax"). The focal point of our Fermilab proposal was a search for something called disoriented chiral condensate (DCC). It went to John Peoples. John didn't like the idea of us going into the Tevatron tunnel at intersection C, halfway between B and D, where the big boys were intently searching for the top quark. We clumsy oafs might carelessly damage the accelerator. But Brenda reported that, after a sumptuous dinner with John and wife Nancy at their house, the subject of Minimax came up. Brenda put in a strong plug for us: "After all, it's bj, yada yada ..." So in a weak moment shortly thereafter. John approved us. Brenda takes full credit. And before long, John regretted what he had done, and for sure breathed a great sigh of relief when we finally exited the tunnel.

ZIERLER: Can you talk about your, how John Streets introduced you to the internet?

BJORKEN: John Streets was our computer guru on Minimax. He was part of the Fermilab computer group and very well connected to CERN and to the Michigan people on Minimax. John, a very soft-spoken Brit, came in one day and announced that "I've installed something on our computer that you might find interesting and useful. It is called the World Wide Web." Of course it was very useful to us. We were one of the first hundred WWW websites on the planet. It is still there: just google "Minimax Fermilab" and see for yourself. But at that time I had no idea how important the WWW would be.

ZIERLER: Did you appreciate, you know, you might not have appreciated how important it was going to be societally, but did you appreciate immediately that it would have a useful impact on your research?

BJORKEN: Of course, it helped our research. Our home institutions were scattered across the country, and it kept us well-connected.

ZIERLER: And--

BJORKEN: I had no idea that the internet would change the whole world.

ZIERLER: When did you formally retire? What year was that?

BJORKEN: I think it was 1998.

ZIERLER: And what was the decision there? Were you just, were you overworked at that point? Did you feel like you had made the contributions that you had wanted to make?

BJORKEN: I was ready for retirement, and especially the personal freedom that came with it.

ZIERLER: But you've remained engaged in the field ever since?

BJORKEN: Yes, but I am not getting paid. There is an advantage to that.

ZIERLER: Right, right.

BJORKEN: If I were being paid, I would feel an obligation to do some serious science.

ZIERLER: Yeah.

BJORKEN: At present, I am choosing very important, but very speculative topics to work on. The odds of making real progress are very small. Were I being paid, I would feel very uncomfortable doing that “full time.” I remember to this day a conversation long ago with T. D. Lee, when I was still in graduate school. He argued that progress in physics occurs in many small steps. This came from one of the primary contributors of the idea of parity violation in the weak interactions. But I think he is right.

ZIERLER: Right.

BJORKEN: If you are employed and getting paid, and are really obsessed with a long-shot idea, I think that’s fine. Work on it, but keep it part time. Have something with a safer payoff in the works at the same time.

ZIERLER: Right. Can you talk about your involvement in standard model parameters and their possible correlation with the magnitude of the dark energy?

BJORKEN: I have for now set aside the dark-energy problem. Instead, I am looking at the clutter of standard-model input parameters, in particular the masses and mixings of the quarks and leptons. There are about a dozen and a half parameters to deal with. They do not look to me---and to many others---as random. The search for correlations is sometimes called in the trade as the texture problem. It is almost a half century old, and of course unsolved. I have played the game myself recently and have my own candidate list of correlations. (I presented it to an indifferent audience at Marty Breidenbach’s retirement fest last February.) Such lists comprise candidate answers to an undefined problem. Although the odds of success are very small, I am finding it a fun problem to work on.

ZIERLER: And you're not getting paid, so you don't have to answer to anybody.

BJORKEN: Exactly! I only write up my stuff as memos in pdf form, with no desire to create formal publications. I can send off my PDF’s to interested persons or to individuals who might provide helpful scientific criticism. Some of them are posted on my website (bjphysicsnotes.com). But I have been lazy and have not updated it recently. So there are a few recent ones in addition.

ZIERLER: Now, you talk about, you write about the dark energy problem. What exactly is the problem, and what do you see as potential solutions?

BJORKEN: The numerical value for dark energy density is extremely small. Why is that the case? One of the most fundamental issues in quantum field theory has to do with dark energy. The most straightforward approach to constructing quantum field theory produces far too much of it. That was in fact a great problem for the creator of quantum field theory, Paul Dirac. I experienced this first hand, because I had the privilege of spending a whole afternoon, one on one, with Dirac himself. When I was still on the job market, I was invited to the University of Texas. They were building up their astrophysics department, and their chair asked me to pay them a visit. A huge inducement was that Dirac was in town, and I could spend an afternoon with them hiking in the hills outside Austin. So of course, I went. This occurred after Sid and I had finished our textbooks. Naturally, the subject of conversation turned to QED. Dirac was famous for being soft-spoken and a man of few words. But on the subject of QED, he was voluble. He went on and on regarding its fundamental inconsistency. It was not the usual criticism regarding the well-known renormalization constants. Instead it centered on what is known as Haag's Theorem: the U matrix does not exist. In more modern vernacular, Haag's Theorem roughly translates into the statement that one should naturally expect the existence of a very large, power-law divergent, amount of dark energy. In the trade, this is handled by a technical gambit called normal ordering. But Dirac would have none of that---too artificial. Actually, during inflation, dark energy, or something very akin to it, was arguably present. That would be much closer to Dirac's comfort zone. But for some reason, that dark energy purportedly disappeared during the reheating epoch of cosmological history.

ZIERLER: Can you explain, what is the rotating mass matrix as it relates to the CKM matrix? And where do you see a solution to what you call here, "an important physics problem"?

BJORKEN: The rotating mass matrix is an invention of Hong Mo Chan, of Rutherford Lab in England, and a friend of mine. He has written a lot about it, but it has not gained much traction. It is an approach to the texture problem, and is a candidate mechanism for relating many of the mass and mixing parameters characterizing the Standard Model. I have taken a liking to it, and have my own version of it. It can be found on my website [bjphysicsnotes.com]. It is in foreground for me even as we speak. But it is still not obvious that the approach will succeed.

ZIERLER: Now, most recently, you've been working on the Kasner metric. What is the Kasner metric and what's your work on this?

BJORKEN: The Kasner metric has been around for a long time. In Kasner cosmology, some of the dimensions of spacetime expand while others contract. In standard GR it is simply a curiosity. It is a more serious option if the theory contains extra dimensions. Maybe the ordinary dimensions are expanding, while the extra dimensions are contracting. And the original Kasner version did not contain dark energy. Evidently that is an interesting thing to add in the present-day context.

ZIERLER: Mmhmm. I'd like to move on now to talking about the relative importance of prizes in the scientific profession. And I know you have some pretty specific thoughts on them. In particular on the Nobel prize. Can you explain your opinions about the usefulness or lack of usefulness of the Nobel prize?

BJORKEN: I don't want to name names, but in my graduate-school days at Stanford it became clear that there were people in the field who wanted the Prize more than they just wanted to do physics because physics is fun. And, just at Stanford, there was Bob Hofstadter. For us graduate students, he did not impress us much. It was Wolfgang K. H. (Pief) Panofsky, an intellectual giant, who provided the means for Hofstadter's prize-winning experiments. Pief ended up on the sidelines, even though he provided the accelerator that was the basis for the experiments. So I developed a dislike for the idea of doing physics in order to win prizes. To this day, I prefer to think of prizes as windfalls. It is fine if it happens, but nothing more than that. But as the years have passed, my opinion has softened somewhat. Prizes, especially the Nobel, provides advertisement of our field to the layperson in a very powerful way. I myself learn quite a bit about what happens in sister fields like chemistry and biology via the Prizes. So my attitude has softened a lot in comparison to my feelings when I was young. And I want to add that my respect for the talents of Bob Hofstadter has also grown a great deal down through the years.

ZIERLER: So what do you see as the value of advertising the field to a broader audience?

BJORKEN: We need public support.

ZIERLER: Mmhmm. Have you--

BJORKEN: There are a lot of prizes out there that are aimed at people who really don't need them, such as aging tenured professors in major universities. They don't need them for their resume. They are well paid, and have good retirement plans in place. It is a windfall, and of course a pleasure to receive. I have gotten some myself and am very appreciative of that recognition. But, especially, in this day and age, I would prefer that those people who are tempted to create a prize would aim at the younger, untenured generation. The bright, up-and-coming job hunters obviously benefit greatly by having such a thing on the resume. That is where I think the prize action should be.

ZIERLER: Well, BJ, I'd like to ask you as we're moving towards the conclusion of our wonderful discussion together, I want ask you some broader questions that you might survey the whole of your career and experience in physics. And the first one is, I wonder if there are certain concepts or fundamentals in physics that stay with you every day. That's something that you may have learned a long time ago, but that are really near to you and really, really help you understand the world. If you could think about some of those concepts that are always close to you.

BJORKEN: That is not an easy question. Certainly "Physics is Fun" is one of them. And it is a privilege to just have a career devoted to thinking about the Big Questions---and getting paid to do it. It is not obvious that society will support this kind of thing.

ZIERLER: Of all the areas in physics that you've been involved in, if you had to identify yourself as a specific kind of physicist, how would you answer that?

BJORKEN: Theoretical physicist.

ZIERLER: Mmhmm. And if--

BJORKEN: Perhaps it should be "theoretical elementary particle physics." My excursions into experiment, and into gravitation and cosmology, have been at somewhat less than the full professional level.

ZIERLER: And what do you see as some of your major contributions to the field of theoretical physics?

BJORKEN: Obviously, deep inelastic electron scattering stands out. And, to my surprise, the contributions to heavy-ion physics turned out to be well-appreciated and very useful to the practitioners. Finally, the (prize-winning!) intrabeam-scattering contribution, with Sekazi Mtingwa, to accelerator physics turned out to be much more important than I ever expected.

ZIERLER: Are there any projects or experiments that you--

BJORKEN: I think that historians will come up with the same three.

ZIERLER: Are there any projects or experiments that you were involved in that you wish you had a second chance at? Either you were involved at the wrong time or the technology wasn't there, or you had an idea that didn't come to you at the time? Does anything stand out in your memory about things that you wish you had a do-over for?

BJORKEN: I have no regrets.

ZIERLER: That's good to know, that's good to know.

BJORKEN: When I look back at my career, I feel really blessed. It has been a great run. No matter what happens in the future. I cannot complain.

ZIERLER: And I like to ask particularly in a field like yours, I'm always curious. What are some of the things at the beginning of your career that were mysterious to you personally or mysterious to the field of theoretical physics or particle physics, that now are not mysterious? That either through your own intellectual development or the development of the field, now make a lot more sense than when you started in your career?

BJORKEN: The entire Standard Model.

ZIERLER: Can you elaborate?

BJORKEN: When I entered the field, the Standard Model did not exist. Connecting weak interactions with electromagnetic theory was a true conceptual revolution. I went through a Golden Age.

ZIERLER: So you see that we're in a golden age presently?

BJORKEN: Not in particle physics now. If there is a golden age, it is in the neighbor disciplines of astrophysics and cosmology.

ZIERLER: Uh-huh, uh-huh.

BJORKEN: Things happen very slowly nowadays in particle physics. It is not only funding, politics and the like. The scale of new facilities is typically very large. There are exceptions like dark matter searches, but for the most part everything has slowed down. When the number of important developments per scientist's productive lifetime gets too low, the field is likely to stagnate. On the other hand, the opposite is happening in astrophysics and cosmology. At the end of a typical seminar, the speaker may well say: "Three years from now, we expect to have ten times the data." That may be a bit of exaggeration. But the corresponding sentence from a particle physicist could well be to change the three to twenty.

ZIERLER: Now, I want to flip that question on its head. Are there mysteries, either to you personally at the beginning of your career or to the field that you're part of, that continue to be mysterious today?

BJORKEN: The real foundations of quantum field theory are still a challenge. How dark energy fits into that issue is an open problem. The problem of interpretation of quantum mechanics also comes to mind. And it may well be mixed up with the QFT and dark energy problems.

ZIERLER: And what are the breakthroughs that are going to be req--

BJORKEN: My guess is that the Einstein/Riemann version of general relativity will be replaced by a variant more suited to unification. In particular, the relevance of non-Abelian gauge theories like QCD came too late for Einstein.

ZIERLER: Now, in these areas that you've identified where there's still work to be done, for mysteries to be uncovered, how do you understand that, the nature of that work? Is there more that ne-- do we need more imagination? Are they technological limitations? Are these limitations of funding? How do you see these limitations?

BJORKEN: They are all limitations. We have to make do with what we have. There will never be enough resources available to keep everyone happy. We should be grateful for what is out there, and make the most of it.

ZIERLER: Do you think that the unified theory is achievable?

BJORKEN: I'm an optimist. But a complete theory of everything may be a bit much. We are merely *homo sapiens*, a species that evolved on a curious planet in a curious way. We have limitations built in by evolution. It may be that to "understand everything" is like asking the family dog to learn general relativity. The brainpower simply may not be there. And the extra brainpower created by technology may not be enough either.

ZIERLER: Is that--

BJORKEN: I worry that modern technology will be misused and destroy the planet first.

ZIERLER: Now, is that to suggest that a theory of everything exists, but it's beyond us to capture it?

BJORKEN: I expect something closer to a theory of everything is in principle accessible to us scientists. But we have to behave well enough to save the planet in order to get there

ZIERLER: Yeah.

BJORKEN: Computation power will no doubt continue to grow a great deal. But getting new experimental data may be harder to get. Anyway, there is no reason to now abandon our goals.

ZIERLER: How do you see your work and the work of physicists in general as advancing humankind? And how much is it simply understanding theory for the sake of theory?

BJORKEN: It's a double-edged sword. For sure, we must take responsibility for what we create. The Bomb is enough of an Exhibit A. What I worry about nowadays is the impact of social media on civilization. For sure that is a double-edged sword. The opportunities for learning on one's own via cellphone or laptop are gigantic, even at the Wikipedia level. If you have a strong personal urge to learn, you can be a da Vinci all by yourself. However, what is happening instead is that these resources are overwhelmingly used to text and tweet. And that is creating tribalism, political divisions, and other well-documented negatives.

ZIERLER: Well, BJ, it's been an absolute delight talking with you, and I think for my final question, I'd like to ask you about your thoughts on the future. You know, both with what you might be able to see in your lifetime and beyond. What, it's a two-pronged question, what are you most fearful about for the future, and what are you most hopeful about for the future?

BJORKEN: I think I already answered that.

ZIERLER: Okay.

BJORKEN: Misuse of social media is a big concern for me.

ZIERLER: Okay. Do you see yourself more as a pessimist or as an optimist?

BJORKEN: I go both ways. I'd like to be an optimist. But, when it comes down to the question of whether humankind can grow up and save the planet in time, I am not very much of an optimist.

ZIERLER: And are you saying that mostly as a physicist or just as a person?

BJORKEN: As a person. I have no special expertise in climate change, for example. But I strongly believe that the public in general needs to be more altruistic if the planet is to be saved.

ZIERLER: Well, BJ, it's been an absolute delight speaking with you. And I really want to thank you for your time, it's been a privilege to hear your perspective and your stories over the decades, and you know, your comments are going to be a tremendous resource for historians and researchers across a variety of fields. So I really want to thank you for your time today.

BJORKEN: Thank you. It has been enjoyable for me as well.