ZIERLER: Okay. It is April 14th, 2020. This is David Zierler, Oral Historian for the American Institute of Physics. It is my great pleasure to be here virtually with Dr. John Galayda. John, thank you so much for being with me today.

GALAYDA: My pleasure. My honor.

ZIERLER: Okay, so let us first -- if you would state your current title and institutional affiliation.

GALAYDA: So, I am Project Director for the NSTXU Project at Princeton Plasma Physics Laboratory, Princeton, New Jersey.

ZIERLER: Okay. Alright. Now, we're going to rewind it all the way back to the beginning. Tell us about your early childhood and birthplace.

GALAYDA: I was born in Newark, New Jersey. Spent my infant years living pretty close to downtown Newark, in an industrial area not that far from Route 1, actually.

ZIERLER: Is this like the Philip Roth Newark?

GALAYDA: He lived on Chancellor Ave. I lived on Sherman Ave. It was an ethnically mixed neighborhood; racially mixed neighborhood. A lot of new immigrants, as were my parents.

ZIERLER: Where were they from, your parents?

GALAYDA: That's a complicated -- it's got some interesting features to it. My mother was actually born in Pennsylvania of a Slovak family. Her maiden name was Slivka, or plum. As I said, she was born in the U.S. Her mom gave birth to her outside of Pittsburgh, and then immigrated back to Czechoslovakia when she was more or less an infant, just a very young child, where she grew up in a town called Mikova. Turns out, I don't know the exact connection, but somehow, I'm distantly related to Andy Warhol's family. I missed my chance to meet him. A strange feature about that was despite his reputation, he went to church every morning. His mother lived in the basement of the fabled Factory, and he engaged her, because he liked her handwriting, in lettering certain images and prints that he made. So, I missed that whole thing. It would have been a golden opportunity to see a different side of Andy Warhol than most people get to see. Anyway, we lived in Newark until about 1956 when we moved to Irvington, New Jersey, which is surrounded on three sides by Newark, more or less. My parents bought a four-family house. We lived in one apartment and rented the other three. Irvington, New Jersey has few claims to fame. It was the center of river boat industry, which is ironic because it has no river anymore. They actually put the river in a culvert. The other ironic feature of it was it used to be called Camp Town, but when Steven Foster wrote that song, they changed the name. Finally, if you look in Portnoy's Complaint by Philip Roth, Philip Portnoy describes how he would go to Irvington Park, ice skating, in order to maybe meet *shiksas*. Irvington was like horse country like Darien, Connecticut. Not. The junk man had a horse. I'm old enough to remember the junk man coming down the street with a horse drawn wagon. But that was it. No equestrians. But that was it. I graduated Irvington High School in 1966. I took my bachelors at Lehigh University in 1970.

ZIERLER: In high school, had you already demonstrated some aptitude for math and science? Was it clear that was the direction you were going in?

GALAYDA: Yeah. I was completely enamored in science. Not the greatest mathematician. I was kind of sloppy. But I chose to go on in physics because of a high school teacher who taught our physics class. I was extremely -- I fell in love with the subject. He made it fun. His last name was Ninesling. I can't remember his first name. I decided before I really had the slightest idea what it meant to be a physicist, that I was going to be a physicist, because of my PSSC physics textbook. I don't know. Maybe I fell in love with the right thing for the wrong reason, but that's what happened. I embarked in a Bachelor of Arts degree, rather than a Bachelor of Science degree in physics, which meant that I took different electives. I didn't take some of the engineering courses that were normally required. I took a little more math than the engineers took, or a different flavor of math: analysis ~~that engineers took~~, and generally enjoyed it, and did all right, I guess. I applied to graduate school and went to Rutgers University, and passed my qualifying exam there. That was administered in the summer after the first year. I hit sort of a philosophical crisis as to what I was going to do with my life. I wanted to do something -- I studied physics, that's fine. I wanted to do something that could have noticeable impact on human lives in ten or twenty years. So, apart from loving the beauty of the physics, I thought I wanted to do something kind of useful. I didn't want to work on something that might or might not pay off in many, many decades.

ZIERLER: Did you enter Rutgers directly from undergraduate?

GALAYDA: Yes.

ZIERLER: Was the draft a consideration for things that you were deciding about what to do? Would you have taken time off if not for the draft? This would have been, what, 1970?

GALAYDA: Yes. That's a good question. I did take my physical and flunked it. I had already applied for graduate school, and got a ~~1y~~(1-Y), because of a knee injury. Didn't, in the end, figure into -- that's right, I forgot all about the exemptions. I did go out for my physical, of course. I remember my number, 99, which people my age might remember as well. And so on. That summer I got married on July 4th, and my wife is downstairs -- same one. After passing my qualifiers in graduate school, like I said, I had this sort of decision point at which I had to decide what kind of research I wanted to go into. I really thought about, again, trying to do something that could have perceptible impact on society in ten or so years.

ZIERLER: Does that mean that you were gravitating toward the experimental side of physics?

GALAYDA: Yeah, experimentalist, and a little bit more applied that than, actually. At that point I gave some thought to changing schools, but at that point, also, an accelerator based fusion energy scheme and experiment got funded by university funds at Rutgers University. The P.I. for that was a guy Bogdan Maglich, who you can look up. Interesting guy. He had just started that project, and it fit the bill. It pointed towards a source of clean energy for the future, had physics, had accelerators --

ZIERLER: What was the funding for the project? Was it NSF?

GALAYDA: It started with Rutgers University funding, actually. So, it was President's funding. I think Rutgers Physics Department got a growth grant -- I wasn't paying too much attention to the funding at that point -- and some of that grant went to this experiment. So, it involved a 110-kV ion accelerator, a beam transport line with quadrupole magnets. It's starting to sound like a real accelerator. [It was] a kind of unusual form of particle storage ring. It was actually a magnet that looked like a cyclotron magnet with a field strength and profile that allowed one to steer this focused ion beam into the dead center of the magnet, and pass the dead center, and go out to the periphery again, but get refocused back to the center. So, you could arrange for colliding beams at relatively high density in the center of this arrangement. So, that occupied me through my master's degree, and I did a master's degree thesis on that subject, on space charge repulsion in that device, assuming that there was no electron neutralization. Just how high would the electric fields get in the center for the densities that would be relevant for producing fusions. It turned out the electric fields were pretty high, and it was decelerating the beam, which of course, reduced the cross section, and a bunch of other stuff. It was an exciting time. I met some great people, but I came to the conclusion that it was not going to yield a viable option any time soon.

ZIERLER: Viable option in terms of what? What energy source would it have replaced?

GALAYDA: It would have been like a plasma fusion device - a super high temperature gas or plasma, it was an arrangement of colliding beams that produced a very high density. It's like colliding beams at CERN, or something like that, except colliding beams coming from all directions into the center of this device, so as to produce fusion interactions. The difficulty was that if the beams were charged, the deuteron beams -- we were using deuterium at the time -- if the deuteron beams were stripped of electrons, they'd repel each other, and decelerate, and if they weren't stripped, they wouldn't stay, and they wouldn't be steered by the magnets. Then, the research went on to investigate whether you could neutralize the center without neutralizing the deuterons when they're away from the center, and keep them coming back, etc., and stuff like that. In the course of that effort, I met a guy from Brookhaven Lab called John Blewett, and learned a good bit about accelerators leading up to my masters. When I came to the conclusion that I really shouldn't finish my PhD in this subject, I decided that I would switch to elementary particle physics, and quantum field theory seemed like a good idea at the time.

ZIERLER: So, with this project, was the issue that it just wasn't scalable, or that it wasn't working?

GALAYDA: I would say it was not scalable. It was difficult to work. I think we had done a certain -- we had detected some neutron production on a particularly good day when the vacuum inside the device was very, very good. I think I became disenchanted with it because I just didn't think it would scale well. It wouldn't scale up to a very large power production facility. In the meantime, the project that got seed money from Rutgers University could not get follow-up funding from the federal government, and the project moved off-site. It did get some funding from a Swiss company, the name of which escapes me now -- Alusuisse, maybe -- and set up shop out on Route 1 in Princeton, not far from Princeton Plasma Physics Lab. Meanwhile, I finished my thesis in quantum field theory, came within an hour, actually -- I had an offer of a postdoc job from a Rutgers professor to go to Fermilab to work on a high energy physics experiment there.

ZIERLER: What was your thesis on?

GALAYDA: My PhD thesis was on dilation field theory, and trying to basically do quantization of a field, with propagation operators that didn't propagate in time, but propagated in 4-D Euclidean geometry.. So, the thesis itself was on a Euclidian version of quantum electrodynamics, and setting up the formalism for creation annihilation operators, and stuff like that.

ZIERLER: Now, did this represent a progression of this crisis that you were referring to before about doing something useful, or was this more sort of like you were intellectually fascinated by this topic?

GALAYDA: It was a bit of both. I thought particle physics and quantum field theory were fascinating subjects, right out at the edge of where the laws of nature were not really all that well understood. It was at a point when some very fundamental aspects of nature were not understood at all. It was an exciting time in that I was in graduate school when the J/psi was discovered. That set the particle physics guys on fire. So, it was an exciting time in the field at that time.

ZIERLER: How connected were you with the particle physicist community? Were you going to conferences? Were you in touch with other people?

GALAYDA: As a graduate student I went to a few conferences. I went to APS meetings I spent some time at Brookhaven and Fermilab actually running shifts on experiments there. We had a succession of speakers come through both Rutgers and Princeton. We'd go out to the Institute for Advanced Study to listen to lectures and seminars. David Gross and Frank Wilczek were there at the time. It was a really exciting time. Curtis Callan. It was a really exciting time, and, you know, an elegant breakthrough in understanding of quantum field theory, and taking advantage of some things that in retrospect look straightforward. That's a sign of something really brilliant, when it's completely invisible to everybody until Callan, Gross, and Wilczek rip the tarp off.

ZIERLER: So, if you can convey experiencing these breakthroughs, how is this playing out? Is it incremental? Are there eureka moments? Is it like, "Holy smokes, guess what?" How does it work when you're experiencing these massive advancements in the field as they're happening?

GALAYDA: It hit our department at Rutgers like a ton of bricks. It really galvanized the place. Rutgers had invested heavily in high energy physics, and theoretical high energy physics as well as experimental physics. So, we had a lot of new people at Rutgers. They were in communication with people at IAS, relevant faculty were in close contact with IAS, with some areas competing, and with Princeton as well. So, it was a really interesting time. A lot of speakers from other laboratories were coming through, each one with a new bright idea, and a new insight.

ZIERLER: What was the impact on theory for this? Were theories getting upended? Were there arguments that were being resolved? Or was the discovery so new that it was just a new day?

GALAYDA: I think the interesting thing about [non-Abelian gauge theories] [00:22:58] and renormalization group was that one could take a field theory that was hard to calculate, complicated, and yet, by using very powerful scaling rules, come to the conclusion that the forces that bound quarks, if they were gluons would actually become weaker at short distance, which is just what was observed, and was quite a mystery at the time. The understanding of the proton form factor came a few years earlier, and there seemed to be all these loosely bound, or unbound little particles inside the nucleon. How could that be, because we can't get them out? So, shebang, this field theory seemed to offer an insight into how that could be -- how they could be bound so tightly and yet act like they're unbound inside the nucleus. So, it was an aha moment, but not like an aha, it's all that simple. It was still pretty complicated. But within that community, of course, it was a watershed moment. So, that was something to look back on. So, finishing graduate school --

ZIERLER: What year did you defend?

GALAYDA: 1977, I think. I think my degree got awarded in the next calendar year.

ZIERLER: Who was your advisor?

GALAYDA: A guy named Claud Lovelace was my PhD thesis advisor.

ZIERLER: Was he working in the same field?

GALAYDA: Yeah, he was a particle theorist and phenomenologist, who had come to Rutgers from CERN, and before that from Imperial College, I think. His thesis advisor was Abdus Salam.

ZIERLER: That's a good grandfather to have.

GALAYDA: Well, he left without his PhD, and got a job at CERN, and thrived anyway. So, he was a very special guy. Okay, so I finish my thesis. I could go back to experimental work at Fermilab. I had that option. I remained in contact with John Blewett at Brookhaven and asked him about opportunities there. He mentioned a project to build a light source at Brookhaven National Laboratory, the National Synchrotron Light Source, which I had learned in a colloquium at Rutgers given by a guy named Herman Winick, what a light source was, why it was such a great thing, and what it could be used for. That sounded like it really could -- the research that came out of a device like that could really affect people's lives on a ten year or so timescale. You really could understand proteins. You could perhaps understand how drugs work on them.

ZIERLER: So, this is kind of responsive to that crisis you were feeling as a master’s student, it sounds like.

GALAYDA: Exactly.

ZIERLER: So, what was so promising about it?

GALAYDA: You know, some of the experiments that Winick described were biological imaging, X-ray absorption fine structure, really understanding the binding together of atoms into molecules, and understanding chemistry in action. It was just a lot closer to real applications.

ZIERLER: Real applications, specifically, it sounds like, in the health sciences.

GALAYDA: Health science; material science; understanding how semiconductors do their thing; understanding the strength of materials and where that comes from; structure of alloys. Just large numbers of applications by virtue of having a very intense and tunable X-ray beam that you can condition and bring to focus on a sample at any wavelength you like on a rotating cathode machine, or something like that, which puts out X-rays in a pretty spiky spectrum. You just get any X-ray energy you want. At the time, a project to build such a light source -- actually, two of them: one funded by NSF, and one funded by AEC at Brookhaven - that's where I went. SLAC and Stanford, as you know, there's a whole other story to tell about the X-ray beam line on the side of the SPEAR ring, and what life was like there. Looking back, that was a pivot point in SLAC's history, and led to an entirely new future. Winick is nothing if not a salesman, so he made it sound really, really great.

ZIERLER: What was the pitch?

GALAYDA: Just that it's like having a Swiss Army knife. It looks like an accelerator, and you can do biology; you can do material science; you could do solid state physics; you could do gas kinetics; you can identify trace elements in matrix with other materials by virtue of X-ray fine structure absorption, and things like that. You can even solve murder cases…

ZIERLER: So, what made it so versatile, at least in theory?

GALAYDA: It's simply that the synchrotron radiation spectrum was extremely broad and tunable. You could basically select out whatever X-ray energy you wanted and bring it to your sample with very high intensity and very high flux, unlike a typical X-ray machine in a material scientists' lab. A rotating anode X-ray machine was not tunable that way. You couldn't just get equivalent intensity at any wavelength you wanted. So, off I went, and started reapplying some of what I learned about accelerators through my master's thesis at Brookhaven, starting from the ground up. The project had just gotten funded, I guess. I think the budget was $24 million, and it was AEC funding. This project was not the center ring at Brookhaven. Another project at the time, called ISABELLE, a colliding beam particle physics accelerator had just gotten funded. They went ahead and built their tunnel, and that's another story. Now that tunnel holds a heavy ion physics machine called Relativistic Heavy Ion Collider -- RHIC. It's thriving as well in that field of physics. So, NSLS was a relatively low-budget project, but it was a major, huge expenditure on material science at the time, the funding of which was primarily oriented toward supporting principal investigators at university, and also at national labs. So, having a user facility like that brought with it a certain amount of culture shock in that this was a departure from doing an experiment in your own laboratory at your own university. You actually had to come to this thing to do an experiment there. We had a very small team including some very memorable people and a shoestring budget. We really didn't have enough money to build it right. It was built, I think, like a typical high energy physics machine, or along the lines of Robert R.

Wilson's philosophy where you build it partway, get it going, and it's maybe not reliable, maybe doesn't do what it quite promised, but if you're the highest energy machine in the world, nobody else is getting your data, so you can afford to wait. That was not the case with the user community for a light source. They did have X-ray sources that, if not so intense, were way more reliable. Material scientists, solid state physicists, and the like, are expected to publish. They can't go three or four years collecting statistics, and then publish a paper with 62 other people. They're expected to publish two or three times a year. So, if the machine is not running, and they had banked on getting beam time to do an extraordinary experiment to get really new data, it was a really bad deal. It was a bad deal for them, and they couldn't afford to wait. Nonetheless, after quite a few years, somewhere around 1984 or 1985, it suddenly started running nicely.

ZIERLER: Had more funding come in?

GALAYDA: Yes, actually, to build out the instruments. There were not a lot of experiment stations around the facility when it was first built, so there was follow-on funding to build more experiment stations. At the time that NSLS opened, private sector funded research by Bell Labs, by Exxon, could actually afford to build a beam line. They could come in with a few million dollars, and get an experiment going on the floor, and have full access to the facility. They'd be required to share it, but it was within the reach of major corporate research organizations.

ZIERLER: How was the promise of versatility and usefulness playing out?

GALAYDA: Maybe it didn't play out in the first year, but I think the number of light sources around the world speaks for the importance of the field in both breadth and impact. I think they've really proven themselves as extraordinarily useful devices. The United States has several. There are several in Europe, several in Asia. It's a real worldwide community. It's a good feeling.

ZIERLER: What was your affiliation? Did you start as a postdoc and then transfer to full-time?

GALAYDA: I was on a term appointment. I started out as an Assistant Physicist.

ZIERLER: Is that the equivalent of an Assistant Professor, like a tenured track kind of line?

GALAYDA: I think that was the idea of the title. Yes, it was a two-year term followed by a five-year term. They gave me a couple of extra years in there. The demands of constructing NSLS meant that I would be distracted from doing -- I was being ordered to do stuff rather than choosing my own research. Ultimately, I did get a tenure appointment at Brookhaven. They don't call it Professor, but they call it tenure appointment.

ZIERLER: What's the work culture? Is this like a 9 to 5 government thing? Are you going in at 3 in the morning if you want to?

GALAYDA: I go home at 3 in the morning if -- I don't know about the rest of the lab. We were a really small organization, and everybody worked pretty hard. I consider myself a somewhat inefficient worker. I always take more hours to do what other people seem to be able to do. I don't know if that's just my neurosis, but that's what it was like. So, I would work quite long hours.

ZIERLER: What's the day-to-day like on a given Tuesday afternoon? What exactly are you doing?

GALAYDA: So, at Brookhaven, moving ahead with design; designing electron optics for the booster storage ring; specifying the shape of iron pole tips for magnets that would shape the field to produce the desired focusing properties of the magnet to keep the beam in the machine. That's what the project was like. I was mostly focused on designing a booster electron optics, commissioning the booster, and designing the rest of the magnets. After that, I got involved in some of the physics, or electromagnetic properties of the RF accelerating system, and got a chance to learn about electronics and implement a beam stabilizing feedback system in the storage rings. Actually, the stabilizing feedback system was -- there were a couple of them, actually: one for the booster, one for the VUV ring, and one for the X-ray ring. What they do is electronic devices that sense the electron beam getting off axis and starting to oscillate. I could go into reasons why it would do that, mainly due to electric fields that the beam itself produces that persist until the electrons come back, and then they get kicked to a larger oscillation by their own electric fields that have persisted since the beam came around last time, , which creates more electric fields. So, one can design radio receivers to detect that motion, and then apply a force with high power amplifiers to cancel it or attenuate it -- reduce it. So, that's a feedback system to damp longitudinal and transverse motion in the bunches of the electrons. So, I did that for a while. I was involved in an experiment at Brookhaven, that actually didn't pay off, called the Transverse Optical Klystron, that was meant to use the electron beam as an amplification medium for an input laser. So, you'd shine a green laser light in. It could be used when superimposed on the electron beam as it passed through a particular kind of magnet (commonly called an “undulator magnet”) to produce little bunches physically separated by the wavelength of radiation that you're using to do the bunching. So, you could then enhance the output of the beam. Instead of the electrons all emitting randomly and out of phase, force them to radiate coherently with each other, so they feel each other's electric field. That's a sort of step in the direction of a free electron laser, which comes a little bit later in my life. We tried to get a free electron laser going at Brookhaven but couldn't quite pull it off. So, that experiment was funded by a collaboration -- or, I should say was funded on a grant proposal submitted by collaboration of two researchers from Bell Labs, and one from Brookhaven. It was Claudio Pellegrini from Brookhaven, and Rick Freeman and Brian Kincaid from Bell Labs. We worked really hard on that. We ended up having to run the electron beam at such a low energy that the lifetime was really pretty bad. It was not great for getting the experiment done. We didn't really get much out of it.

ZIERLER: Was there a single research question you were trying to answer, or were there a variety of things, and you were just looking for more information?

GALAYDA: Right. This should be categorized as applied physics. Basically, manipulating an electron beam to produce electromagnetic radiation, which is analogous to a klystron, except it requires an accelerator. So, it was a bit of a stretch. It was thought that a device like this could produce hard UV radiation, or possibly even X-rays. The earliest papers were by a guy named John Madey from Stanford University. It triggered a tremendous amount of research and manipulating electron beams to get super intense radiation, which arguably is what the LCLS and other free-electron lasers are. It was all in the general direction -- maybe a random walk in the general direction of producing coherent X-rays on a reliable basis as a research tool for any number of research applications. Okay, so that is a summary of my life at Brookhaven.

ZIERLER: What year did you leave Brookhaven?

GALAYDA: 1990. One of the Bell Labs researchers around the light source, a guy named David Moncton, another bigger than life character in the field, had taken a job as Associate Lab Director at Argonne National Laboratory. Argonne had won in a competition for construction of an X-ray light source like the European Synchrotron Radiation Facility, which was already well under construction. It was a new direction for Argonne, which had been sort of a reactor research engineering laboratory. It was a reactor laboratory when reactors were physics, and then evolved toward a reactor engineering laboratory. It had a very strong engineering workforce. Alan Schriesheim was Lab Director at the time. He had come to Argonne from Exxon and brought David Moncton to Argonne as well. So, we built an organization. We had very good funding based on NSLS having very meager funding at the start and needing a lot of fixer uppers.

ZIERLER: What was the push and pull factor in terms of transitioning from Brookhaven? Did you feel like you had maxed out what you were able to accomplish at Brookhaven? Was Argonne offering new and exciting things that you wouldn't be able to achieve at Brookhaven?

GALAYDA: It was a mixed thing. I really enjoyed working with the people at Brookhaven. They were just fantastic people. AEC, I guess was already Department of Energy at this point, had decided -- two things had happened. One, I think Brookhaven was more committed for institutional considerations at getting RHIC built, and not another light source. You know, you can't get your bowl filled too many times in the space of a few years. And Argonne was starting down this new course building from the ground up a light source. They had proton accelerators there for some time to do high energy physics, and they had a small accelerator staff there, but was handsomely funded to build a light source that would be competitive with the European

Synchrotron Radiation Facility. It was a clean sheet of paper. I could make sure I didn't make the same mistakes. I could make new, improved mistakes going forward.

ZIERLER: And where was Argonne's funding coming from?

GALAYDA: It was from DOE Basic Energy Sciences, I believe. I can't remember what year DOE split up into its programs. I believe it was already Basic Energy Sciences, at that time. It was the highest priority project of the lab site. The Advanced Photon Source accelerator organization grew from 25 to 400 by the time we got done. We actually built a lot of magnets in house, and we had to down-staff to about 200, or 220 by the time I left. It was another great experience. I had some old friends come to work there from Brookhaven. I had the opportunity to recruit a bunch of really great accelerator scientists. Accelerator people, there's a sociological thing, I think, about the relationship of accelerator physicists and researchers that use light sources. In high energy physics, the accelerator almost is the experiment. It requires close collaboration over a decade, or something, between accelerator designers and high energy experimenters.

ZIERLER: So, you're looking for team players.

GALAYDA: Yeah, there's just a certain kind of collaborative relationship. There's sociology in that, too. Light source users are so different from accelerator builders that they really don't want to know anything about the accelerator. They are focused on their own research, and probably don’t consider parts of the accelerator physics less important than the research of their competitors. That kind of thing.

ZIERLER: Yeah. Where are you recruiting from? Are you trying to get a diverse range of experience? Are you looking for postdocs, full professors? Who are you looking for?

GALAYDA: Mostly a variety of backgrounds. At Argonne, we hired some magnet builders who had gotten done building magnets at Fermilab. Among the accelerator staff, we made some great hires of PhDs from Stanford and Cornell, both of which are among a small number of outstanding accelerator research programs at university. Most accelerators and accelerator research is funded by the Department of Energy. Typically, its mandate is not to fund university-based research. It does fund all of the research necessary to build its big facilities, which is a deterrent, to some extent, for NSF to fund university-based accelerator research, I would say. So, I think it's hard to get tenure at a university if you're an accelerator guy. There are a few examples of universities with strong programs: Cornell, Maryland, Stanford, Stony Brook, UCLA, now Arizona State U and Colorado State U. I have a colleague at Arizona State U who's building a really nice program. I think you'd find that there are more programs in Europe, for example, at university.

ZIERLER: Just because the scale of the projects is simply too big for most universities.

GALAYDA: It tends to be like that. Of course, SLAC built SPEAR, and Cornell built CAESAR. So, these are counter examples, but yes, the field has outgrown even the biggest universities. Even if a big university would be considered by DOE, most big universities don't want the obligation that comes -- at least a few of them, that I'm aware of, don't want the obligation that comes with hosting a national user facility. They want to have -- that's just too far removed from their mission of education combined with research, as opposed to sort of poaching into national lab space. There hasn't been that much effort to get there by university. Okay, so a great run at

Argonne. My boss there, David Moncton, told me early on that if I mentioned the word "free-electron laser" to him, I'd get my ass fired. He went to a future light source conference with a few others from Argonne. I missed that conference. It was in a January. I think it was at DESY. He went there, and the subject of the conference was the TESLA X-ray Laser, as it was called at the time. Although my boss went there with great skepticism that X-ray lasers are a waste of time, he came back demanding that his organization build a free-electron laser. By a few audacious moves, he found us the money to append a free-electron laser that could lase in visible, first at green, and then up into the edge of violet, just while it's still visible. That was attached to the injector accelerators for the advanced photon force. So, the tide was turning. The fact that it was getting funded got the U.S. user community more focused on what you might be able to do with them. Got my boss at Argonne more focused.

ZIERLER: What accounted for this tide changing? What were the new interests, or the new bases for support for this?

GALAYDA: I can only speak from a very narrow point of view. Moncton came back excited about time domain X-ray experiments, where you could produce such an extremely short pulse of X-rays to actually observe in time atomic and molecular dynamics. Whereas X-ray users around light sources would always take the pulses of X-rays, the bursts of X-rays that come out of the machine and put it in what they call a monochromator. It basically throws away most of the X-rays and takes and stretches -- to make it monochromatic, you basically turn the short pulse into a somewhat longer pulse of X-rays by bouncing it off a grating which produces a succession of time delays, and stuff like that. That's what makes it monochromatic. It's the duality of time and energy bandwidth.

ZIERLER: So, what are the advantages of the new approach? What can it allow you to do or to see?

GALAYDA: Oh, you can do freeze-frame photography of atoms as they form and break molecular bonds. You can really look at chemical dynamics. You can get useful data out of small signals, or from minuscule samples. Nanocrystalline X-ray crystallography is one typical example that's used a lot, whereby light sources -- storage ring light sources have become -- among their biggest user committee are X-ray crystallographers. Membrane proteins, and other biological molecules formed into a crystal, and do crystallography. But it turns out that it can take an extremely long time to figure out how to crystallize a protein, even to make the tiniest of crystals, and they're usually of poor quality. So, the promise of X-ray lasers that actually did tip the balance of funding was the possibility of determining the molecular structure of a single atom. I mean, not a single atom. A single complicated molecule and drop them in one at a time into the X-ray beam, and get enough X-rays scattered out of that one molecule to be able to take a bunch of pictures to reconstruct that molecule, indexing each picture. A lot of progress has gone in that direction. I'm losing touch. There is still interest in ~~I don't believe there's been a tremendous amount of effort, or success in~~ literally dropping single molecules in. But minuscule crystals of a few molecules -- you know, a dozen molecules on the side of a cube, is enough to do protein crystallography with an intense source like an X-ray beam from a free-electron laser. Molecular dynamics, actually seeing atoms forming and breaking bonds in real time. Initiating the bond breakage by a laser, and then watching as the atoms move as they come apart. Where do they go? Being able to probe a sample of the interior of a Jovian sized planet by taking a regular quantum laser and blowing up a small sample, and then have the X-ray beam come in before it has a chance to explode, so that you can actually see material at very high temperature, and high density, the sort you might find in the center of a Jovian planet. And just a wide variety of only loosely bound by imagination -- or imagination only loosely bound by the physics. So, it's been an exciting time. A fast break in science. Like I said, the number of light sources around the world kind of speak for it. You find people like me or my colleagues going to work in similar facilities in Europe and Asia, South America, India, all over the place. So, it was a good field to join. I was lucky. And now I'm here, back in the fusion game.

ZIERLER: Yeah. So, you left in 1999.

GALAYDA: I left -- okay, back it up. I went to Argonne in 1990.

ZIERLER: 1990 you were there. I think '90 to '99.

GALAYDA: Right. And then I went to SLAC after that. 2001 is when I really got going there. We had slow startup of funding. That was a big culture change. It was still a high energy physics lab through and through. Since SLAC was proposing to build the Next Linear Collider, the high energy physicists were more or less unperturbed by seeing SLAC go do something else. That attitude changed when it was realized that SLAC would not be the lead laboratory in building a linear collider.

ZIERLER: So, I'll ask the same question from Brookhaven to Argonne as from Argonne to SLAC -- the push and pull. Do you feel like you accomplished everything you could have by '99 at Argonne? The next big thing was at SLAC?

GALAYDA: I think so. At the time, we had a new lab director named, Hermann Grunder, who did not see Argonne's future as being in the direction of more powerful light sources. It's pretty clear to me he was hired by University of Chicago to bring the facility that is now known as FRIB to Argonne. It was clear that if I stayed at Argonne -- Grunder himself made it clear that if I stayed at Argonne, he was going to want me to work on FRIB.

ZIERLER: And this was not palatable to you.

GALAYDA: For many reasons. If I didn't have an alternative, I would have stayed there and done that, but at that point I got a job offer to build the thing that we wanted to build. It became clear that the free-electron laser was going to be built at SLAC, the one that we had been working at Argonne to get Department of Energy interested in building there.

ZIERLER: So, who at SLAC really got that started?

GALAYDA: I think the two people who got free-electron lasers started and actually attracted interest in them were Herman Winick, and Claudio Pellegrini. Pellegrini had been interested in free-electron lasers for many years. He was one of the first authors to publish on the subject. Herman Winick was always -- just continued his -- what shall I say -- his journey towards calling attention to the capabilities of ever bigger and more versatile light sources. He had the independence of working at SSRL, which was not totally at time, way back in the day, not totally part of the DOE Laboratory. It was still run by Stanford. So, he had a certain amount of independence in that regard. Yeah, he's a world traveler, and telling everybody how great these things are, and he made his point, that he was on the right track. His interest went on to building light sources in Asia, and Jordan, and all kinds of places. He played a role in hiring me there at SLAC, and certainly one of my heroes, James McEwan Paterson was at SLAC. That's another guy who, on top of being a star in the accelerator community is just a really great guy.

ZIERLER: Now, you said that there was a cultural change coming to SLAC. What was that? Was it like West Coast, it's more laid back? What was the change?

GALAYDA: I could talk about a couple of different cultural changes. One going from Brookhaven, which was a physics laboratory, and a high energy physics laboratory primarily when I was there. I wasn't thinking quite in terms of culture while I was there. Then I went to Argonne and saw an engineering culture. The money-maker at Argonne was nuclear reactor engineering. Whereas, at the time, maybe all but the very best engineers at Brookhaven were second class citizens. At Argonne, the money makers were reactor engineers, who were first class citizens there.

ZIERLER: And the physicists were second class?

GALAYDA: They were in their own world. The high energy physicists and the material science departments were very strong, but in terms of directing the laboratory's priorities, it was a reactor lab. They were building a new kind of reactor. They were going to build it in Idaho. So, Argonne had a strong material science program, and it was complimentary to the reactor design program, too, but it was strong in its own right. Building a light source there served to diversify the laboratory and provide a research tool that the material scientists could use. I think University of Chicago was the managing contractor for Argonne, and I think the connections between University of Chicago and Argonne became somewhat less intertwined programmatically as reactor research evolved towards engineering, because University of Chicago didn't have an engineering school. But it was strong in its own right -- that is, the lab was strong in its own right, and it had plenty to do. And a light source fit the place well, particularly with regards to material science. So, we actually built a really nice free-electron laser within the Advanced

Photon Source, trying to attract funding for the big facility. But the Department of Energy finally made its decision and slammed shut the FEL program at Argonne. Shut it down and made major changes in management. Like I said, I had to choose between working on a relativistic heavy ion collider -- I should say rare isotope accelerator, and free-electron laser I got myself so excited about, and I could go to SLAC and do it. So, I got there and had a good run. I went to a lot of -- I think I mentioned Beamtimes and Lifetimes. There was a cultural change from high energy physics to a more diversified laboratory.

ZIERLER: And what was the pecking order at SLAC? Who were the first-class citizens, and who were the second-class citizens?

GALAYDA: There were a lot of really super engineers, but high energy physics, and the next linear collider was what the lab was about. It was what the lab director was about at the time. It came as quite a shock to the lab management when the entire lab budget was shifted from the High Energy Physics program of DOE to Basic Energy Sciences. That happened on very short notice. It came as, I think, a shock to the lab director at the time. But gave it a whole new future. It was disruptive. So, yeah, I think the lab's culture changed pretty quickly. Everybody got behind the LCLS, and made it work.

ZIERLER: What were the big questions that the LCLS were trying to answer? What was the big motivation?

GALAYDA: On my end, there was a document called *LCLS: The First Experiments* that posed six experiment concepts that have largely been pursued and implemented at LCLS. One being atomic dynamics, learning about highly stripped atoms and what the energy levels are. Chemical dynamics, whereby one does the freeze-frame photography analogy of watching atoms form and break molecular bonds. Protein crystallography, which has turned into -- sort of has gone into production mode. There's continuous demand. And high energy density physics. Basically, probing extremely hot, solidly dense -- dense is a solid materials after they've been heated by a laser, and probing the dynamics of the atoms inside that tiny speck of super-heated material, which is, like I said, more or less analogous to the center of a planet, ~~star~~, using the X-ray beam, in a few instants that the sample exists in that state. That's, to a good approximation, what has been done, and I think it's delivered on every one of those concept experiments. Since external lasers are essential to pursuing some of the experiments that one would like to do with the X-ray laser, laser physics and laser technology become a core competency of SLAC, too. It doesn't quite match any one person's dream, but it's turned out pretty nicely. It was great to see that transition take place. It's a little bit like saying it's great to go to the dentist. You do feel great after you've gone through it.

ZIERLER: Now, how did your joint appointment in 2005 -- you were appointed to photon science and particle physics and astro physics. Those are separate faculties, right?

GALAYDA: Right. Yeah, I was blessed with being the first joint appointment. They have things called billets, so each faculty had to give up only a half a billet to get me in there.

ZIERLER: What did that represent? Did that represent some acknowledgment that these faculties really should not have been so distinct?

GALAYDA: Yeah, I think it was an acknowledgment that accelerators were important to both.

ZIERLER: And this was not obvious before?

GALAYDA: The kinds of accelerators that each discipline required were so dramatically different that it didn't seem like a natural thing to do. The faculties think carefully about who to appoint based on what they think the future of their research programs, globally viewed, will be. They try to bring in faculty and make faculty appointments that will support a longer-range future. I think if you ask a faculty member, certainly back when I got appointed, they would say, "No, we do a worldwide search. We want to find the best of a particular kind of physicist as we can find in the entire world." But it is, to some extent, influenced by what they see is the future of their research programs. I once joked, Yo-Yo Ma is a great cellist, but the faculty doesn't need a cellist. Actually, the faculty had a cellist, but that's another story. There's always a programmatic component to decisions like that. Of course, they don't want to make it very narrow, and simply guess that one particular guy's research is going to save the laboratory. They do think about it in terms of what programs will grow. I did have the opportunity to work with some very bright accelerator faculty there, and students who chose to study accelerator physics related subjects to free-electron lasers and take their PhDs in that subject. So, there's a relatively small number of accelerator PhDs that have come out of research at SLAC, but they've all gone on to prosper. They've all done great. I did get the opportunity to teach one seminar course at graduate level in the applied physics department. Really, I did a really crappy job. At least one student went into the field, so I feel good about it.

ZIERLER: Meaning that you could have prepared more for what you were teaching?

GALAYDA: Yeah, I think I could have done a better job teaching. I only taught one course, and maybe everyone does a poor job the first time they teach a course. I hadn't taught in a long time.

So, yeah, had I taught it again, maybe I would have cleaned up my act. It was in the midst of a lot of other work on LCLS, so I didn't really devote the energy to it that it deserved.

ZIERLER: What's your last year at SLAC?

GALAYDA: Last year. Goodness. I came here in August of 2019. Started, I guess, in September, officially, and I'm still on unpaid leave of absence from SLAC. So, I haven't completely disconnected from SLAC.

ZIERLER: Do you feel like the big excitement that drew you to SLAC in the first place, do you feel like that played out more or less how you had hoped?

GALAYDA: Yeah, I think it did. It couldn't have been scripted better. It surprised me in many ways. Not everything went smoothly. Not everything changed the way I expected.

ZIERLER: What do you see as the major contributions, both yourself personally, and what SLAC generally was doing during your time there?

GALAYDA: Okay. So, personally, I had experience from Argonne in the expectations that Department of Energy had, and Basic Energy Sciences program had for big projects. Basic Energy Sciences had been funded to build spallation neutron sources, and light sources. They were really growing, and they were building the big facilities. There were certain expectations that they had. Knowing how to work with them was important. Knowing how to run the project with good control of budget was important. I think in the earlier days of SLAC the laboratory director was the principal investigator and had great discretion over the expenditures of funds. BES has always, because of the way it distributed money among relatively smaller research programs perhaps -- I don't really know -- they had a combination of Basic Energy Science's approach to managing big projects, and also the evolution of the Office of Project Assessment in DOE that set higher expectations, or shall I say professional standards in accounting, in running a project. It may sound strange, but somebody as old as I can think back to days when oversight of project budgets was much more loosely handled at DOE level. It may have come out of a time when the competition for funds within the Office of Science was less, maybe because more money was available. As time went on, expectations changed. So, I brought that experience with me to SLAC from Argonne. It was a pretty big project with what are now quite standard project management tools for tracking budget and schedule. As I've been termed with Basic Energy Science, which maybe it's SLAC, the light source there was always a bit of an orphan in the larger scheme of the laboratory. Basic Energy Sciences was aiming to bringing about a change and wanted a different approach by people that they had some calibration on, like me, I guess, to look after the project. It was a traumatic transformation of the lab for the high energy physics community at the lab, and it was pretty traumatic for me, too. You know, I must like this stuff.

ZIERLER: Being on leave from SLAC, is the idea there that you might go back, or is that more an administrative classification?

GALAYDA: It is a bit of an escape hatch. It's funny, I had an extended appointment at Argonne, too, when I went to SLAC. I had an opportunity to come back. I was on the Argonne roster for a while after I left to go to SLAC, and I did in fact have an opportunity to go back, but I never really considered that seriously. I'm in a different realm now. Princeton has accelerators, but they're not the centerpiece. Fusion device is the centerpiece, and I'm much more removed from the intuitions and instincts that I developed in the accelerator business. They don't serve me as effectively here, because there's totally different physics.

ZIERLER: I want to ask you, when you won the Free-Electron Laser Prize in 2012, what did that mean to you, both personally and professionally?

GALAYDA: Oh, goodness. One of the best parts about working in this field is to have the opportunity to work with people that I admire. I had that opportunity at Brookhaven, and Argonne, and SLAC. I can tell you that I admired the winners of the Free-Electron Laser Prize. They were always among my heroes. I never quite figured that I did enough innovative research to merit it. It was -- what am I doing here? Oh, goodness.

ZIERLER: The question is what the prize meant to you professionally and personally.

GALAYDA: Yeah. You know, these guys were my heroes. They were guys that I wanted to hang out with and be around wherever they were. Of course, they were all over the world. When I had gotten into project management, I never quite felt that I'd done enough to warrant that kind of recognition. But I didn't turn it down. It was extremely important to me. It made me feel better about everything I'd done.

ZIERLER: You couldn't have screwed up too badly if they gave you that award.

GALAYDA: I don't know. I just never felt -- like I said -- okay, go on. But yeah, it was extremely important to me. It made me relax a little bit about not having done enough. I don't know what else to say about it. Those guys are still my heroes.

ZIERLER: Let's talk about your appointment last August. First, let's unpack the name of this. There's a lot there to work with here. National Spherical Torus Experiment Upgrade Recovery. So, we'll start with the National Spherical. What does that mean?

GALAYDA: So, it's a research facility, a tokamak with external neutral beam heating accelerators that can raise the temperature of a gas inside to plasma temperatures. It can also heat the gas by electromagnetic induction by powering coils on it. The purpose of which is to create controlled thermonuclear fusion, or to do research in thermonuclear fusion, and in doing so to produce results that will be relevant to researchers at ITER, when that facility starts up. Princeton Plasma Physics Lab has got in the magnetic confinement fusion business for a long, long time. They built a succession of machines for that purpose, and STX sustained an equipment breakdown after an upgrade.

ZIERLER: This was in 2016?

GALAYDA: Right.

ZIERLER: Were you aware of that at the time?

GALAYDA: I read about it in the news. I didn't devote a whole lot of attention to it. I didn't realize at the time, it really made me realize that it really put the whole laboratory in jeopardy and made the Department of Energy question the lab's future.

ZIERLER: What does it mean that the magnet failed?

GALAYDA: Okay, so it's a magnet that's supposed to carry ~~500~~ 20 kilo amps and produce a magnetic confinement field -- it was one of the magnets that contributed to this magnetic confinement field. It's densely wound with conductor, and it developed a turn-to-turn short, because the high voltage applied to the magnet somehow found its way through the insulation, perhaps through a burr -- something as minor as a little metal burr that was not noticed in winding the magnet. The magnet coils and had to be replaced. It was not an easy replacement. It required a lot of equipment removal.

ZIERLER: So, this is a mechanical failure. This is not a theoretical failure. This isn't something that was supposed to work but didn't.

GALAYDA: It's a quality control related failure of a magnet that I think was built out in industry.

ZIERLER: What's the meaning of the time lapse between 2016, and then bringing you on 3 years later? Does that mean that they're trying to fix this thing for 3 years, and it's not working, so then you're supposed to swoop in and take care of it?

GALAYDA: I don't think so. I think they actually had a great design when I got here. They'd spent the subsequent years going over the failure and getting most of the way through the design of the upgrade of the facility before I showed up.

ZIERLER: So, forgive me. A mechanical failure, right? Why not just call the coil guy, and have him come in and fix it, and you're good to go?

GALAYDA: It was hard to get at. It caused the Department of Energy and Princeton University to take a deep dive into engineering practices of the laboratory, and they came up with areas that that deserved attention. .

ZIERLER: Oh, you mean that this ushered in a broader quality control, and this was one symptom of a larger problem, kind of thing?

GALAYDA: Right. That was the way it was viewed.

ZIERLER: Did that review play out that way? Did this suggest that there were systemic problems, or this was an isolated incident?

GALAYDA: The laboratory basically took on -- I should say Princeton and the laboratory management took on the responsibility for delving into the conduct of engineering at the laboratory, while Department of Energy monitored what they're contractor, Princeton University, was doing about it. It was a typical kind of relationship between Department of Energy and the contracting entity, in this case Princeton, responsible for running a national lab. So, it was kind of -- Princeton and PPPL carried out its responsibility to the Office of Science, to show that the science is worth further investment. Clearly, PPPL succeeded in making this case to the DOE Office of Science and to the authors of the 2019 National Academies report, "A Strategic Plan for U.S. Burning Plasma Research".

ZIERLER: To further unpack the name of the project, what's the Upgrade, and what's the Recovery? The Recovery, I assume, is fixing the magnet, and the Upgrade is making it bigger and better than before?

GALAYDA: It is still called the NSTX-U 99.9% of the time. The Upgrade is I think what they built with the magnet that broke, and the Recovery is what we're doing now. So, we're also relining the interior of this spherical chamber with heat absorbers and plasma absorbers as well, in order to take the heat loads that will come along with a sustained high temperature plasma inside that Torus vacuum chamber. So, a lot of mechanical engineering, and careful attention to those materials in extreme conditions, was necessary to build this thing to meet its more ambitious goals when it turns back on again.

ZIERLER: Where are you now in that process? Is it going to happen? Is this still up in the air if it's going to happen?

GALAYDA: Well, I think the funding is not a line item. So, the funding has been reliable, but it's not a line item. It is part of the FES budget. I guess I don't worry too much about getting funding cut, but we've certainly got -- we're off on a good start already, and I'm personally not that worried about running out of funds. We haven't needed much contingency up until this point. Now that the lab is shut down, it's going to cost us some money.

ZIERLER: What's the best-case scenario for how this plays out?

GALAYDA: I'd say, 2021 we turn on, produce first plasma, and then I think it might be time for me to retire.

ZIERLER: Crowning achievement.

GALAYDA: I don't know, but --

ZIERLER: So, you turn it on. Even if you retire at that point, what's the data? What are the discoveries? I'm asking best case scenario. This thing works, you turn it on, it's doing all of the things you want it to do. What is it that it's doing?

GALAYDA: What they would like to learn from it is plasma confined times, certain instabilities, observation, and if possible, suppression of certain instabilities in the plasma. That can disrupt it. Heavy ions can disrupt it. Can they keep it clean enough to stay hot long enough to produce fusions? It'll be a while before they actually try producing fusions, but it will be used for that purpose. Hopefully get done in time to inform the research at ITER and contribute to the knowledge that your researchers walk into the facility with. So, that's it.

ZIERLER: What cosmic comedy might you read into the fact that your career has taken you around the country and the world, and here you are back just a few exits south on the Garden State Parkway from where you began?

GALAYDA: Jersey Boy comes home. Yeah, right. I don't know. There are certain ironical aspects to this. I never really expected to be moving on from any of those laboratories, exactly. It just seems like I got a knock on the door, or something disrupted me out of a stable equilibrium, and off we went. I do have to say that just the cost of living in California was such that when I retired, I knew I couldn't live in the Bay Area, as beautiful as it is. It just wouldn't make sense economically. So, we knew we had to move.

ZIERLER: Well, I want to ask now some -- the narrative portion of the interview is complete. I want to ask a few final questions that are a little retrospective and introspective. First, given your unique vantage point of working at all of these national laboratories, I wonder if you can talk a little bit about, very broadly, what is the overall mission of having the national laboratories within this DOE structure. For all the diverse things that they're doing, what are they all working towards in terms of this deserves the support of taxpayer dollars?

GALAYDA: Right. Well, the logistical aspect of the mission is to make research tools, too expensive for one researcher or one university to create. As all research ought to be, it's an investment in a longer-range future that's not going to turn anybody a profit or turn into profitable industries on the short term. That's become more and more true as time goes by. Even the biggest corporations -- maybe IT is different -- maybe that exception, the biggest corporations don't invest in speculative research anymore. This is speculative. It's an investment without full knowledge of what might come out of it, or knowledge of whether it might ever be profitable. A historian would say in the long run that it's paid off, but it's hard to show it, actually. It takes quite a bit of effort to see how the research at a national lab actually finds its way to the prize, but it does.

ZIERLER: Because it's so incremental?

GALAYDA: Yeah. It goes in many directions, gets used in different ways, seldom one-to-one, except perhaps with protein crystallography, or something like that, where people are really developing drugs.

ZIERLER: Does it really need to have that applied element to measure success? What about just learning about how the universe works for the sake of knowledge in and of itself?

GALAYDA: I think that is extraordinarily valuable to do. I found it more motivating to me, personally, to work on something that can bring about a change for the better, on a shorter timescale, than understanding astronomy, which I think is a marvelous field, which I read about a lot for my own enjoyment.

ZIERLER: So, on that issue of bringing about change, and assessing your contributions, and your legacy, what is that change that you feel like you've helped bring about?

GALAYDA: I don't know about helping but being at one of the first purpose built light sources in the United States was a great experience. I met a lot of people there. I met researchers from industry and university there that really did some important stuff. And I met accelerator physicists and engineers who were inspiring. I got the opportunity to apply what I learned from my mistakes there at a bigger facility, which is still running -- well, is now being taken down and replaced with yet another more modern, more powerful light source at Argonne. I think the first hard X-ray free-electron laser is going to be first forever, and that feels really good.

ZIERLER: Right. If you had the opportunity, are there are do-overs that you'd grab and do differently?

GALAYDA: Yeah. There was some technical shortsightedness at Argonne that could have made the facility a better performer. It'd get too technical to describe. I think we could have worked harder on vibration suppression. In the end, I think we got the facility to work nicely because of active control of the electron beam. At LCLS, I'd say I really mishandled the management of civil construction there. Not so much in building a facility that didn't work out, but in terms of understanding the marketplace, and understanding what it means to work with a contractor, an architect, or a construction firm. I was totally naive about that. Those guys, they want to make a profit. What I mean by that is after all my studies of history and getting into socialism versus a capitalist system, a profit is -- those guys could lose their house, too. They could lose their kids' college education, too. So, I was just naive about that. Had I been smarter, or had I been luckier, or known somebody with deeper experience in managing a big sub-contractor, things could have gone better. NSLS -- I think we did the best we could. We built a pretty good ring with leftover power supplies and time when even the users didn't exactly know what they wanted. I was fortunate to be in the field when these extraordinary sources like wiggler magnets that could produce extremely powerful X-ray beams got invented in time to put to use at Brookhaven and Argonne, and of course, SLAC. The technological improvement has produced transformational ways of doing experiments at these facilities. Nice interplay of technology and applied physics. It's just been really inspiring and fascinating.

ZIERLER: I wonder if you can quantify in very rough terms, when the ball really gets moved forward, when there's a real breakthrough or discovery, how much of it is grinding out day in and day out? How much of it is about technological improvements? How much of it is budgets, and just resources that are put into a given project? And how much of it is insight? You know, a real stroke of -- genius is a hard thing to quantify, but the power of human intellect to really figure something out. If you could roughly do a pie chart, if you would, of how these things play together to push a discovery forward.

GALAYDA: I think just in terms of number of events over time, the inspirations, there's more time between them. I think in the business of light sources, Ken Green and Renate Chasman at Brookhaven wrote down why it was important to have a nice, small electron beam – a consideration that’s pretty far removed from the world of X-ray users - they didn't really think that hard in those terms for light sources. That was a breakthrough that --

ZIERLER: An intellectual breakthrough.

GALAYDA: An intellectual breakthrough in understanding, backtracking from properties that one would have to carry out a successful experiment, and having sufficient understanding of what those properties were, and which ones were important, and backtracking them into the design of a complete accelerator. The users of NSLS, their career did not take them down those lines of reasoning. I think I commented earlier that in particle physics, the particle physicists and the accelerator people work closer together. Generally not the case with light source users. The accelerator guys were coming in from left field and had to work that out. Ken Green and Renate Chasman worked that out for themselves. They produced *bright* sources, not just a garden hose of X-rays. So, that was an insight that bridged to pretty disjoint fields that has been pursued ever since and elaborated upon ever since. So, that's a pretty big deal. Then, of course, came the possibility that one could trap synchrotron radiation from the electron beam in a resonant cavity (a pair of mirrors) and force this beam to interact with its own synchrotron radiation so as to produce micro bunching in the electron beam, is at the heart of free-electron laser as described by John Madey in 1971. I met Claudio Pellegrini at Brookhaven, who saw a way to get an electron beam to form Angstrom-scale bunching without the need for a resonant cavity. He wanted to produce a really bright X-ray source, and he knew how to do it. It took some years for U.S. researchers to realize that such a source could be used to do x-ray research, instead of (or perhaps an instant before) blowing up the sample. That was another big gulf there to bridge.

ZIERLER: So, it sounds like what you're saying is that it's the intellect, the insight, that's really the driver, and then the resources and the day to day, that's what actually makes it come to fruition.

GALAYDA: It's the combination of the insight not in a vacuum, but somehow propagating out to the community and the funding agency, and having it presented in a way that more people can comprehend what this all could possibly mean. So, that can take time. I think in the case of free-electron lasers, the quantum chemical laser community thought that they could get to X-rays, and just give them a little more time, and it wouldn't be necessary to use a big fat accelerator to produce X-rays at that intensity, or temporally coherent X-rays. That may someday be true through some combination of visible light lasers, perhaps interacting with low energy electron beams. There are people working on producing X-ray lasers that way as well, that could fit in a pretty big sized laboratory room at university. In fact, there's one under construction at Arizona State. So, then they build it, and everybody comes to use it, and then there are all these great results that come out. Each tick of the clock that measures the field’s progress is somebody else's new discovery, either great or small, or Earth-shattering or not. But it goes on. Great people come to these places, and like I said, I like to be around people I admire, and I've never been let down in that regard.

ZIERLER: You have about fifty years of experience working in this area. I don't want to make you feel old, but it's a remarkable milestone. I want to ask, at the beginning of your career, entering into graduate school to now, I want to ask you both personally and also as a representative of your field, what are some of the things that truly were not understood in the early 1970s that are really understood now? And what are some things that were not understood in the early 1970s that today remain basically as poorly understood, 50 some odd years later?

GALAYDA: Hmm. That's an interesting question. That's one of the things that -- you don't get to build a facility unless you understand it in advance pretty well. You don't get the chance to even build it. You've got to prove that you understand it. So, I never got very far in trying to propose to do something that wasn't explainable as doable. Certainly, free-electron lasers were just a wild scheme, even at the time I left Brookhaven. The accelerator community was excited about it. Numerical capabilities, computer simulations, were less back then, so more depended on pencil theory. The whole idea of getting that many X-rays in one little pulse, it took a long time for researchers to get used to the idea that they could do something useful with it, instead of just blow stuff up. So, I think accelerator guys -- it's a sociological thing. The accelerator guys and high energy physics users tend to be moving together towards the forefront, and always forced to give up the old machines in order to build a new one. Light sources can stay useful for a long time. They've evolved faster than I thought, and evolved towards higher performance more than I thought, but there's always this gulf between the users. The good side of the gulf is that it's got such a diverse user community. People who can go back to their labs and do research, and their thesis advisor taught them how. The guy who hired them as postdocs knew how. You could come to light sources and use a really intense X-ray beam to do remarkable experiments. It was built brick on brick. So, that's been a great -- the way that plays out has been an edifying part of my being in this business. Other areas, there were side trips that didn't pay off, like X-ray lithography, which based on accelerators never really took off. We thought we'd be cranking out electron accelerators for everybody in order to produce X-rays for X-ray lithography and semiconductor devices, but that's never been necessary. It got a lot of people excited back in the '80s, but it never happened. I've always thought that light sources couldn't get bigger, but they keep getting bigger. There may be tabletop versions of LCLS in the future. That would be cool. Little accelerators. So, I'm kind of rambling here. I don't know whether there's a continuity of evolution from this point forward, or whether we've come to the end of the road. Do these things really get bigger? I don't think so.

ZIERLER: That's a good transition to my last question, which is forward looking, and that's whether or not you can answer that in the present. I want to ask you, both personally in your own career, and also where you see the field headed, what are the things that are exciting to you? What are the new frontiers? What are the things where a postdoc coming to SLAC today, or to PPPL today? What kinds of advice would you give him or her about, you know, this is where I see the field headed, and this is what I think you should concentrate on? This where I think the biggest impacts can be made.

GALAYDA: Well, in the light source business, while I was trying to get people excited about time domain control of the shape of the X-ray pulse by sculpting the electron beam pulse with a laser. I just talked about that. The people at SLAC have done some serious research about that, and they're actually doing it now. So, pump/probe experiments with chemical lasers in university labs has been an exciting area of research. The straightforward extrapolation for X-rays is, you know, brings in two different color X-rays, beams to bring onto a same sample, or with a controllable time delay in order to clock or steer a chemical transformation. That's actually in progress now at SLAC and the other x-ray FELs around the world as well. Other visionary things have been pushing X-ray lasers into the harder and harder X-ray range, and gamma rays. I run out of imagination as to what kind of experiment I would do with that. The photon energy is beyond the range of relevance to material science, and even plasma physics, which goes to the pretty hard X-rays, but not super, super hard X-rays. So, I'm a little out of my depth there, and I should add that I spent a lot of time telling people to tell people to tell people to do research rather than actually doing any myself. On the other hand, running a project has been fascinating too. You meet fascinating people. You run into problems that you don't get taught about in physics departments, and that are every bit as challenging intellectually. Building the organization, planning a budget, knowing what risks to take, what you can get away with and what you can't, what risk you're willing to take, and what's your escape route? It's more like real life. Gave me new respect for venture capitalists and business startups. You know, I'm always gambling with the government's money, not my own. So, that's it. I think just every aspect of, whether it be the physics or other aspects, things with other tags attached, is all really fascinating. People are fascinating. They're all working hard. They're smart. They're dedicated. That's as much of the reward as the rest. It's more than the rest. I don't long to go back to the experiments I did in the past. I long for the people that I worked with, for whom I had such great respect

ZIERLER: Well, Dr. Galayda, it's been an absolute pleasure talking with you today. I want to really thank you for your time.

GALAYDA: Don't hesitate to contact me again. Thanks for taking the time to talk to me. Now I've got to get out of my nostalgia, here, and get back to work.

ZIERLER: There you go. Well, great. Thank you so much.

GALAYDA: Thank you so much.