Interviewee: John Kormendy By: David Zierler
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ZIERLER: Okay. This is David Zierler, oral historian for the American Institute of Physics. It is May 31st, 2021. I am delighted to be here with Professor John Kormendy. John, it's great to see you. Thank you for joining me today.

KORMENDY: It is very much my pleasure.

ZIERLER: John, to start, would you please tell me your title and institutional affiliation?

KORMENDY: Yes, I'm a professor emeritus of astronomy and Curtis T. Vaughan, Jr. Centennial Chair emeritus in astronomy at the University of Texas at Austin.

ZIERLER: Now who was Curtis Vaughan?

KORMENDY: The Department of Astronomy at UT Austin is closely associated with McDonald Observatory. And McDonald Observatory has a group of supporters called the Board of Visitors. They are about 200 prominent citizens mostly in Texas but also from elsewhere in the US. They tend to be influential – business leaders, politicians, attorneys, engineers, present and former CEOs – these sorts of people. They have an absolutely wonderful interest in astronomy. They involve themselves as much as they can in our work, and they look out for our well being. We meet with them twice each year and tell them about our work. They encourage us with their support. They help us with the legislature when trouble threatens. McDonald Observatory is a separate line item in the state budget, and when financial times are hard, we are in an exposed position. Then BoV support is especially important. They help us with donations. They help us in all sorts of ways. And they really do make us feel special. We are enormously grateful to them. Curtis Vaughan was one of these people. His family business was lumber, but his involvement in the San Antonio and broader Texas community was widespread. Curtis and his wife Phyllis donated the money for an endowed Chair. The Department and University were kind enough to entrust it to me in 2000. And I've been here since the start of January 2000.

ZIERLER: Is the designation, "Centennial Chair," specific to the Vaughan endowment or are there other Centennial Chairs at UT Austin?

KORMENDY: There are other Centennial Chairs and named professorships. There are two levels of endowment. One is the Chair endowment; there are now three of these. There were two when I arrived, but a new one was created recently for the McDonald Director. There are also nine named professorships, five of which are Centennial Professorships. The Centennial Professorships and Chairs date back to the 1983 celebration of the centennial of the opening of the University of Texas at Austin. At that time, UT undertook a big fund-raising campaign to create more endowed positions – professorial positions with higher salaries and built-in financial support for research. They let people concentrate more on research and less on the infrastructure of life, which includes a lot of time spent on getting and administering financial grants that support salaries and research expenses. Or at least: that provide seed money to let people do enough so that grants are easier to get. This makes it possible to do better research, and it is attractive enough so that you can hire good people. My Vaughan Centennial Chair position is the reason why I came to Texas. The work to get Centennial endowments started in 1983, but some of the positions didn't get filled until much later. Mine was the latest, in 2000. The Centennial Professorships have smaller endowments, and they are used to reward senior and productive professors with better salaries and more research freedom.

ZIERLER: John, I'm always curious about the ways that top-flight universities categorize their departments. Sometimes physics and astronomy are together, sometimes physics and astronomy are separate, and sometimes they're even more specialized with departments such as astrophysical astronomy or things like that. How did these distinctions work at Texas and what are some of the advantages of those bureaucratic and administrative distinctions?

KORMENDY: Right. Astronomy and physics are separate departments. There are of course people in physics who do astronomy and vice versa. You can also have a joint appointment. Steven Weinberg has a joint appointment. He is also a professor of astronomy, but mainly he is a professor of physics. So at U Texas, the departments are separate. There is also a separate geophysics department, and we are slowly developing contacts with them because of work on exoplanets and exolife. But the connections with them are still less strong than those with physics, and I have not met the geophysics people. I have met a fair number of the physicists. Advantages and disadvantages are harder to judge, but on the whole, a separate Astronomy Department has the advantage that it doesn't get drawn into the conflicts for resources that inevitably happen in a broad Physics Department. Our Physics Department is very broad, which of course is good. And the subgroups in physics are friendly. But it is inevitable that there is competition for resources. In our Astronomy Deprtment, that's less visible. There are subject divisions in astronomy too -- stellar astronomy, extragalactic astronomy – those sorts of divisions. But there is probably less competition between them than one feels in a physics department. We recognize the close relation between, for example, stars and exoplants – they form together by a common process. So I rather like how we are organized. We are also very friendly with physics. There are people here like Paul Shapiro whose work is very astrophysical. He is good friends with Steven Weinberg and collaborates with him. You can also have PhD supervisors from both departments on one committee. That happens a lot. But formally and for budget purposes, we're separate.

ZIERLER: A related question, one that involves both nomenclature and real scientific distinctions -- I ask this both generationally and with regard to your educational trajectory, the schools that you went to, and your mentors. The distinctions between astronomy, astrophysics, and cosmology -- do you see these distinctions as fluid, over the course of your career, and where are the boundaries that are most useful for your own research agenda?

KORMENDY: I tend not to pay close attention to these distinctions. Let me give you an example of why I say this. In our department, we have many subjects, as I said. Within these subjects, we have people who are observers and others who are theorists and sometimes people who are both. We have instrumentalists. And we have numerical simulators, who used to be grouped with theorists but who by now have developed a culture of their own. So now, there are times in faculty meetings when you get the feeling that there is conflict between these groups. For example, theorists may want to hire more theorists, not, say, another observer. I don't feel that way. If I categorize people at all – and doing that always makes me a uncomfortable – I do so mainly by astronomical objects studied. You can be a stellar astronomer who works on stellar evolution or a stellar astronomer who observes and measures element abundances or a stellar astronomer who works on star formation (with theory or observations) or a stellar astronomer who builds the instruments that observers need. But they are all stellar astronomers, and they are even part-time exoplanets people, because after all, star and planets form together. It's great that some people specialize in theory and others specialize in observations. But these are tools that we use, right? I also don't like to distinguish people who observe with visible light or those who observe in x-rays or at radio wavelegths. The important distinction is the one between subjects.

And subjects have life histories. Early on, we don't have the technology to ask meaningful questions. Later, there is a heyday when answers come in rapidly because the technology makes breakthroughs. Later still, subjects reach a maturity in which the easy stuff has been done and the next steps are difficult. We always need to understand where each subject is, right now, in that life cycle. But I don't focus much on distinguishing between people who use different tools. I value them all. We need them all. But I don't like to make a big deal about distinctions, because the moment you have them, you have the potential for conflict.

ZIERLER: John, just for a snapshot in time, what are you currently working on right now? What's interesting to you in the field?

KORMENDY: I have been working since 2017 – since just before I retired – on a subject for which I am NOT well known. What I'm mostly known for is studies of galaxies. But for the past four years, I have worked on a book that tries to develop quantitative tools with which we can measure the impact or people's research careers on the history of their client subjects. I have to emphasize: I don't measure intelligence or likability, only impact. Why? Well, contrast the different ways that scientists do two jobs that are central to our careers. When we do research, we use the well known and agreed-upon scientific method. We use well-defined machinery to make measurements and to estimate uncertainties or errors. We try to be rigorous, quantitative, and careful. Then, in another part of our professional lives, we go to faculty meetings to talk, say, about a potential hire. Then all that rigor goes out the window. And we base our discussions and our decisions on the squishiest of personal opinions. We would never dare to do research that is so strongly based on personal opinions. I have always thought that we should do better. And there are well known metrics – numbers that measure something about careers – that we could use to be more quantitative. The trouble is that people don't know how to use them. One famous metric called the Hirsch index is always used in exactly the opposite way to what I think is best. In the end, metrics are occasionally used in these discussions to try to make them more rigorous. But it is not done well. People disagree on how it should be done … or even on whether it should be done at all. Confidence is low. I think we can do better.

So, when I was a professor at Hawai'i in the 1990s, I developed a graduate course on career planning called Judgement in Research. About ¼ of the course was about metrics that young people can use to judge how well they are doing. What chances do I have in the job market? What can I do to improve those chances? What can I do to improve my research so that it is adopted as an important contribution to the history of my subject? Students liked the course, because it demystified the "black box" of their developing careers. Quite a few postdocs and faculty members attended the courses and contributed to our discussions. Not surprisingly, some faculty members did not like the course, because it is uncomfortable to be judgmental. My answer to them is: "The real world is the only one we have to live in. We need to learn how to cope with the real world. And if we are teaching the next generation of astronomers, we need to prepare them to live effectively in the real world." OK, all that happened in the 1990s. Then the course lay fallow in Texas until in 2016, I gave a colloquium tour across various parts of the US and Canada. During such tours, you are always invited to a lunch or dinner with graduate students. And every one of those sessions ended up discussing the metrics. Because that's what students want to know. All of those students encouraged me to turn what was then a rough draft into a serious book. So – gulp – I did it. It took four years. That's what I've been working on. The book is in press, published by the Astronomical Society of the Pacific. [Note added on August 11^{th}: The book is now published both electronically and on paper.]

ZIERLER: Oh wow. That's so exciting.

KORMENDY: Yes, it worked out well. But when you take the big step from an early first try to a final version, then you realize that this work is going to be seen by people who distrust the subject or even hate it. You have to be very, very thorough. And there's another reason why you have to be careful. My job is to calibrate the interpretation of metrics that are well known and publicly available. I provide this calibration using the personal opinions held by 22 well-known astronomers about a study sample of 510 faculty members at 17 universities world-wide. These 22 "voters" naturally want their opinions to stay anonymous. And the people in the study sample have every right to want opinions about them to remain anonymous. So I had to be very careful to do this job thoroughly and transparently enough that people can understand it and believe it without actually telling anybody any numbers that apply to any person or voter. That was hard. I needed three years for the analysis and a year more for publication.

ZIERLER: John, who is the intended audience for this book, and given its obvious policy outcomes or possibilities, what would you like to see changed as a result?

KORMENDY: The core audience is everybody in astronomy. Because one of the things that I try to do is calibrate subjects against each other. It is famously hard to compare apples and oranges, right? But if you want to judge people who apply for a faculty position, you need to be able to compare people who work in different subfields of astronomy. Can you do that? Turns out that the answer is "yes", at least as far as impact is concerned. Departmental decisions on which subjects to develop are a different matter. I don't address that. But I try to be careful to calibrate different subjects and research styles – like observers, theorists, and instrumentalists – together. This works pretty well. A few subjects need tweaks, but by and large, the calibration works. It applies to anybody in any research field in astronomy, even into the not-well-defined border between physics and astronomy. So people can apply metrics to themselves, to see how their investment of their time in their careers is doing. Other people can apply metrics when they judge candidates for a job, for a prize, or for tenure. In all subjects of astronomy. And this means that my study also should interest people who study scientometrics, or more generally, bibliometrics. Those people are social scientists. And in principle, the methods that I use could be used in other fields. The calibration is specific to astronomy. But physicists, say, can see from my results what metrics work and what metrics don't work. For example, many institutions like to count published papers, because this seems like a straight-forward and uncontroversial measure of productivity. But I find that numbers of papers correlate very badly with impact. I have to conclude that paper counts are almost useless in judging impact. This is not a surprise – it is easy to publish a lot of papers that make only minor contributions. What counts most are the few papers that make history. They get used. They get quoted. So citations measure impact, but publication numbers do not. This seems obvious in retrospect, but it is not the first or even the preferred choice for many people.

OK, the details of how one would apply or calibrate metrics are different for different sciences, but the notion that metrics work and the possible ways of constructing a calibration should be applicable across essentially all physical sciences. So the audience is intended to be very broad. The second part of your question was: How do I hope that my book will be used? I hope it will be used to lend more rigor to the process of making career judgements. I would never expect that a committee – say a hiring committee – would base decisions only on metrics. But they can be a tool that wasn't well understood or well calibrated before. I hope that metrics will be useful in more holistic judgement processes that, of course, involve judging people's teaching ability, service work within a department, how will they contribute to the culture of the department, and lots more. These factors are not measured by my metric machinery.

ZIERLER: John, to zoom out a little bit from your own work, it's such an exciting moment right now in astronomy with all of the big observational projects that either are coming online or are planned in the not-too-distant future. What's most interesting to you? What are you most excited about looking five, ten years into the future?

KORMENDY: There are two ways to answer that question. What do I hope to be doing? And what is interesting overall? I'll answer the second one first. What interests me most overall is exoplanets and the search for life. Cosmology and the early Universe. Gravitational wave and "multi-messenger" astronomy, that is, astronomy using more than just photons. Also surprises – they are inherently unpredictable. Those are the subjects that look poised for breakthroughs. Cosmology is in its heyday, when results flood in at enormous rates and with spectacular quality. I think that this is likely to continue. I hope that it continues to the point where inflation can be tested better than it has been tested so far. Those are the areas that, to me, are most exciting. And you'll notice that the overlap between these areas and what I do is almost zero.

The other subject that I should have added is dark matter. I hope that our attempts to figure out what dark matter is will succeed. The caveat is that I would be surprised if the dark matter – not the dark energy, but the matter that's dark -- turned out to be just one constituent that we haven't been able to discover. In fact, we know already that it can't be just one, because neutrinos are dark matter even though they are not significant to the cosmological density. But they exist and they have mass. So they are a dark matter. I hope that the dark matter constituents will gradually be found. I have no idea how many there will be.

These are the subjects that I think will be most exciting. If I were starting my career now, I would probably work on exoplanets.

But I work on galaxy structure. This has become a mature subject. There are engineering details that we don't yet understand. I hope to be active in figuring them out. That's the answer that is specific to me. I plan to continue to work on galaxy evolution, with emphasis on supermassive black holes. That subject has been good for my career. And it continues to need work.

ZIERLER: Another generational question. Obviously, computational power is central to making sense of all of the data that's coming out of observations. Is this something that you've embraced, or is this something where you're relying on younger generations of astronomers to do the work?

KORMENDY: I mostly rely on younger generations. I'm a classical optical observer. If I have a strength, it is that, over a 50-year career, I've seen a lot of history, a lot of ideas that got developed. I've seen ideas that were wrong got discarded for good reasons. And I hope I have a fairly broad overview of the details of the structure and evolution of galaxies. I hope that my understanding is illuminated by the fact that I learned a little bit about gravitational physics from Peter Goldreich. (laughs) I hope I have some intuition about how orbits work. You know, orbits work quite differently than most people think. So I hope to use my background and history to pull pieces of the galaxy evolution puzzle together in ways that young people who have absolutely spectacular analytical muscle but not so much depth have trouble doing. That's where I see my last couple of decades – the future of my work.

ZIERLER: John, let's take it all the way back to the beginning now. Let's start first with your parents. Tell me a little bit about them and where they're from.

KORMENDY: OK. My father's name was John Kormendy – in Hungarian, Körmendy János. He was born in Vál, which is a small town on the Austrian side of Budapest. He was Hungarian. He came from a sort of post-feudal society. People had been free for hundreds of years. They owned their own properties. But they spent much of their adult lives working agriculturally on a big landowner's land, and as a result, they got parcels of land they could work for themselves. Plus the land that they owned. That was the society that my father grew up in. So he grew up as a farmer. When he was an adult, he became essentially a policeman. He was a water gendarme on Lake Balaton, the only lake in Hungary, until the Second World War, when he was at just the right age so that he was inevitably, whether he wanted to or not, sucked in. So he served in the war on the other side for a while. Nothing spectacular. I remember no stories from the front lines. But he was in the army, and at the end of the war, he ended up in one of these long marches on foot. … Remember the movie Gandhi?

ZIERLER: Yeah.

KORMENDY: After the independence of India, there is a scene in the movie in which Muslims troop in one direction and Hindus troop in the other direction, both in long marches to get from places where they are a minority to places where they are a majority. My father was in a march like that, from Hungary down through Italy and back up northward, ending up in Graz, Austria. Graz is south of Vienna, fairly close to the Hungarian border. That's where he met my mother. He was in a low-security internment camp run by the British, and they farmed people out into the community to work. My father was sent to a small farm in the country, where my mother was the local daughter. Her name was Margarete Winkler. She came from a middle-class background. They met and got married and I was born in 1948. At that time, international discussions were under way on where the dividing line would be between the part of Europe that was to be controlled by the West and the part that was to be controlled by the USSR. Those discussions hadn't finished. My parents did not want to risk being on the USSR-controlled side. As a matter of safety, they decided to leave Austria. And so we emigrated to Canada when I was about two years old. My mother was a homemaker and an artist. My father's police background wasn't much use in Canada, so he became a laborer in a factory for most of his life. In the last decade before he retired, he became a don at Brock University, which had opened in nearby St. Catharines. He was well suited to that life, being a natural authority and father figure. You get that as a policeman, right? He was very well liked as a don.

ZIERLER: What language did your parents communicate in? What was your mother tongue?

KORMENDY: (laughs) That's an amusing question. My mother tongue is German. We communicated in German when I first learned to speak. Then, as time passed and I went to school in Canada, my English got better than my German. So when I was in my 20s and 30s, we would speak a weird mixture of languages at home. My mother spoke mostly German with some English. My father spoke mostly German with some English and occasional Hungarian when he got excited. I spoke mostly English with some German. When I met my now wife, Mary, and she first visited us at my parents' home, she semi-freaked out – in a nice way – because she couldn't understand what was going on. Our conversations were happening in three languages. (laughs) In the end, as time passed, I spoke more in English, and my mother got quite good in English. With a job at Brock University, my father was pretty good in English too.

ZIERLER: Did you have any family in Canada?

KORMENDY: My mother had a cousin in Canada. I still have a second cousin in Canada. That's the only family I have in the New World. I may have relations in Graz that I don't know about. I certainly have cousins in Hungary; I met them in 1980. My parents and I visited Hungary, including Vál and Graz. I even met my father's sister. But I'm not in contact with any relatives now except for my cousin. One reason is that I don't speak Hungarian.

ZIERLER: And where did you grow up? What town in Canada?

KORMENDY: I started off in Welland, Ontario. The Welland of the Welland Canal. Later, I grew up in Fonthill, which is a small town between Welland and St. Catharines, about 15 miles from Niagara Falls.

ZIERLER: Were there any cultural or religious customs that your family had that made you feel distinctly of a European family in Canada?

KORMENDY: Sure. My parents were very strict about work. They were very focused on what needed to be done. My father worked so hard that you could almost say that he didn't know how to relax and have a good time. He could be social with friends. But if you gave him a day off, he would almost not know what to do with it. We did a little sightseeing as a family; the Niagara River – especially the gorge downstream from the falls – was a favorite place. But mainly, I was brought up to work hard. That made an enormous difference to my career. The amusing thing was that, 20 or 30 years later, my parents worried that they overdid it. Because I never ended up having children. That's in large part because of my focus on work.

More generally, I felt culturally a mix of Austrian and Hungarian. I interacted more with my mother than my father. That had an Austrian flavor. From my mother, I got Austrian stories and Austrian food. From my father, I got Hungarian stories. I remember, when I was very young, that my father would read books by Ludwig Ganghofer to practice his German. Aloud, to us all. That was evening entertainment before TV. Very Austrian. I still like those books. And at home, we continued to live a farmer's life – we had goats and rabbits and vegetable gardens. Christmas was Austrian, celebrated on the evening of December 24th. Family friends were Hungarian. But then, Welland is an area with a lot of foreigners, including many Hungarians. I sometimes felt ostracized, because kids who liked to bully thought of me as German. In the 1950s, being German wasn't welcome. And telling people that Austrian is not German didn't work.

ZIERLER: Yeah.

KORMENDY: So when I got bullied, that was the flavor of the bullying. But it … wasn't real, right? It was just an excuse to do what some kids are inclined to do anyway.

ZIERLER: Yeah.

KORMENDY: Which was to exert dominance. Anyway, I was an only child. So what I mostly felt were consequences of being an only child. They were not bad – I liked being an only child. I have always been a bit of a hermit. I learned how to live with myself, how to enjoy myself with nobody else around. And to find ways to entertain myself.

ZIERLER: Did you express any innate curiosity about the Universe or about science or nature that would foreshadow your career in astronomy?

KORMENDY: Sure. One fun pastime when I was maybe 10 to 15 years old was chemistry. I had a chemistry set in the basement. It wasn't a commercial chemistry set, I just pulled together bits and pieces. I made rockets and gunpowder. And my father built for me a roll-off-roof observatory, just in time for me to go off to university. It was finished the summer before I went to Toronto as an undergraduate, and I used it as an amateur. I was an amateur astronomer for years before I went to university, and I stayed that way for years afterwards.

ZIERLER: Would you say your high school had a strong program in math and science?

KORMENDY: It was pretty good. But it was a small school. My high school typically had three classrooms per grade. There were five grades, nine to 13. Three times five times maybe 30 students in a class means that the high school had about 450 students at a time. So it wasn't big. But the teachers were good. They were not at the level of the wonderful high schools in cities like New York. But it was much better than the average. And that certainly had an effect on me. A couple of teachers really nurtured my interest in the sciences. They were inspirational in how they behaved, how they taught, and what they liked.

ZIERLER: When you were thinking about undergraduate programs to apply to, how broad was your vista? Were you thinking about schools in the United States?

KORMENDY: No. For undergraduate work, the only place on my mind was Toronto. Toronto was – by a lot – the best school in Canada. It was the only one I was interested in. There there was never any doubt that I wanted to go to Toronto.

ZIERLER: And how specific –

KORMENDY: At that time, I hadn't decided on astronomy. I went into a program called honours math, physics, and chemistry. It was the starting program for all physical sciences. Then, gradually, over the years, I specialized in astronomy. But at the beginning, I had not picked astronomy as the ultimate destination.

ZIERLER: So as an undergraduate, by the end, you focused on asronomy.

KORMENDY: Yes. By the third year, I was an astronomy major. And in my fourth year, as an undergraduate, I took all of the first-year astronomy graduate courses.

ZIERLER: Was there a particular class or a professor who was central to that transition?

KORMENDY: Not really. It was just a matter of pruning as necessary, because every year's curriculum got more complicated. I decided gradually that it was going to be astronomy. It helped that the Astronomy Department had two telescopes on the roof of its high-rise building. Over the summers, I and a friend – also an astronomy major – were allowed to use the telescopes to take pictures. I got my introduction to using photographic plates as an undergraduate by taking pictures on the roof of the astronomy, physics, math building at the University of Toronto. Those pictures remained an Astronomy Department display for decades after that. We had a lot of fun. That gave me enough hands-on experience so I could see that I liked this subject.

ZIERLER: Did you take classes on general relativity as an undergraduate?

KORMENDY: As an undergraduate, no. As a graduate student at Caltech, yes.

ZIERLER: What about black holes? Do you have any specific memory about people talking about black holes at Toronto as an undergraduate?

KORMENDY: No. Toronto was a very classical place. You could learn about spectroscopic parallaxes of stars in enormous detail from people who really knew about them. But Torono wasn't into what was then the most modern astrophysics. So for example, there was no specialist on subjects like AGNs [active galactic nuclei] and quasars. That turned out to be useful, because the graduate courses that I took at Toronto and the graduate courses that I took the next year at Caltech complemented each other very well. Almost nothing in the Caltech courses had been in the astronomy courses at Toronto. And vice versa. One result was that later, when I had to teach astronomy to undergraduates, I understood stellar astronomy and the HR [Hertzsprung-Russell] diagram pretty well, because that was what people in Toronto were good at. But I also needed to understand the subjects that were gradually becoming my research focus, and that's what people at Caltech did. And taught.

ZIERLER: To the extent that there's that distinction between theory and observation, how did you see yourself as an undergraduate?

KORMENDY: As an undergraduate I wanted to be a theorist. Then two things gradually became obvious. I enjoy the mechanics of being an observer. I enjoy taking plates. I enjoy what astronomical objects look like, in an artistic sense. I want to understand that artistry.

And I got fascinated by galaxies. Toronto had an annual program in which four distinguished astronomers were invited to give four lectures each. One of those people was Kevin Prendergast from Columbia Univesity. And Kevin was a marvel at galaxies. His physical intuition was among the best that I've seen. We became good friends later. He gave a four-lecture course on galaxies. It included some of the first numerical simulations, and they made spiral structure that looked realistic. Meanwhile, I'd seen lots of pictures of galaxies, and their artistry grabbed me. So it became clear that I loved the observational side of astronomy. And the other thing that became obvious was I don't have the mental horsepower to become a top-ranked theorist. People usually specialize in the stuff that they do well. I specialized as an observer.

ZIERLER: On the social side of things, being an undergraduate in the late 1960s and early 1970s, what was the scene at Toronto? Had the counter-culture or anti-war movement reached the campus there?

KORMENDY: To some extent. I was completely immiscible with that stuff. Okay? I knew that it was happening, but I didn't participate. Here's a story that tells you about the sociology. I once walked from my residence to the physics building, and I ran into a couple of people who were looking for the local demonstration. I asked them what it was about. They didn't know. (both laugh) They were going to the demonstration because that's what you do. It was "cool".

ZIERLER: Yeah. (laughs)

KORMENDY: I won't say that everybody was like that. Of course, there were people who were passionate about the subjects of the demonstrations. But more people were just … into that social scene. I didn't have a social scene. (laughs) As an undergraduate, I was very, very focused. As a grad student, I was very, very focused on work. It was part of the post-WWII work ethic that I learned from my parents.

ZIERLER: Did you publish as an undergraduate, or write anything that would be publishable later?

KORMENDY: I wrote one paper with a theorist whose name was SPS Anand. He was an Indian faculty member. He had the idea of applying to rotating globular clusters of stars a numerical trick that's used in models of rotating stars. He understood the theory and taught me enough so that I could execute the numerical work. It was published as a paper in 1971, after I left Toronto. But the work was done as an undergraduate. It's a paper that almost nobody remembers.

Because the central assumption that we made was that stellar random velocities in globular clusters are the same in all directions. That isn't right. It has to be right for the gas in stars, but in globular clusters, it doesn't have to be true and it isn't true. So the trick was cute but not relevant. I also started a program with Toronto Professor Robert Garrison, who was a specialist on stars. That ended up as my first observing program when I got to Caltech. So I arrived at Caltech in the summer of 1970, before my grad classes started, and I immediately had an observing run at Palomar to do photometry of stars to allow Garrison to derive the HR diagram for a stellar association in the constellation Cepheus. The resulting paper gets used occasionally.

ZIERLER: John, what kind of advice did you get from professors or anyone else about graduate schools to apply to or specific professors to work with?

KORMENDY: I talked about this with Anand. He painted a pretty clear picture of the hierarchy of potential graduate programs in which the places near the top were, in no particular order, Princeton, Caltech, Berkeley, and Harvard.

ZIERLER: But not Toronto? The idea was to leave Toronto?

KORMENDY: Well, I ended up applying to five places. Toronto was one of them. It was my "insurance policy", because I thought I would get accepted at least in Toronto. And it would not be a disaster if I ended up at Toronto. But that wasn't what I wanted. So I applied to Caltech, Berkeley, Harvard, Princeton, and Toronto. I got accepted by all of them. And I picked Caltech, because of its repuation (especially with Anand) and because it was associated with Mount Wilson and Palomar Observatories. If I was going to be an observer, that's where I should go.

ZIERLER: Santa Cruz had not yet established the reputation that it would get later on? It was not on your radar?

KORMENDY: It was not on my radar.

ZIERLER: What about Arizona? Was Arizona a contender?

KORMENDY: No. It wasn't on my radar. It wasn't a bad place; it just wasn't on my radar. My radar was pretty much defined by Anand's experience. And that turned out to be "right on".

ZIERLER: Yeah.

KORMENDY: I was not at that time reading the literature. We didn't have the inernet, right? You couldn't browse the kind of information you can now browse. You can become an expert on almost any place now in five minutes. That stuff didn't exist then. So I had an insular life, talking basically with Anand. I wasn't unfriendly with the other professors, but I don't remember talking to any of them about this subject. And so Anand's opinion about what was a good place was all I had. He probably would have picked Princeton or Harvard above Caltech, because he was a theorist. But there's another cute story that had a lot to do with my decision. I picked Caltech because of the observing facilities. But here's the story. Only Caltech interviewed me. So I had a phone interview with them. And it went in a way that surprised me.

ZIERLER: Who was on the other line? An individual or a group?

KORMENDY: I do not remember. Strange, but I don't remember. I would not have known the person anyway. The funny thing about the inerview was this: After a few minutes, I realized that they were not asking me questions that were designed to figure out whether I was smart enough. They were asking me questions that were designed to find out whether I was crazy enough.

ZIERLER: (laughs)

KORMENDY: Hungry enough. Dedicated enough. And evidently, my craziness showed, because they took me. (both laugh) And evidently they weren't totally wrong.

ZIERLER: I wonder if you got the sense that somebody like Feynman, even though he was in a different department, would have been the apotheosis of that kind of an approach to science?

KORMENDY: If I had known him, I would probably have made that connection. But I didn't. I didn't know about Feynman until well into my graduate work.

ZIERLER: What were your impressions of Pasadena and Caltech when you first arrived?

KORMENDY: I liked Pasadena a lot. I still do. I felt at home in Pasadena very quickly. It is more high-rise now than it was then. But I remember fondly the way it was then. Caltech was a formidable place. It wasn't the sort of place where the prevailing feeling was congenial collegiality, you know? It became clear over the first months that, if you ran into somebody in the halls, the natural question to ask them was, "What have you discovered lately?"

ZIERLER: Yeah.

KORMENDY: You know, not: "How are you?" or "How's the wife?" or "How was the last vacation?" It was, "What have you discovered lately?" And that focus on discovery really was the single most important psychological influence on the rest of my life. Even now, when I ask myself, did I have a good day? That's the question that I ask.

ZIERLER: Who were the luminaries in the department at that point?

KORMENDY: I suppose there are two ways to answer that question. Which ones influenced me, and which ones were luminaries? Luminaries that didn't influence me were people like Jesse Greenstein. Except by example. Which counts for a lot, at that age. But I didn't have anything to do with him. He was a white dwarf specialist and not particularly interested in the younger students that didn't work on white dwarfs. Although I did give a substitute lecture for him once. The most obvious luminary was Maarten Schmidt. The discovery of quasars had happened not long ago. He was famous for that and for a lot of other things. He deserved the fame, and he wore it well. He was modest and gracious. But he knew his field. In fact, in those days, the best astronomers knew pretty much everything that was worth knowing. Jim Gunn was a person who could do everything. He was a theorist, he was an experimentalist, he was an observer, he was whatever he wanted to be that day. And whatever he wanted to be that day, there was nobody better. (laughs) He deserved that reputation. And he developed it even further at Princeton. But at that time, he was at Caltech. Peter Goldreich was the other luminary who I knew well. I did a one-year reading course with Peter on dynamics. And what little I know about orbits and galaxy dynamics traces to his wonderful intuition and ability. If you want to ask who is the single professor who had the biggest effect on my work, the answer is Peter Goldreich.

ZIERLER: Would he end up becoming your graduate advisor?

KORMENDY: No, he didn't. My advisor turned out to be Wallace Sargent. Peter was a theorist, half in geophysics and half in astronomy. And I didn't, as I said, believe that I had the horsepower to be a theorist. Not the kind of theorist he would want to work with. So I ended up doing an observational thesis with Wal Sargent as my advisor. And he had a big effect on me too, although I'm sure he did not know it at the time. What I learned, or at least, what I hope I learned from Wal, was taste. At that age – at the age that most students are, professionally, in their first years – the thing that's most on your mind is: “Can I develop the muscles to ask and answer astronomical questions?” Whether those questions are important or not is not on your mind yet. What's most on your mind is whether there is a path from where I am right now through a series of observations or theoretical calculations or whatever to a thesis that's publishable and a PhD that I can take with me. Can I develop the muscle? The theme of the graduate course that I developed in Hawai'i was that you shouldn't be thinking only about that. OK, feasibility is important. But you should also ask yourself, “Who is going to care?” The notion that not all questions are equally worth your time was not one that I understood then. But I learned from Wal Sargent that I should pay attention to that question. Because Wal was famous for having good taste. And while I didn't develop good taste instantly, I hope I eventually developed a little. Anyway, Wal became my thesis advisor. My PhD committee consisted of Wal Sargent, Peter Goldreich, Maarten Schmidt, Jim Gunn, and Allan Sandage.

ZIERLER: That's pretty good.

KORMENDY: It was great. I knew I was lucky. You asked about my relations with Caltech. I also had good relations with Carnegie Observatories. I actually felt more at home at Carnegie than at Caltech, and I had friends there. My best friend there in the early years was Chip Arp. I didn't much believe his science, but I liked him very much as a person. He was a class guy. And we had friendly arguments about science. Also Allan Sandage was a sort of model or hero to me. I asked him whether he'd be my advisor, and he said, "No, if we're going to stay friends, I'd better not be your advisor." But he did agree to be on my committee. I learned a lot from him, and he was very important for my career later. So Sandage was on my committee. And he was certainly *the* luminary at Carnegie.

ZIERLER: John, what was Wal working on at the time you connected with him?

KORMENDY: His work was related to my thesis. Actually, he worked on lots of things. But he had recently written papers on blue compact dwarf galaxies, as catalogued by Fritz Zwicky in his famous catalogues of blue and red compact galaxies. The blue compacts were interesting, because they included AGNs and starbursts. Both were Wal's specialties. And Zwicky's catalogs also included compact galaxies that are red. But Wal wasn't interested in them. So he farmed them out to a student that he was not very interested in either, namely me. I started a thesis on red compact galaxies. And that turned into a study of early-type galaxies in general, because I quickly discovered that elliptical galaxies are compact when they are low in luminosity. That is, there is a scaling law such that higher mass galaxies are fluffier and lower mass galaxies are denser and smaller. That's what the word "compact" means. The red compact dwarfs in Zwicky's catalogue are analogs of the Local Group dwarf (true) elliptical Messier 32. And that's how my thesis developed from what started out as a study of Wal's discarded objects to a more full-fledged study of early-type, especially elliptical galaxies. Which is what I wanted my thesis to be about, once I understood enough.

ZIERLER: What were some of the broader questions in the field, as you were developing these ideas? How did you see your thesis research being responsive to these broader questions?

KORMENDY: Well, there had been some work on elliptical galaxies. They were generally thought to be uninteresting in the sense that they were viewed as bigger versions of globular clusters. People measured surface brightness profiles. Gérard de Vaucouleurs famously found that log surface brightness varies like the fourth root of radius. That was called de Vaucouleurs' law. People had measured other parameters and scaling laws – relationships between galaxy luminosity and parameters such as size and density. But they got them wrong. Anyway, nobody knew what to do with them.

So in those days, occasional papers on ellipticals tried to make dynamical models that were clones of models of globular clusters. And that was the wrong approach, because elliptical galaxies are anisotropic. That is, the random motions of stars can be very different in different directions, something that we then thought does not happen at the centers of globular clusters. Actually, it happens in the outer parts of globular clusters, too. But the revolution in work on elliptical galaxies – the realization that anisotropy is important, and the science that that realization implies – didn't happen until after my thesis was finished. So elliptical galaxies were a backwater at the time. I didn't have the feeling that my thesis was directed at fundamental questions that astronomers were hungering to answer. I mentioned that theme to you earlier, that I learned about taste from Wal. But it wasn't a controlling factor in my choice of a thesis. Practicality was the controlling factor. Elliptical galaxies didn't really get recognized as interesting until after my thesis was finished. So I was doing my thesis research because I knew how, or because I thought I could learn how. And because Wal told me that this looked like a decent topic. But gradually, I realized that my thesis was a small part of a bigger subject. What are ellipticals like altogether? What do they tell us about galaxy evolution? I started to think about that, but it wasn't central to my research until a few years after 1976, when I got my PhD.

The revolution that "put ellipticals on the map" was started by friends of mine while I was at UC Berkeley as a postdoc. Garth Illingworth, then a postdoc at Berkeley, made the observations, and James Binney, also at Berkeley, developed the theory of anisotropic, non-rotating ellipticals. They discovered that the dynamics of elliptical galaxies are not dominated by rotation. They are controlled by anisotropy. The stars in elliptical galaxies mostly have random velocities, not rotation around the center of the galaxy, as in a galaxy disk or as in the Solar System. And the magnitudes of the random velocites are different in different directions. All this turned into quite a major revolution a few years after I finished my degree. I got involved in the revolution then.

ZIERLER: What was observing like as a grad student? Was it different from today?

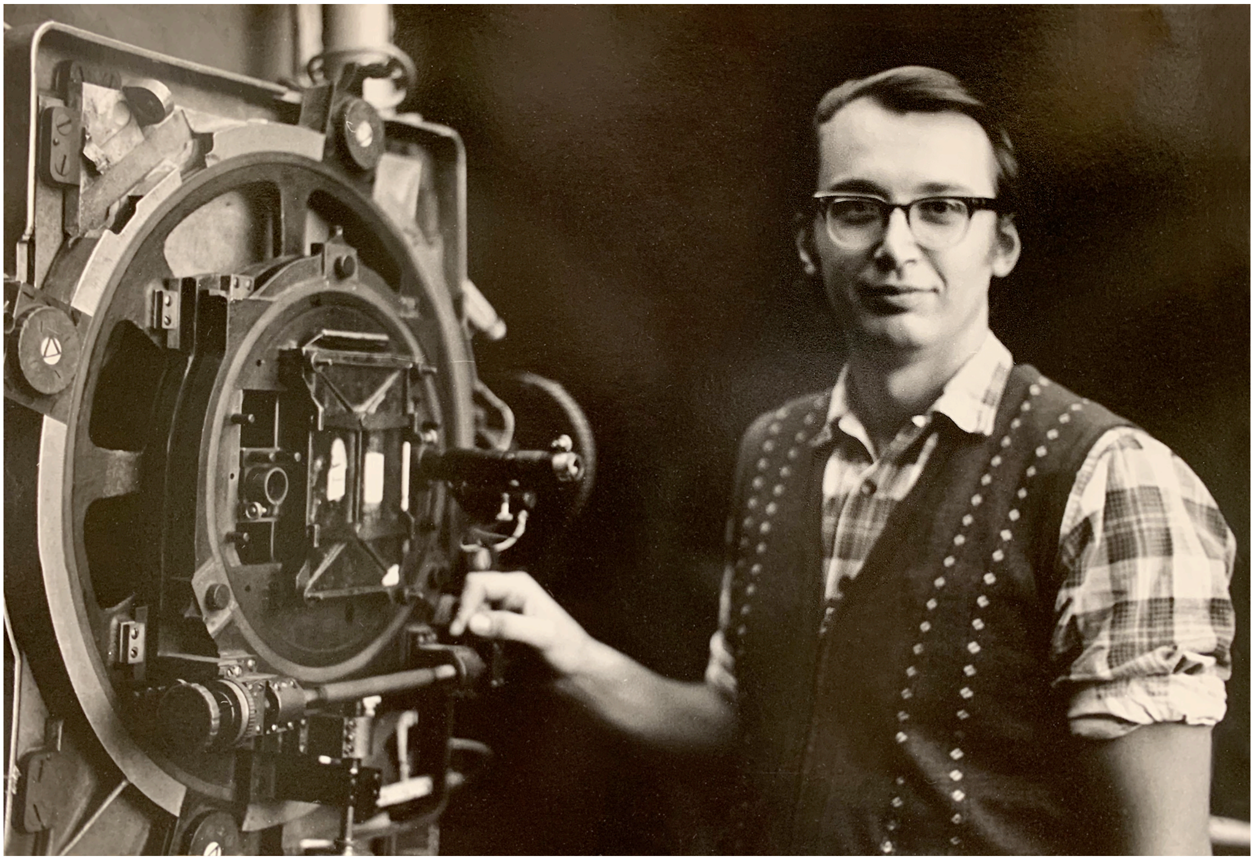
KORMENDY: Oh, yes – it was a lot different. For a start, we – even we students – got a lot more telescope time than anyone can get now. We used photographic plates. They are inefficient. You need lots of time to answer questions. And the pressure on observing time was lower, especially for telescopes on Mt. Wilson, but even at Palomar. I used the Palomar 48-inch Schmidt telescope a lot. I liked it very much. Elegant telescope. Beautiful 14-inch plates. It was a pleasure to go deep and see faint halos around galaxies. I did that work with John Bahcall, who was then at Caltech and later at the Institute for Advanced Study in Princeton. John was a theorist, but he was the person who taught me how to observe on the 48-inch. The paper that we wrote reached surface brightnesses of one-hundredth of the night sky brightness, convincingly, for the first time. It has had pretty good impact. And observing was relaxed and enjoyable. I learned to love classical music while guiding that telescope. I felt very "in the moment".

As far as doing astronomy is concerned, the difference then compared to now is that instruments were simpler and could be run by one person. A night involved only me and the "night assistant" who ran the telescope. Some telescopes I even ran myself. You had to be "up for it" – engaged and aware and careful – in part because the telescopes had no fail-safe devices against accidents. Most of my thesis work was done on the Mt. Wilson 100-inch Hooker telescope. I worked at the Newtonian focus, on a movable platform that you had to nestle around the top of the telescope. Well, the Earth turns, so the sky "rotates" around the north and south celestial poles. You have to track this by moving the telescope. A motor does most of the work, but the observer has to make small corrections to keep a guide star centered on cross hairs in a guide eyepiece. You're looking at the sky, not the dome, while you do this, right? But the dome does not move itself, and neither does the Newtonian platform. The observer has to keep moving the platform little by little to keep up with the telescope. The observer or night assistant has to keep moving the dome. Everybody has to be careful to keep the platfom from hitting the telescope and to keep the telescope looking out the dome slit at the sky. Despite care, accidents can happen. I dropped a pair of my glasses 50 feet down from the Newtonian platform to the concrete floor. Not good. More than once, something hit something, although, thankfully, nothing truly bad was my fault.

Now all this stuff is handled automatically by computers. Tests keep it safe. Even guiding is mostly done by computer, although I had to guide my black hole spectroscopy observations, in the 1980s and 1990s, by hand.

But the biggest difference, at least as it feels to me, is that you felt very close to Nature – to the sky and to Nature on the mountain. I remember one fourth of July, when I stood in the dome slit of the Mt. Wilson 60-inch dome, watching the fireworks a mile below and several miles away, down in the Los Angeles basin. The colorful starbusts were tiny. Silent. And I was alone with the sky. On other occasions, at the end of a night on Mr. Wilson, I would walk back to the "monastery" – the sleeping and eating quarters were called the "monastery" because wives were not allowed to come up. OK: between Mt. Wilson and Pasadena, there is a mountain ridge a few hundred feet below the summit, ending in a subsidiary peak. More than once, there was a low cloud deck, below the ridge to my right, with wind blowing from right to left. And the wind blew the clouds up over the ridge. Gray clouds in morning twilight fell several hundred feet, like a waterfall, to a lower cloud deck on my left. The "cloudfall" was maybe half a mile wide. Totally silent. I was completely alone with Nature and felt enormously privileged.

That doesn't happen now. Often it takes a team to run the telscope and a team to do the science. Observing is more a social phenomenon now. And more stressful, because time is valuable and complication needs concentration. I've had runs during which I never saw the telescope, because I was tied up with running the spectrograph and data system in a downstairs control room.



At the prime focus of the Mt. Wilson 100-inch telescope during my thesis observations in about 1973. I was very conscious of the history of this telescope. Sandage and Hubble used this same focus and same equipment. Hubble used this telescope to discover that the Andromeda Galaxy lies far outside our own. This proved that the Universe is much bigger than our Milky Way. The expansion of the Universe was discovered in part using this telescope. Many of the Hubble Atlas plates were taken at this focus of the 100-inch. I felt a strong sense of responsibility to carry on these traditions to the best of my abilities.

There's one more difference that is also a social phenomenon. In the 1970s, there were almost no women observers. I think it is well known that, during Margaret Burbidge's time at Caltech and Mt. Wilson and Palomar Observatories, she was not allowed to apply for telescope time. Her husband, Geoff, had to apply for the time. Geoff got scheduled to observe. But Geoff was a theorist. So he would sit off somewhere in a warm room, reading, and Margaret would observe. And I'll bet a lot that they didn't stay in the monastery. That was the situation before my time in the 1970s. I was there in transition times. I remember vividly arriving for an observing run at Palomar, where the monastery had bedrooms on two floors. The stairs to the upper floor were barred with a long chain, and hung from the chain was a sign that said "Woman – Keep out". I kid you not. Word for word. Times have changed for the better now. Rooms at Palomar come in pairs, with a bathroom between them. During my last observing run at Palomar, I shared a bathroom with a woman astronomer. And that's many years ago now.

ZIERLER: John, during those years, was Anneila Sargent around? Did you interact with her?

KORMENDY: Slightly. Anneila was a co-inhabitant of the “cave” of five or six adjoining rooms in the first basement of Robinson Hall at Caltech. My office was a tiny but happily private room in the back of that “bat cave” (as it was called) for most of my undergraduate career. I started in first year in the third basement and moved gradually up to the bat cave. Anneila was a resident, too. I guess she was a student or a postdoc, early on. I didn’t know her at the beginning. By the time I graduated, I certainly knew her, although we didn’t interact much.

ZIERLER: When you talk about the influence of Carnegie, just physically, how much of your time would you spend there versus at Caltech?

KORMENDY: Well, you do most of your work in or near your office, so I spent most of my time at Caltech. But I would visit Carnegie almost every week for maybe a half a day and talk to people there. And those people were disproportionately important to my development.

ZIERLER: Did you have a sense of the brewing tensions at that time, or was that too early?

KORMENDY: Oh no, there were all sorts of tensions that went far beyond brewing. They were boiling over. The tensions that I was aware of weren’t political – weren’t about running the institutions. They were disagreements about science and personal frictions that originated in disagreements about science. The one that most comes to mind was Fritz Zwicky against the world. Because Zwicky's office was down the hall from me in the third basement of Robinson during my first year. When Zwicky got tired of blinking supernova plates, he would come into our office and talk with us students. Or with Charlie Kowal, who was at the cubicle next to me and who was working on similar subjects as Zwicky. So I got to interact with Zwicky a lot. I wouldn’t call them conversations, right? But it was illuminating to observe his "performances". He was not well liked by the people on the second floor of the building. For reasons which I'm sure you know.

ZIERLER: Yeah.

KORMENDY: I have an autographed copy of his “red catalogue” in which describes – for example – the people on the second floor as the “high priests of astronomy”. They didn’t like that. But Zwicky got along with students pretty well. We were not important. We were “test particles” that bounced around a little as a result of the winds buffeting from Zwicky. It was interesting to watch him. And of course the other thing that I got to see every week happened when Zwicky came to the Departmental colloquium. You may have heard this many times …

ZIERLER: Please.

KORMENDY: At the end of the colloquium, often but not every time, Zwicky would get up laboriously and say, "In 1933, I told the **bastards**" … whatever the speaker had just said. And I soon realized: remarkably often, it was true. (**ZIERLER:** laughs) He did tell the "bastards". And he loved the word "bastards". It rolled off his tongue with enormous relish. It was part of his self-image that everybody else was bastards, right? And he was Fritz Zwicky. So, in terms of formative influences, Zwicky was important. I learned two kinds of things from him – how to behave and how not to behave. (laughs) I learned the importance of thinking outside the box. I didn't use those words back then, but I understood the concept. And if I understood the concept, it was more because of Zwicky than anybody else. I also understood that the way to influence the world is not to call people the "high priests of astronomy." So he was significant to my development, and he was significant to the question that you asked about tensions. Well, there were tensions between the third basement and the second floor of Robinson. And there were tensions between Caltech and Carnegie. I was less aware of those. I don't now remember what years Maarten Schmidt was Director at Carnegie. But I was aware that there were issues then.

And you can imagine, scientific tensions were central to my interactions with people at Carnegie. There was constant (but quiet) tension between Allan Sandage, who believed that galaxy spectral redshifts are cosmological, and Chip Arp, who believed that they are not. Who believed, for example, that quasars are not at the large distances implied by large redshifts but instead are nearby and ejected from galaxies. Sandage was eventually on my thesis committee, but in my early years at Caltech, I mostly talked with Arp. They were good talks. We were both interested in observations in much the same way. Arp had an artistic bent, so he tended to have the psychology of an artist more than the psychology of a physicist when he looked at data. I tried not to be that way, but I understood it and I could empathize with it. And we got along well, even though we didn't agree. We even wrote one paper together. This is one of the more amusing things about my graduate career. Chip Arp and I wrote a paper together in which we agreed on the observations but disagreed on the interpretations. So in our published paper, we had two interpretation sections, one credited to Arp and the other credited to me. We said different things. The amusing thing is that both of us were 100 % wrong.

ZIERLER: What made you so wrong? Both of you?

KORMENDY: We had both taken deep photographic plates with the Palomar 48-inch Schmidt telescope. Two of the objects that I'd worked on were NGC 7331, which is a normal giant spiral galaxy, and Stephan's Quintet, which consists of five disk galaxies close to each other in the sky and not far from NGC 7331. Actually, Stephan's quintet consists of four galaxies with the same big redshift and one more, NGC 7320, that has a redshift about 8 times smaller than the redshifts of the other four galaxies. The redshift of NGC 7320 is almost the same as that of NGC 7331.

NGC 7320 and the quintet look close together in projection, about 3 or 4 diameters of NGC 7331 apart. So the distance between NGC 7331 and NGC 7320 is not unlike the distance between the Milky Way and our companion in our Local Group, Messier 31. In other words, I thought that NGC 7320, NGC 7331, and several smaller galaxies form a Local-Group–like group of galaxies. That part of my interpretation was correct. What Arp and I had found was filaments of light between NGC 7331 and Stephan's quintet. Chip's plates and mine showed the same filaments. I thought that they were a tidal tail pulled out of one of the galaxies NGC 7331 or NGC 7320 as a result of a close encounter between them. Chip thought that the high-redshift galaxies in the quintet were ejected from 7331. We were both dead wrong. The filaments are real. But they are Galactic cirrus. (laughs) They are foreground stuff, part of the interstellar gas and dust in our own galaxy. We couldn't have been more wrong. (both laugh) The whole story is hilarious now.

ZIERLER: John, George Wallerstein has been on my mind. I was sad to hear of his passing a few weeks ago. Did you have any interaction with him at Caltech?

KORMENDY: No I didn't. He wasn't there at the time. I knew him later, and we were friends – the sort of friends who run into each other at an IAU general assembly but not at a symposium. Because we worked on unrelated subjects.

ZIERLER: Given the powerhouse nature of your thesis committee, was there anything memorable from the oral defense?

KORMENDY: The oral defense went well. It was important to know those people well. Peter Goldreich, in particular, was a terror on thesis committees. But I had done a one-year reading course with him, so I knew what was on his mind. I guessed the question that he was going to ask, and he asked exactly what I expected. So I was ready. (both laugh) He could have asked plenty of things that I wasn't ready for, but thankfully, he didn't. About the others, the main thing that I remember was an interesting question that was designed to see how well I could think on my feet about something that I didn't expect. Somebody on the committee asked me, "How do we know that stars rotate?" That's not close to my thesis subject, right? Of course, we easily see our Sun rotate when we look at sunspots. But they wanted to know about other stars. And I had to figure out in real time that you need to do something like cover up different parts of the star at different times, so you see different "redshifts" or "blueshifts" from the parts of the star that rotate away from us or toward us, both with respect to the total motion of the star. My first thought, as I fumbled around for an explanation, was to cover up parts of the star with our Moon during an occultation. But they complained – fairly – that you don't get enough time, because the Moon covers or uncovers the star too quickly. So you need to cover up parts of the star that you are studying with something that is close to it. … A binary star companion. YES! Binary stars orbit around each other. Sometimes they eclipse each other. During parts of the eclipse, one side of the star in the background is covered and later the other side is covered. And then I remembered that when you look at the velocity curves of binary stars that orbit around each other, you see the signature of rotation. There can be a big difference in the observed velocity of one side of the eclipsed star with respect to the other side. That means that the eclipsed star rotates. So my committee was happy, because I had figured out an unexpected puzzle in real time. That was the most memorable question in the whole exam. I passed.

ZIERLER: What postdoc opportunities did you have? What was most compelling?

KORMENDY: I remember getting only one offer. I'm not sure how many I applied for. But I was offered a Parisot postdoctoral fellowship at UC Berkeley. I took it for a number of reasons. One was that I already knew Berkeley well, because the photographic plates that I took in order to measure surface brightness profiles of compact and other elliptical galaxies needed to be measured with a microdensitometer, and the best microdensitometer in those days was hard to find. Caltech didn't have one. Neither did Carnegie. But Berkeley did. So I ended up making many trips as a senior grad student to Berkeley to spend a week or two at a time measuring my plates. I got to know Berkeley, and I liked the environment. It was almost as good as Caltech scientifically, but psychologically, it was much warmer and friendlier. And I wanted to live in California. So I was happy to spend two years in Berkeley.

ZIERLER: Did you see Berkeley and the postdoc as an opportunity to expand on your thesis research or to do other things?

KORMENDY: I didn't know what was going to happen yet. One of the things that can happen to students – it certainly happened to me – is that you are so focused on finishing your thesis that you are not thinking about what comes next. But now, when reconstruct my thinking from those days, I realize that I did have a feeling of where I wanted to go. I wanted to study disk galaxies. I wasn't immediately inclined to continue to measure ellipticals.

Let me try to describe my psychology at the end of my thesis and the start of my postdocs. This may sound pretentious, and I apologize for that. But it was the way my mind worked. I had the feeling that, in practice if not by design, I inherited the subject of galaxy structure and the evolution of galaxy structure from Sandage, who was one of my thesis committee members and one of my early heros. He inherited it from Hubble. And in the meantime, I was also involved in galaxy morphology as encoded in de Vaucouleurs' extension of the Hubble-Sandage types. I felt that my job was to try to understand the Hubble sequence and the de Vaucouleurs extra details. That was my career goal. I had just done ellipticals. Now I wanted to study disk galaxies.

I was still interested in spiral structure. I had originally thought about doing a thesis on spiral structure. Wal Sargent talked me out of it, and I am grateful to him for that. But I still had ideas, and they developed into a paper that had moderately good impact, written with Colin Norman.

Colin was also a postdoc at Berkeley. And, I wanted to understand bars and their evolution. That turned into one of the main themes of my career, which is the slow evolution of disk galaxies that is driven by nonaxissymmetries that rotate around the galaxy with a pattern speed that is different from the stellar orbital speed. So I started to work on this "secular evolution", this slow evolution of disks, as a postdoc. That was part of my aim to understand all the details that are identified in the de Vaucouleurs, Sandage, and Hubble types. Plus a few other details that I knew about but that were not encoded in anybody's morphological types. These are phenomena like nuclear rings. We knew about nuclear rings. But no part of the de Vaucouleurs classification calls out nuclear rings. So in Berkeley, I tried to branch out into studies of those aspects of galaxy structure that don't involve elliptical galaxies.

ZIERLER: Who did you work with at Berkeley? Who were some of the key people that were collaborating with you?

KORMENDY: In Berkeley, the only person who I collaborated with was Colin Norman on that paper about spiral structure. We quantified some hints that people had seen that global spiral structure is connected with bars and companion galaxies. By global spiral structure, I mean two arms that stay coherent over most of a galaxy. We found that, if you see global spiral structure, then the galaxy has one of three properties. You have a companion, like the well-known spirals Messier 51 and Messier 81, or you have a bar. Or – and this was the new result – you have the kind of rotation curve that allows the spiral structure to go all the way to the center. The technical term is that the galaxy has no inner Lindblad resonance. Such a resonance acts in the same way that a beach acts to water waves. People had realized that spiral structure consists of waves in the stellar density. And that Lindblad resonances are like a beach. If you have an inner Lindblad resonance, then the waves wash up on the beach and die. So, our evidence supported the idea that bars and companions drive global spiral structure. But if you don't have a bar or a companion, then you had better not have a beach, either. We showed that this is true. For this to be true, you need a low central concentration of mass, so that the rotation speed of the galaxy rises gradually outward. Colin and I wrote that paper together, and it has done moderately well. That was my only collaboration in Berkeley. I also started to work on disk secular evolution, but I didn't have a collaborator. That was still a time when you could do interesting work by yourself, not in a big collaboration. That's not easy any more.

ZIERLER: (laughs) John, I asked you earlier about your exposure or not to thinking about black holes as an undergraduate. What about in graduate school or postdoc? In other words, when did black holes start to become real for you and something that you could devote serious research to?

KORMENDY: Not when I was a postdoc at Berkeley. And not yet during my second postdoc at Kitt Peak. There, I worked on the rotation of the elliptical-galaxy-like bulge parts of galaxies. That work was in collaboraton with Garth Illingworth, who had moved to be a staff member at

Kitt Peak. At Kitt Peak, I learned two subjects that are enormously important to my work. One was digital detectors and image processing techniques. The other was spectroscopy, which I had done only a little of as a grad student. I learned that stuff from Garth. But I wasn't yet thinking about black holes. To look for black holes, you need a spectrograph on a telescope that gets wonderful image quality. I didn't have that. And I was immersed in other subjects. The time wasn't right yet – as far as I know, nobody was looking for black holes by measuring galaxy rotation curves to look for massive central dark objects.

After my time at Kitt Peak, I went to the Dominion Astrophysical Observatory or DAO in Victoria, British Columbia, Canada. I started as a staff member on the first work day of 1980. When I got there, I suddenly had institutional access to the Canada-France-Hawaii Telescope. It had something that I never had before, which is incredibly good image quality. But when I started, I could only do photometry. They didn't have a spectrograph yet. So I worked on the central parts of elliptical galaxies. And then I started to get interested in black holes, because the image quality on Mauna Kea might make them accessible. I had understood for a long time that the search for supermassive black holes was a "holy grail" of astronomy. But I didn't have the technology to look for them. So I did photometry first. Because I also knew that I would need photometry in any black hole search. Meanwhile, I waited for CFHT Corporation to finish a spectrograph. As soon as they finished, I was on the CFHT with it. The first spectrograph they finished was built by the French. CASSHAWEC was wonderfully detailed, but it was so detailed that it never became a facility instrument – it was too complicated for maintenance at almost 14,000 feet altitude. But it was on the telescope a couple of times, and I got one of my black holes in part with it. That observing run was in April of 1985. Soon afterward, we had the Herzberg spectrograph, built in Canada. It was not as sophisticated as CASSHAWEC, but it was "bomb-proof". As soon as it became available, I went into high gear, looking for black holes. That was in 1985 and 1986. So it was already five years into my time at the DAO. But it was the earliest that I had a spectrograph available in a place that had the world's best image quality.

ZIERLER: Before we get too far afield, tell me about your time in Cambridge. How did that opportunity come about and what did you do during those years?

KORMENDY: In those days, many people of my age – people like Garth and Colin – spent summers at Cambridge, in the same way that physicists in the US spend summers at Aspen. It was an intellectual gathering place where you could meet people with a variety of backgrounds and get stimulated and grow in your breadth and interests and contacts. How I got invited, I can't now remember. But I was invited to visit, and I visited twice for extended periods. I worked on the same things there during the summer that I worked on in Berkeley over the winter. I worked on secular evolution – on the evolution of galaxy disks as driven by bars and oval distortions. Part of the work with Colin was done there. I enjoyed the environment. I didn't work with anybody there, but I got to know people like Donald Lynden-Bell and Martin Rees. Martin and I didn't overlap yet in research, because I was not yet working on black holes. But I talked with him a few times, and we had dinner in groups together.

ZIERLER: Did you detect a culturally unique Cambridge approach to astronomy?

KORMENDY: I can't say that it was different. But it was very broad. It was very collegial. The Cambridge tradition of morning or afternoon coffee in which you all got together and talked about things was not something I was used to. It's not the sort of thing that would happen at Caltech, right? People were not especially social there. People at Calech were focused on what they needed to do now in order to write a useful paper tomorrow. But Cambridge, there was constant give and take. It was a theory-oriented culture, where people bounced ideas off each other. I had never experienced that as strongly, either at Berkeley or Caltech.

ZIERLER: You liked that? It was enjoyable to you?

KORMENDY: It was enjoyable. But I'm not good at small talk. The easiest way to make me uncomfortable and unable to function socially is to put me in a party. Even a party that's focused on science. I'm still not good at parties, but I was especially clumsy then. So I did my best, but I must have stood out as a scientific version of a wallflower. It's a relic of being an only child.

ZIERLER: Now in terms of the research you were doing, was Kitt Peak the place to be? Is that really where you wanted to do your next research?

KORMENDY: Yes, absolutely. Kitt Peak at that time was the world leader in the technology of developing new detectors and new ways of observing optical light. So Kitt Peak was the place where I first got to see data from digital detectors. This was before CCDs. The detectors were vidicons, so there were nasty geometric distortions produced by the magnetic fields that run the detector. But vidicon quantum efficiencies were higher than those of photographic plates, and the data were digital. We got images that we could process using the "interactive picture processing system". This "IPPS" was the first real-time image processing system, where you look at digital images on a TV screen, and you use software to process and analyze the images, and you see the results of every step immediately on a TV screen. It's an enormous improvement in data analysis. It revolutionized astronomy, and it revolutionized my life. I spent unbelievably long hours working happily with the IPPS. I keep records of the hours that I work. My all-time record of 84 hours in one week was spent using the IPPS. That's 84 hours of concentrated work, not counting time spent on the non-research stuff that makes up a work day. Plus, I added spectroscopy to my arsenal. I didn't know in advance that this was going to happen. I took the position because Kitt Peak was a hive of activity in all these ways. But I didn't picture this revolution as being just around the corner for me. So I went there for the environment. And the environment changed my life. It made possible all of my later work on black holes.

ZIERLER: What was your funding at Kitt Peak?

KORMENDY: I had a normal postdoc. But the unique thing about postdocs in those days was that I had no duties other than research. That was an enormous privilege.

ZIERLER: Yeah.

KORMENDY: Isn't that remarkable? It doesn't happen anymore. I spent ten years as a staff member at the DAO in Victoria, Canada – the Canadian national optical observatory – and the amount of service work that I had to do there in a typical year was about 2 weeks. Remarkable.

ZIERLER: Yeah, yeah.

KORMENDY: So I lived a charmed life. And I knew it was charmed, because I certainly saw other people whose lives were not so charmed. I appreciated my chances to do pure research, and I tried to do my best with them. I enjoyed research.

ZIERLER: Those advances at Kitt Peak that you were so excited about, what specifically did they allow you to do with your research that you would not have been able to do otherwise?

KORMENDY: They allowed me to measure kinematics and deduce dynamics. I already knew about the surface photometry half of what you need observationally to understand dynamics. But I didn't have the kinematic half. I depended in those days on the small number of people who measured rotational curves of galaxies. And kinematic research in those days didn't go much beyond observing rotation curves to measure masses. In those days, we had no detailed velocity fields to understand the internal workings of galaxies. So I was suddenly able to get such observatons. And I was able to get them using stellar absorption lines. That's great. Stars have relatively clean dynamics. The dynamics of orbits aren't naively intuitive. But stars are a lot simpler than gas. Because gas collides with other gas, and the consequences are complicated. So the question that I could address at Kitt Peak was, "Okay, we just learned that ellipticals don't rotate when we expected them to. That revolutionized work on ellipticals. What about bulges? Our folklore says that bulges are like ellipticals. So … do they rotate like ellipticals? Or not?" Garth Illingworth and I set out to measure the rotation of what we now call classical bulges, the kind of bulges that are like ellipticals. And we discovered that they all rotate. That was new. So, what's going on? I could now work on that collection of problems. The paper on bulge rotation was one of the most important of my career. And my job prospects.

ZIERLER: On that point, after three postdocs, was it finally time to grow up and look for a staff position or a faculty job.

KORMENDY: It's really two postdocs. I put the Cambridge visits in to my vita because they were important to my development. But they were not a postdoc; they were just summer visits. Still, after two postdocs, yes, it was time to be a staff member or a faculty member somewhere. I applied to a fair number of universities. I don't remember exactly how many. The only offer that I got was from the Dominion Astrophysical Observatory. It was a great offer, and I was more than happy to take it.

ZIERLER: Tell me about the DAO. I haven't heard much about it.

KORMENDY: The DAO is the Canadian optical national observatory, the Canadian equivalent of Kitt Peak National Observatory. The important telescope that had just come into operation recently was the Canada-France-Hawaii telescope – the CFHT. It had its own tri-national headquarters in Hawai'i. But some instrument development for that telescope happened in Canada, and that was the responsibility of the DAO. The Herzberg spectrograph was made at the DAO. Plus the DAO had telescopes of its own, in the same way that McDonald Observatory does here in Texas. It had a 72-inch reflector, which was used largely for classical stellar work. And it had a 48-inch telescope, which had one of the world's best Coudé spectrographs. The staff members at the DAO were heavily weighted towards stellar astronomy, and they were among the world's best in that subject. But the Director was Sidney van den Bergh, and he was extragalactic. I knew Sidney from my days at Toronto. I ran into him many times at Palomar. He was a friend, and we were professional colleagues. And he made it possible for me to have a good career at the DAO. He put new emphasis on extragalactic astronomy, and I was the main hire in that subject during those years. I did feel a bit isolated scientifically in the sense that there was nobody else doing my kind of work there except Sidney. There were people who worked on AGNs. But AGN specialists and the people who look for black holes with stellar dynamics don't overlap much in techniques. Each is a client of the other. But they don't need each other's observations in order to do their daily work. So I didn't have a lot of people to talk with at the DAO. But I was very used to working on my own. I had no trouble continuing that for ten years at the DAO. And I enormously enjoyed the freedom to work on whatever research I wanted, with almost no distractions.

ZIERLER: So I'm still waiting to hear about when black hole become part of your research agenda.

KORMENDY: Right. I promise we'll get there, but other stuff happened first. I had been interested in working on black holes for years. I had measured the rotation of the outer parts of bulges, and I knew that I needed to measure the central parts of galaxies to look for black holes. I wanted to do that as soon as I could use a spectrograph on the CFHT. But there wasn't one available when I moved to the DAO. So I had many observing runs on the CFHT to do photometry, and I would need that photometry for the black hole search. Meanwhile, I learned interesting things – things that were important in their own right, independent of black holes. In fact, I learned something that was a big surprise:

The start of this story is that I was able to prove something that people were not sure of before, namely that giant elliptical galaxies have nearly-constant-density central parts called "cores" in analogy with the cores of globular star clusers. Cores have parameters that satisfy scaling laws. Which led to the most fundamental new thing that I discovered during that period, something that sounds less exciting than black holes, but something that was totally unexpected. It was this: A kind of galaxy that was well known and called a "dwarf elliptical galaxy" is not in fact a small elliptical galaxy. I had been taught otherwise by people like Allan Sandage. I remember Sandage giving a talk in which he showed a scaling law for elliptical galaxies. At first, they get denser at lower luminosities. But then, below a certain luminosity, ellipticals get less dense until the smallest dwarfs are faint objects with such low densities that you can hardly see them. This complicated behavior was thought to be what ellipticals do, for reasons that were not known. That's what I learned as a student. So I thought that, for example, the dwarf ellipticals in Draco and Ursa Minor are typical little ellipticals. But it turns out that they are not ellipticals. That was what I found in my first years at the DAO, before I started to work on black holes.

I am probably best known for work on black holes. But if you ask me what discovery I am most proud of, or what I consider to be the most fundamental new thing that I have found, it is the realization that those little guys are not ellipticals. They are defunct late-type galaxies – defunct little spiral and irregular galaxies that lost their gas and stopped making new stars. And there's a story connected with that work.

ZIERLER: How did that happen? Take me blow-by-blow.

KORMENDY: Happily. It's an example of what you live for, as a scientist – the special moments when you know something that nobody else knows. It had the flavor of a small scientific revolution as discussed by Thomas Kuhn in his book, *The Structure of Scientific Revolutions*. Do you know that book?

ZIERLER: Absolutely.

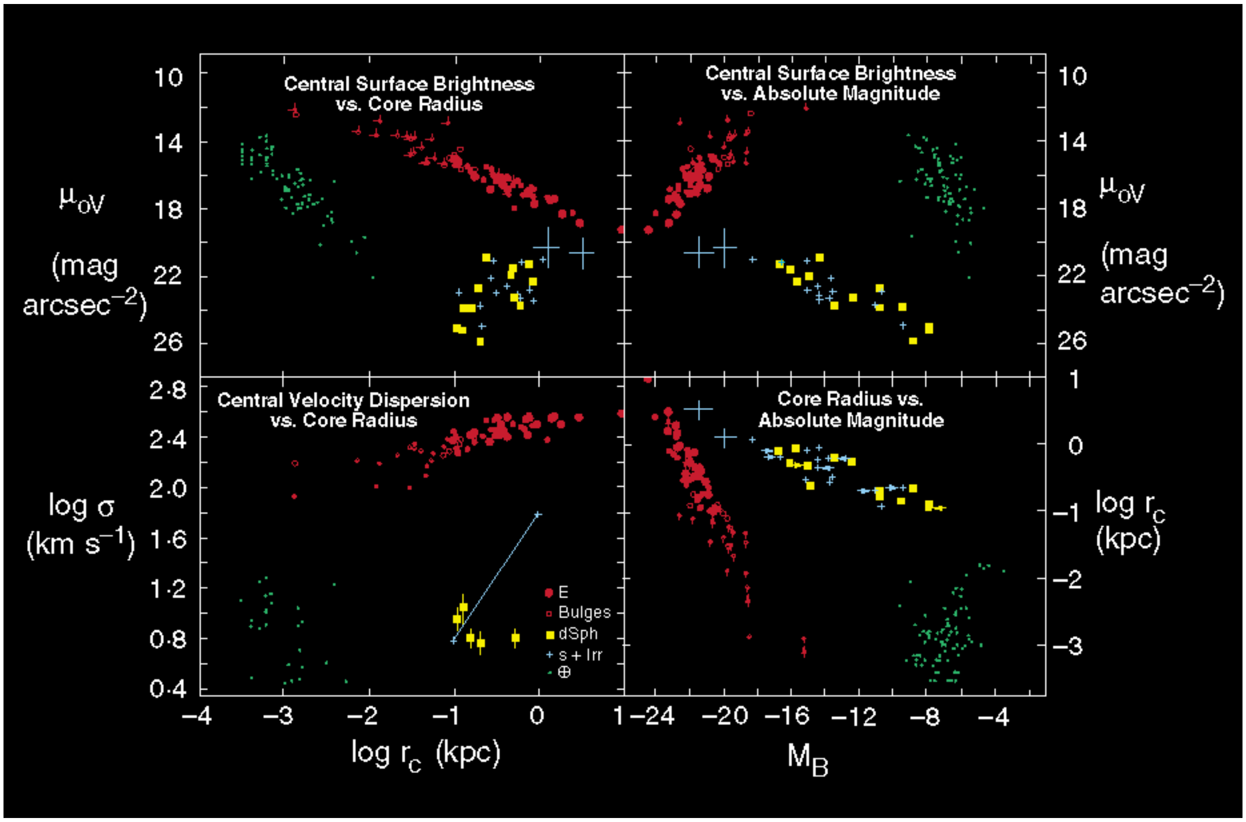
KORMENDY: Great! I would characterize that book in two ways. Kuhn suggests that science doesn't progress toward more accurate truth. Rather, it just progresses from one paradigm that's believed at one moment to a different paradigm that's believed at a later moment. I don't buy that, and neither do most scientists. That's why Kuhn's image in the scientific community is less than wonderful. But his description of the sociology of how discovery happens is absolutely right on. And my work on "dwarf ellipticals" is a mini version of that story.

I had been observing with the CFHT to measure surface brightnesses of elliptical galaxies at all luminosities. I was able to do this well, because I had the wonderful image quality that you get with the CFHT on Mauna Kea. Sometimes images were blurred by Earth's atmosphere by only one-third of an arcsecond. I could measure smaller ellipticals in ways that nobody could manage before. I had collected a lot of data, and I had partly analyzed it. Then in 1984, I attended a workshop at the Weizmann Institute in Israel, in which about 15 or 20 astronomers were invited. It was a workshop like none that I've seen before or since. It was wonderful. Each person was asked to give a one hour talk that started at 9 AM that ended … whenever it ended. Most talks lasted all morning. You got to see how people reacted to everything you said. If they didn't react well, you could address that. If something was unconvincing, you had a chance to convince them. Or you got marching orders. That was great. And then, the afternoons were free, and in the afternoons, we did research. Well, some people talked. But I did research. So I started to plot the scaling laws for the central – the "core" parts – of ellipticals, using my CFHT data that had been reduced but not yet analyzed. Every day, I would add a few points to a diagram that correlates central surface brightness with galaxy luminosity. And I could see the expected correlation that lower-luminosity ellipticals are denser and smaller. But the dwarf ellipticals did not plot near that correlation. There was no "break" in the relationship – no luminosity above which smaller ellipticals get denser and below which smaller ellipticals get less dense. No, ellipticals always get denser as they get smaller, with the well known, tiny elliptical Messier 32 as part of the correlation. Messier 32 is a tiny companion of the much bigger Andromeda Galaxy, Messier 31. People had thought that its high density was a fluke caused by tidal effects. They thought that M31 stripped off the outer, low-density parts of M32, leaving behind a high-density weirdo. But I found that other little ellipticals and small bulges behave the same way. They included M32 analogs that had been catalogued by Sandage and Tammann in the Virgo cluster of galaxies. In contrast, all the objects that they called "dwarf ellipticals" defined a different correlation in which smaller dwarfs are less dense. And the two correlations were completely disjoint – they did not join up at some magic transition luminosity.

It's may be hard to picture what I'm saying, so I'd like to illustrate it with two pictures. The first is a picture of the Andromeda galaxy, Messier 31. It happens to have two companion galaxies that are almost equally bright and massive. But one is an elliptical and the other is a spheroidal. So you can form a mental picture of how they are similar and how they differ. The next picture will show the correlations that I just discussed.

Messier 31, the Andromeda galaxy, fills the field of view. It is the first galaxy in which I found a supermassive black hole. It is also relevant here, because it happens to have two companion galaxies, and they nicely illustrate the difference between elliptical and spheroidal galaxies. Both galaxies are dwarfs compared to Messier 31 and other big galaxies, including our own. What helps is that they have the same total brightness and stellar mass. So you can form a mental picture of why ellipticals and spheroidals are distinguishable, even though both are made of old stars, and they both have elliptical isophotes with no spiral arms. This is the Astronomical Picture of the Day from December 17, 2018, copyright Robert Gendler.

Messier 32 is seen in projection at the edge of the visible disk of Messier 31, mostly to the left and slightly up from the center of Messier 31. It is very compact – it is small, and it has a high surface brightness, higher than this photograph can show. It is a true elliptical, the smallest one that I had measurements for in the 1980s. In contrast, NGC 205, below and to the right of the center of Messier 31, is much bigger but correspondingly lower in surface brightness, so its total luminosity is the same as than of Messier 32. NGC 205 is the brightest spheroidal in the Local Group. The brightest spheroidals in the Virgo cluster are about 5 times brighter. And so I call Messier 32 a (true) dwarf elliptical and NGC 205 giant spheroidal. We know no elliptical that is significantly less massive than Messier 32. It may be a small classical bulge of a disk galaxy that got its disk stripped off by gravitational tides from Messier 31.



These are cosmetically prettier versions of the correlations that I derived at the Weizmann Institute workshop. Here M_B measures total brightness of the galaxy and μ_V measures the surface brightness in a small area of nearly constant surface brightness near the center. Smaller numbers correspond to higher brightnesses; a difference of 5 magnitudes is a difference of a factor of 100. Also, r_c measures the radius of the "core" – the region of nearly constant surface brightness near the center. And σ is the typical speed of motion of stars in the galaxy. The red points for bulges and ellipticals define a tight sequence – *fainter* (which means less massive) galaxies have *smaller* cores of *higher* surface brightness. The yellow points show that galaxies that were then called "dwarf ellipticals" and that I now call "spheroidals". The slope of their correlation is opposite to that for ellipticals: *fainter* spheroidals are also *smaller*, but they are *lower* in surface brightness. The two correlations overlap a little in galaxy total brightness; where they overlap, spheroidals are lower in central surface brightness than ellipticals by10 magnitudes per square arcsecond. That's a factor of 10,000! So it is no wonder that Allan Sandage could distinguish ellipticals and dwarf ellipticals of the same total luminosity just by looking at photographic plates. The other surprise that I found was that the blue points for spiral and irregular galaxies agree exactly with the yellow points for spheroidals. My conclusion was that spheroidal galaxies are not dwarf versions of ellipticals. No: they are related to spiral and irregular galaxies. Finally, the green points show globular clusters of stars. They are unrelated to any kind of galaxy.

Getting back to the sequence of events:

Gradually, over the course of first week of the workshop, my picture of ellipticals fell apart in the way that Kuhn describes. That picture – that paradigm – of how ellipticals act was wrong, because my new and higher-resolution observations clashed with it. Then all the stuff that Kuhn describes as happening to a scientist at a moment of revolution happened to me. You just lost your picture of how things behave. You don't know what to do next. So your first reaction is, "My god, have I screwed up?" So you check everything, and you discover, if things go well, (and they did) that you didn't screw up. So the prevailing paradigm was wrong. But now, you don't have any guidance from that picture any more. You don't know what to do. So you start to mess around. You try everything you can think of. I started to plot in my correlation diagrams all the other objects that I knew about. I plotted globular clusters, and sure enough, they are different from both ellipticals and dwarf ellipticals. That was no surprise. And then I plotted disk galaxies. And they overlayed the dwarf ellipticals exactly. Really exactly.

ZIERLER: Which tells you what?

KORMENDY: It tells you that there's a close relationship between dwarf elliptical companions of our Milky Way plus bigger "dwarf ellipticals" in the Virgo cluster and – surprise! – disk galaxies that are completely different from elliptical galaxies. So the true ellipticals are absolutely monotonic in their properties all the way from Messier 87 [the biggest galaxy in the Virgo cluster] to Messier 32. Smaller ellipticals have higher central desities. In contrast, both galaxy disks and "dwarf ellipticals" have the same scaling relations, in which smaller galaxies have lower densities. It helped that good CFHT spatial resolution let me fill in what used to be a gap between giant ellipticals and M32. You can see why I have worked on scaling laws for all of my life. The correlation that I just talked about for the central surface brightnesses of true ellipticals parallels a correlation between effective brightness and effective radius that I found in my thesis. ["Effective" parameters are measured at the radius that encloses half of the light of a galaxy.] That's called the Kormendy relation by a lot of people, for which I am grateful. Both correlations are valid only for true ellipticals. Disks, irregulars, and "dwarf ellipticals" behave the same way. That way is opposite to the correlation for ellipticals. Smaller dwarfs are less dense. So I realized that dwarf ellipticals are not ellipticals at all. Instead, they must be related to spirals. So we should not call them "dwarf ellipticals". Luckily, an alternative name was already in the literature. Many people had called extreme dwarf ellipticals like Draco and Ursa Minor "dwarf spheroidals". So I suggested that we call all of these superficially elliptical-like galaxies "spheroidals". Draco and Ursa Minor are "dwarf spheroidals". Bigger analogs in the Virgo cluster are just "spheroidals" or even "giant spheroidals". There are no spheroidals brighter than a certain luminosity. [It corresponds to a V-band absolute magnitude of about -18, for listeners who are specialists.] Jumping for a moment to the present time, I realize now that the reason why spheroidals vanish brighter than the above limit is that they start to contain bulge components. And then, we give them a different name. We call them S0 galaxies. That's a morphological type that goes back to Hubble and Sandage. I suggest now that spheroidal galaxies are nothing more and nothing less than S0 galaxies that contain no bulge component.

But my thinking had not progressed this far yet during the Weizman Institute workshop. Let's get back to that story.

Back then, over the course of a few days, a new picture fell into place in my head. Because I started to realize other things. One is that spheroidal galaxies have heterogeneous star formation histories. Some, like Draco and Ursa Minor, are made exclusively of old stars in the same way that Messier 87 is made of old stars. But a lot of them, like Sculptor, were still making stars just a few hundred million years ago. What kind of galaxy has the structural parameters of a disc and makes stars? Well, it's an irregular galaxy. It's a dwarf irregular. And the dwarf irregulars were already in my scaling relations, and they overplotted the dwarf spheroidals exactly.

I realized that irregulars had been turning into spheroidals episodically over the whole age of the Universe. Some did it long ago. Some haven't done it yet. The new picture fell into place during the second week of the meeting. In the third week, I gave my talk and showed the new results. That was great. I had gone through a mini version of a Kuhnian scientific revolution in which the old picture fell apart; I was confused and upset for a while; a new picture came into place, and the new picture made sense and has grown stronger ever since. By now, we have measured hundreds of galaxies in both the elliptical sequence and the sequence of spheroidals plus disks plus irregulars. Conclusions are robust.

I felt lucky at the workshop that, by the time my talk was scheduled, I had something new to say. In the audience were Joe Silk and Avishai Dekel, and about a year later, they wrote the famous Dekel and Silk paper in which they discussed how irregulars turn into spheroidals when supernova explosions eject gas from tiny galaxies that don't have enough gravity to hold onto their gas. So we soon had an explanation from theorists who were in the audience of my observational talk. Their explanation still looks like part of the truth, although we now realize that there are other ways to lose gas, too. The main one in the Virgo cluster is ram-pressure stripping of cold gas out of spiral and irregular galaxies by the million-degree hot gas that fills the cluster. Still, the story started at the Weizmann Institute workshop, and it turned out well.

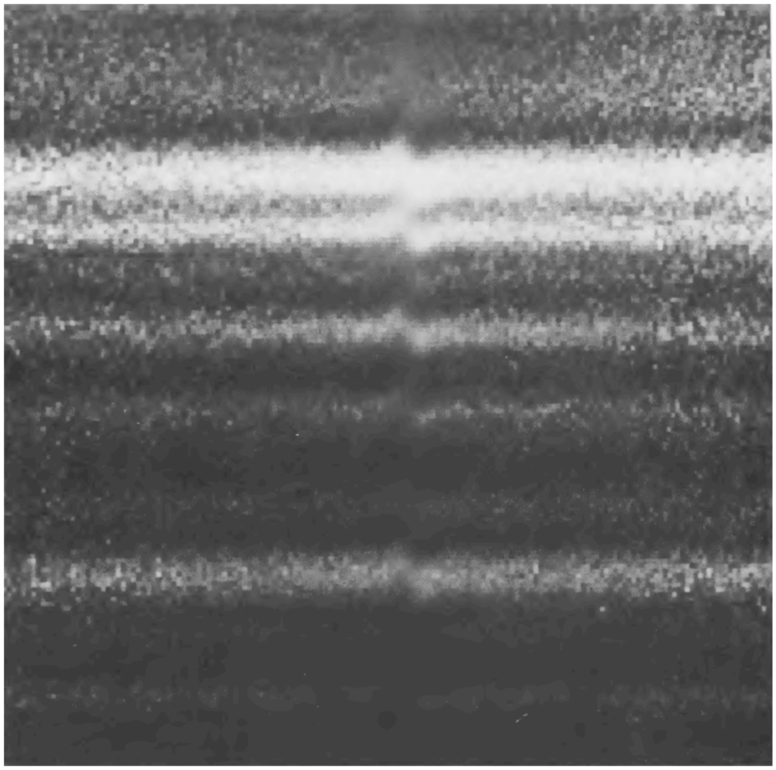
ZIERLER: And more broadly, John, where were the advances in photometry in all of this?

KORMENDY: Well, this work really was enabled by charge-coupled device [CCD] detectors. The development of CCDs revolutionized the kinds of observations that I make. They were linear enough so that the tweaks that you have to make to turn electronic signals into light measurements are easy. And the computer power and the software systems with which you can manipulate images gradually evolved into the Image Reduction and Analysis Facility, IRAF, at Kitt Peak. And other descendents of the IPPS that I now use. The combination of almost-linear and sensitive detectors – much better than photographic plates – and interactive analysis software completely revolutionized how we do research.

ZIERLER: Getting back to black holes, what was it like – the first night when you found evidence for a black hole?

KORMENDY: As I've said, I started to look for black holes as soon as a spectrograph became available on the CFHT. My first try with CASSHAWEC became useful for my third black hole discovery, in NGC 3115. But it was not conclusive by itself. So my first stellar-dynamical discovery of a supermassive black happened for the Andromeda Galaxy, Messier 31. I remember that night very well. I used the Herzberg Spectrograph, which was designed before we knew that the image quality on Mauna Kea is better than 1 arcsec. So the spectrograph had a scale of 0.9 arcseconds per detector pixel. That's not good enough. Luckily, I was able to get the DAO to build an optical "f/4 module" that converted the scale to 0.44 arcsec per pixel. It was installed for my first night of the black hole program. The result was a complicated instrument. So it took some work by my setup engineer from the CFHT Corporation to get everything to work – to focus the spectrograph and to focus on the sky. Also, CFHT politics were more than a little Byzantine, and the CFHT Director had forbidden the staff to work on non-facility gizmos, specifically my f/4 module. Happily, my setup engineer was Derrick Salmon, who was a friend and a fellow Canadian. Derrick set me up with the f/4 module. I owe Rick big-time – his breaking of the CFHT rules made my black hole discoveries possible. The setup was not entirely easy. For the first half of the night, we were not sure that everything would work. But by midnight, everything was focused. The seeing was excellent. I started to get spectra of M31. And as soon as the first spectrum displayed on the TV, I knew I probably had a black hole.

I should explain how the search is done. Any black hole in M31 is dark – it doesn't emit light. But we expected from work on quasars that the dead relics of their black hole engines would have masses of hundreds of millions to billions of Solar masses. So we could estimate how far from the center the gravity from such a black hole would affect stellar motions. And the answer was … marginal. That always was the reason why the search would be difficult. In M31, you could expect that stars within one or two arcseconds of the center would have to move faster to orbit a black hole than they would if all the gravity came only from stars. So that's what I looked for – faster rotation of the galaxy or faster random motions of the stars near the center than you would get without a black hole. You measure how fast the stars move by looking for doppler shifts in spectral absorption lines. Or you find that the lines get very broad near the center, because random motions are big. Or both. That's why you need spectra. And the fact that black holes affect stellar motions only within an arcsecond or two of the center is the reason why you need good image quality.



So this shows the black hole discovery spectrum of the center of M31. Bluer colors are at the top, redder colors are at the bottom, and the horizontal dimension is along the major axis of M31. Pixels are 1.2 Å vertically by 0.44 arcseconds horizontally. The surface brightness gradient in the galaxy has been divided out; otherwise, the center of the galaxy in the middle column would be so much brighter than the bulge component farther out that I could not show both with the limited dynamic range of a picture. This picture is a negative, so absorption lines look bright. The zigzag in the spectral lines around the center is a signature that the stars near the center move at hundreds of km/s toward us on one side and away from us on the other side, both with respect to the galaxy's total motion. So the galaxy spins very rapidly near the center – much more rapidly than it would if its gravity came only from stars. The implication turned out to be that M31 contains a central black hole with a mass of a few tens to a hundred million Suns. Further improvements, first with the CFHT and later with Hubble, confirmed the detection and refined the black hole mass to a hundred million Suns. This interview focuses on my research, but I want to emphasize that independent measurements that led to the same conclusion were made and published at essentially the same time by Alan Dressler and Douglas Richstone.

There's one more good thing about the discovery night. We had some idea that black holes might be there and might be detectable, from work on quasars. So I had some inkling that the night might be important for my career. I wanted to share it with my wife, Mary. CFHT rules are that spouses are not allowed at the telescope. Well … I broke the rules and smuggled Mary up. I thought I would get away with it, because Rick and the telescope operator were good friends. So Mary was there to share the experience. And thanks to her, I have pictures of that night.

Here I am guiding the CFHT to stay centered as precisely as possible on M31 while I took the spectra that were used to find the supermassive black hole. Picture by Mary Kormendy.

After that first run, I had many more. Black hole detections in M31 and the Sombrero Galaxy, NGC 4594, were published in 1988. The detection in NGC 3115 was published in 1993, this time in collaboration with Doug Richstone, whose dynamical models added a lot to the strength of the results. I found two more black holes with the CFHT, in the elliptical galaxies NGC 3377 and NGC 4486B. 4486B is a tiny companion to the giant elliptical Messier 87. By that time, the black hole search had largely moved to the Hubble Space Telescope. Still, one thing that contributed to people's belief in the results was that, while black holes were discovered from the ground essentially as early as improving technology allowed, all of the CFHT detections were confirmed with Hubble at several times better resolution. Even the resolution of Hubble measurements improved over the course of its mission. So the first black holes have now been through several generations of improved measurements. Conclusions get more believable when better data make them more robust.

ZIERLER: When did you start thinking about dark matter in galaxies?

KORMENDY: I started to think about dark matter only gradually. It is a revolution that I witnessed almost – well: not quite – from "day one". The idea originated much earlier, but I lived through the time when it caught on to become what was, by the 1900s, the most important revolution in progress in astronomy.

But the first evidence came much earlier. In 1933, Fritz Zwicky told the *bastards* … that some form of dark matter may make up most of the Universe. But during the following three decades, nobody believed in dark matter. I remember the events that changed our opinions. And as the evidence became strong enough, Sandy Faber and Jay Gallagher wrote a famous paper in the *Annual Review of Astronomy and Astrophysics* which acknowledged the discovery of dark matter as a revolution in progress. I lived through that transition, and I was aware of its importance. But in the 1970s and early 80s, I was working on other subjects. I started to work on dark matter in 1986, when I was invited to give a series of review lectures at a summer school in China. I wanted to review all aspects of galaxies. But by that time, dark matter was an aspect that you could not ignore. So I made it the centerpiece of my lectures.

I spent a couple of months doing research on rotation curves to look for scaling laws between dark matter parameters. Not between dark matter parameters and visible matter parameters. Just between things that you can measure about the dark matter halos of galaxies. I found correlations between halo size, halo density, the speeds at which particles move in the halo, and the brightness or mass of the visible part of the galaxy. So I wrote my lectures for the Chinese summer school, and they were published in the proceedings of that school. That's how my work on dark matter got started. The book was published in 1988, but it is not well circulated, and most people have never heard of it. A few years later, I was an invited by Ken Freeman to visit Mount Stromlo Observatory in Canberra, Australia. Happily, I also had a sabbatical from Hawai'i. So Mary and I spent a few really wonderful months in Canberra, and Ken and I started to turn my first measurement of dark matter scaling laws into something better. And that work took only … let me see … 20 years. (laughs) But we turned it into a thorough paper with many new results. That paper was published in 2016. It's a paper that I like a lot.

ZIERLER: Just to foreshadow all the way to today, when dark matter remains mysterious. At that time, was there any sense that this would be a 40-year+ nut to crack?

KORMENDY: No, during the first years when the dark matter revolution was under way – was acknowledged to be happening – we didn't know how or when it would end. It's also a Kuhnian scientific revolution. When your old picture falls apart, you don't have a paradigm. At that stage, it's hard to predict how long it will take to make the required breakthrough. I remember a talk that was given by George Lake that was important to my personal understanding of the problem. George was a theorist who, in his later years, worked in Zurich. Unhappily, he has passed away. Very, very smart guy. He gave a talk in which he pointed out that there are three candidates for what the dark matter can be. One is "more stuff". That is, it could be more of something that we already know about. Examples are rocks and planets and failed – that is: undermassive – stars. The second possibility is "different stuff" – some kind of matter that we don't know about yet. That maybe we could discover. For example, neutrinos. We already knew that neutrinos exist, but we didn't yet know that they have mass. And the third possibility is "new physics" – new physical laws. When he gave this talk in 1992, we didn't know which of these would be right. That was the point of his talk. We didn’t know whether we were looking for more stuff, different stuff, or new physics. Now, in 2021, we know that all three are right. Because there is some more stuff – neutrinos do have mass, although their mass is small enough so that neutrinos contribute only a little to the dark matter. We also are pretty sure that there is some kind of different stuff that we have not yet discovered. There are good reasons to think that it is a fundamental particle, even though many experiments to look for it haven't succeeded. Still, we think that the different stuff is an unknown particle that has mass, that moves slowly compared to the speed of light, and that interacts with other matter very weakly. It’s called “cold dark matter”. And we are now confident that there is also some new physics, because observations tell us that there exists an unknown form of dark energy that actually contributes more mass-energy density than do either the “more stuff” or the “different stuff”. We don't know what the new physics is. But we think we know it is new physics. So we've made progress. We know that all three possibilities categorized by George are right. We just don't know what the constituents are.

But we still have far to go to emerge from the dark matter scientific revolution. At the moment, I could not “stick my neck out” and guess the timescale on which we make progress. Within a decade, we should know the behavior of dark energy as a function of cosmic time well enough to kill a few extreme theories. But the “new stuff” particle detection experiments – I am not an expert on those. I have the feeling that they could succeed tomorrow, or they could succeed in 50 years, or they could not succeed at all.

The only strong feeling that I have is this: I am not willing to indulge in the arrogance to think that there is only one kind of dark matter that we don’t yet have the technology to discover. This is a situation in which Occam's famous razor is a fast way to cut your throat. There could easily be more than one kind of dark matter. And part of why I have this underlying feeling that we're thinking too simply is that fundamental physics itself is evolving in a direction that should surprise us. It is becoming biologically complex. It is becoming complex in a way that does not invite me use Occam's razor. There is even a danger that some observed results – either in astronomy or in fundamental physics – are emergent behavior in complex systems and not consequences of simple physical laws that we try to discover. Maybe we are only scratching the surface of something that is fundamentally more complicated than we like to think. Dark matter is a small piece of that complexity. It could turn out that there are a dozen different dark matter particles. Maybe most of them don't contribute a significant amount of density … but neither do neutrinos, and nobody doubts that they are important. So I can’t guess how the ultimate physical picture may look. But we may have to give up our cherished notions of simplicity. And whether all this will happen in my lifetime – or in our children’s lifetimes – I am not willing to guess.

ZIERLER: What were the circumstances of your decision to move to Hawai'i? Was part of the reason that you wanted to be a professor and to interact with students?

KORMENDY: It was a combination of several things. I felt a bit isolated scientifically at the DAO, and the community of astronomers in Hawai'i was more in line with what I work on. I wanted to be in that culture. Mary and I wanted the physical beauty of Hawai'i and the joy of living in a place where you can swim in the ocean on the days between Christmas and New Year. (laughs) We'd grown to like scuba diving. A big attraction was the University of Hawai'i's institutional access to every telescope on Mauna Kea. Which meant that I could broaden the kinds of things that I study. I'd get less CFHT time, but I'd get more time on other telescopes. I'd get colleagues that were close to my research areas. And we'd get to live in a place that's beautiful. The down side was that it was expensive to live there. So we knew we were giving up some aspects of the quality of life, but we would gain others. The decision was not easy. One other factor was that changes in Canada, especially in the Herzberg Institute of Astrophysics, the DAO's parent institute, suggested that the DAO would become more of a service organization. My preferred concentration on research would get harder. So having to teach in Hawaii was not an unhappy prospect – teaching is important, and it would keep me and my students closer to active research than new support duties would have done at the DAO. So we moved at what turned out to be the right time, both personally and from an astronomy point of view.

ZIERLER: Observationally, what could you accomplish at Hawai'i that was not possible anywhere else?

KORMENDY: The big gain was in the research environment – I had a lot more interactions with people whose interests are similar to mine. From a telescope access point of view, the move affected my career less than I expected. Because I could still get time on the CFHT. "CFHT" contains "H" (laughs) as well as "C". I had access to the telescopes that Hawai'i owned, but I rarely used them. That wasn't clear when we moved, but that's how it turned out.

ZIERLER: Now, at Hawai'i, your affiliation was with the Institute for Astronomy. Is that different from the Department of Astronomy?

KORMENDY: To the extent that Hawai'i had a Department of Astronomy, it was housed at the Institute for Astronomy. We taught astronomy to the University's undergrad and grad students. But the IFA is a research organization that's pretty independent of the University of Hawai'i. It has more autonomy than an astronomy department. If, at the University of Texas, you combined McDonald Observatory with the Department of Astronomy, left the result within the university, but gave authority to the Director of McDonald, not to a Department Chair, then you'd have what the IFA was. The IfA had a Director. It didn't have a Department Chair. So the authority and the budget and the Mauna Kea responsibilities were more separate from the University than our UT Department of Astronomy is from the University of Texas. And that was a good thing.

ZIERLER: Did you enjoy the transition to the professor's life? Did you enjoy teaching? Did you take on graduate students?

KORMENDY: It was mixed. I enjoyed teaching in some ways. The process of trying to make ideas understandable to people who are afraid that they can't understand them – that's enjoyable. I like it a lot and I do it a lot. I like to give public talks for the same reason. And of course, the nice thing about teaching the undergraduate intro course is: You get to tell them the good stuff. The good stuff might not even be part of your research. When you think about the history of all of astronomy, what's the good stuff? That's what you tell them. And you try to make it understandable – to demystify it. You try to get them enthused about it. That's fun. I enjoyed it. The infrastructure work that goes into preparing lectures is more a mixed bag. Some of it is OK; some is just work. This is much easier now. But those were the days before PowerPoint. So preparing lectures in those days involved overhead transparencies. They give me a stomach ache now. (both laugh) And slides – 35 mm slides. Remember 35 mm slides?

ZIERLER: I don't, no, that's before my time.

KORMENDY: I, unfortunately, do. (both laugh) It was work, and it got in the way of research. I like research better than teaching, I admit that. But it had rewards. I had a few grad students, but not many. And I have mixed feelings about that – more regret than not. Even including my time in Texas, I never had a grad student who was really a protégé – who has gone on to be a leader in the fields in which I work. If you look at the best astronomers, people like Sandy Faber and Ken Freeman, they have many such people. Most astronomers have one or two at least. I would have liked to have such a student. But there was never an opportunity until it was too late. I had an opportunity just before I retired, a woman who I thought was capable of being as successful as Sandy Faber. But I was too close to retirement, and it wasn't sensible for me to tie her future to the uncertainty of what I would be like in retirement. So we talked, and I ended up "biting the bullet". I told her that I didn't think it was right for her. But I'm sorry that it wasn't. If she'd come two years earlier, I'd have a descendant. As it happens, I don't.

ZIERLER: What was the overall impact of Hubble on your research agenda?

KORMENDY: That's an interesting question. I was doing with the CFHT exactly the sort of things that Hubble was designed to do in my area of research. Hubble was designed to give good spatial resolution to look at galaxy centers. Hubble was designed to look for black holes. That's what I worked on. So Hubble was both a threat and an opportunity. In the early days, before Hubble was fixed, the undercurrent was to "pick the low-hanging fruit" that Hubble would eventually do better. To get as many results as I could get from the ground. That worked. As I've said, I ended up discovering three of the first four supermassive black holes that were found by dynamical techniques, and four of the first eight. Later, the subject "took off" with Hubble.

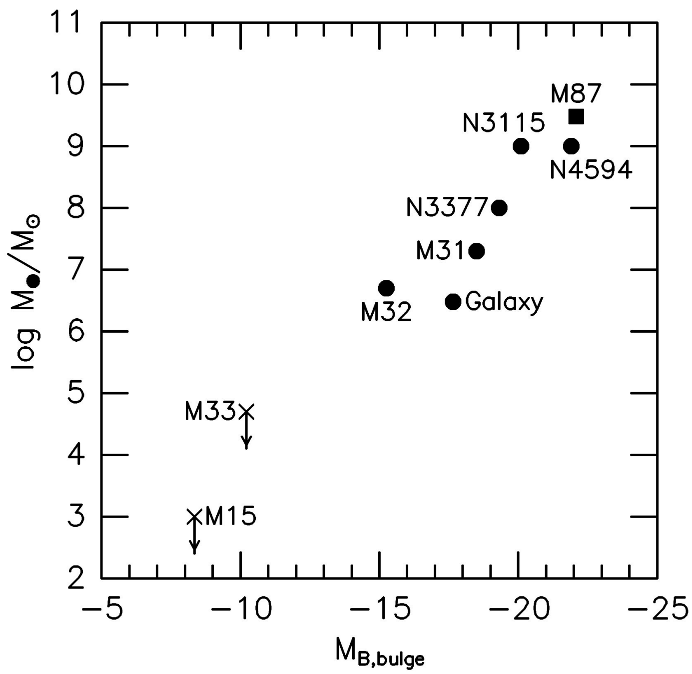
Also, the most important result that I could get from the scaling law for elliptical galaxies was this distinction between spheroidal galaxies and ellipticals. I found that from the ground. It was demonstrated better with Hubble. But it was already established.

So when Hubble came along, I had to change my lifestyle. Because it was clear that, to use Hubble, you needed to pool the expertise and resources of lots of people so you could get lots of telescope time. The Space Telescope Science Institute and the culture surrounding the telescope emphasized this point to the community. And the community understood. So the Nuker team was formed, and as a result, the work that I had been doing on my own or with one or two collaborators became part of the work that the Nuker team did. I don't want to make it sound like I brought that work to the Nuker team. The team's people came together from different directions, each bringing a piece of the subject with them, and we melded it all into a bigger team that had more clout over both telescope time and science results. And then we used Hubble to do the kinds of things that we all had been working on separately, in my case with the CFHT. Of course, Hubble results were more powerful than ground-based observations. And Hubble had an enormously powerful public relations machine. So results were broadcast to the public and believed by the public – including by astronomers who were not in our special area – in a way that ground-based astronomy could never match.

ZIERLER: More broadly, for yourself and for the field in general, what were some of the advances in galaxy formation and the stellar dynamics at the center of our Galaxy during the 1990s?

KORMENDY: Well, the 1990s were the times when enough black holes were found via dynamical searches, first from ground and then with Hubble, to discover the correlations – the scaling laws – between black holes and host galaxies. That work reached its culmination in 2000 with the publication of the correlation between black hole mass and the stellar velocity dispersion of the host galaxy. That correlation had a small enough scatter so that it captured everyone's attention. It created a "phase change" in the thinking about galaxies and black holes. Before that, we realized that galaxies can have central black holes, and they may have something to do with each other, but we didn't have a compelling story about what they do. After the change, people now think that black holes and galaxies grow together in lock step, with black hole activity – for example, as quasars – controlling galaxy evolution. And with galaxy evolution feeding black hole growth. This idea has taken over so much that I think it is now overdone. It has turned into a bandwagon in which belief is stronger than reality. That's the consequence of the discovery of correlations between black holes with host galaxies. We Nukers wrote some of those papers, but we weren't the only people who found such results. The first black hole scaling laws were found in the 1990s. Alan Dressler first suggested that bigger black holes live in bigger galaxies, based on 3 detections. I published the first illustraton of a correlation based on seven galaxies, four of which had black hole detections from my work with the CFHT. This was in

1993 at a conference in Madrid, Spain. The proceedings are not well circulated, so I include the figure here. I showed a version with 8 galaxies in a 1995 *Annual Review of Astronomy and Astrophysics* review with Doug Richstone that is well known. And now we have almost 100 black hole detections. The correlation has gotten better and better, but it hasn't changed. That correlation was between black hole mass and bulge luminosity. Now, since 2000, we also have a correlation between black hole mass and the random velocities of stars in the host galaxy. That correlation more than anything launched the belief that galaxies and black holes evolve together.



This was the first illustration of the correlation between black hole mass and the blue (B-band) absolute magnitude of the host galaxy's bulge component. For the elliptical galaxies M32, NGC 3377, and M87, the bulge luminosity is the total luminosity – there is no disk. For NGC 3115 and NGC 4594 (the Sombrero galaxy), the bulge luminosity is more than 90 % of the total – the disk is a minor component. We had only limits on black hole detections for M33 and the globular cluster M15. From Kormendy 1993, in *The Nearest Active Galaxies*, ed. Beckman, Colina, and Netzer (Madrid: Consejo Superior de Investigaciones Científicas), 197

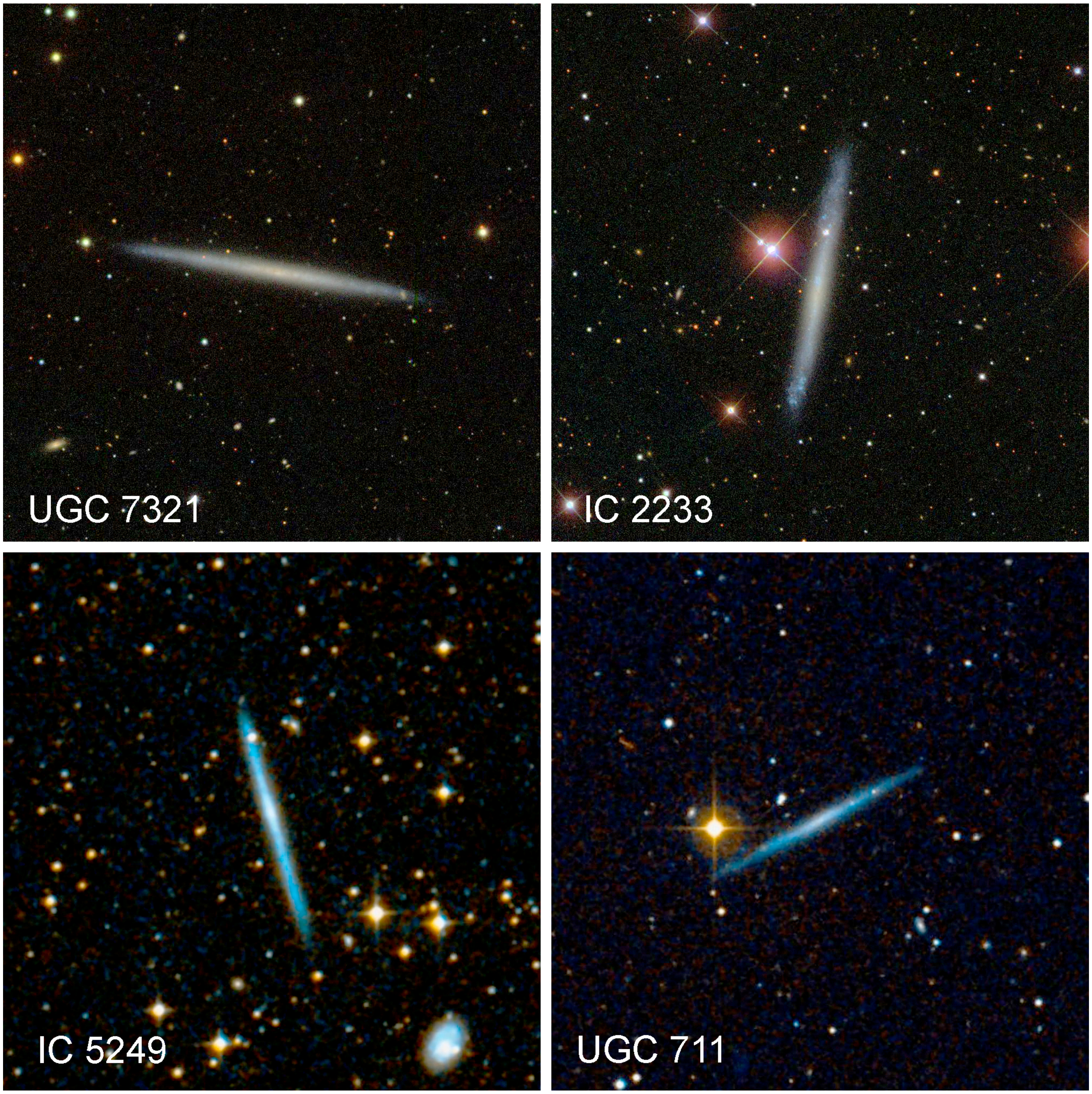
ZIERLER: What resonated as this research started to become more well known?

KORMENDY: The thing that resonated most was the discovery of the correlation between black holes and the stellar velocity dispersion of the host galaxy. It was thought – mistakenly, as it turned out – that the scatter in this correlation is smaller than the scatter of the correlation between black hole mass and bulge luminosity. We though that the scatter is consistent with the uncertainty in black hole mass measurements. So people thought that the intrinsic scatter could be zero. It is not zero, but it is small. And that work was done by two independent groups that published at the same time – two groups that don't get along with each other. I was in one of those groups. The fact that rival groups agreed convinced people who had no stake in either group. That happened in 2000. Theory made progress, too. Joe Silk pointed out that if only a tiny fraction of the energy that's emitted by a quasar somehow coupled to the gas that's making the galaxy, then that energy could control galaxy evolution. The fraction was so tiny that you don't have to be sophisticated in how you make the connection. That, too, convinced a lot of people. It's a correct comment. It’s a powerful comment. When compelling observational discoveries and powerful theoretical arguments appear at the same time and the community finds the results useful – and it certainly did! – then you can easily get an enormous bandwagon.

ZIERLER: Maybe it's a naive question, but just looking at a spiral galaxy, what else besides a black hole could account for its shape?

KORMENDY: That's a wonderful question that gets us into one of the big puzzles in today’s research on galaxies. First, let me say that, when you have in your mind a picture of a spiral galaxy – a very thin, very flat disk with gorgeous spiral structure outlined especially by bright young blue stars – almost none of that has anything to do with black holes. An example is Messier 101. It is a pure disk, with no “bulge” component that’s like an elliptical galaxy.

Messier 101 – a pure-disk galaxy as imaged by the Hubble Space Telescope



Messier 101 is face-on, so you can’t see how flat it is. But it has to be very flat to let it have the spiral structure that we observe. Also, measurements of the velocities of stars and gas perpendicular to the disks of face-on galaxies show that they are very flat. I’m going to make a big deal about how flat these galaxies are, so I want to show you some clearcut examples. This picture shows four of the flattest galaxies known – clearly, because we see them exactly edge-on. They are a bit smaller overall than Messier 101, but they make my point. It is hard to understand how so many galaxies can be so flat, with no central bulge component that’s like an elliptical galaxy, when they are repeatedly pummeled from all directions by “cannonballs” of dark matter. That’s a theme that I talk about later in this interview. Here, the point is that these galaxies have no bulge. This picture is from lectures that I gave at a Canary Islands summer school in 2011. All pictures are from http://www.wikisky.org.

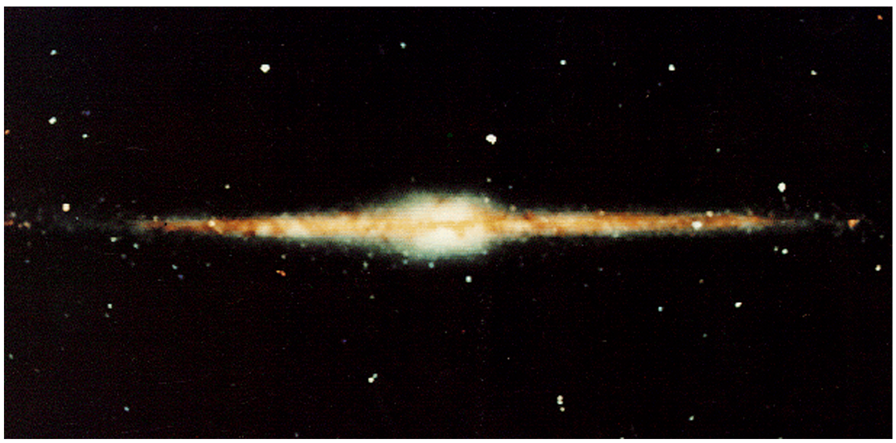
Together with three colleagues, I wrote a paper in 2010 that detected no central black hole in Messier 101. Our limit on the mass of a possible black hole is 2.6 million Suns. You can get much better limits for much closer galaxies. In 2000, Karl Gebhardt and the Nuker team showed that the (rather smaller) pure disk galaxy Messier 33 – a member of our Local Group of galaxies, along with Messier 31 and our Milky Way – cannot have a black hole that is more massive than 1500 Suns. So at least some pure-disk galaxies contain essentially no central black hole. Others do. But in our 2013 review of supermassive black holes, Luis Ho and I argue that black holes do not much affect the evolution of pure-disk galaxies such as Messier 33 and Messier 101. One reason why we conclude this is that we see no correlation between black hole masses – when we detect black holes – and any property of disks. So in answer to your question, we conclude that black holes affect galaxy evolution only when the galaxy is an elliptical. Or for galaxies like Messier 31, the black hole affected the evolution of only the bulge part of the galaxy. Not the disk. And not the dark matter halo.

ZIERLER: OK.

KORMENDY: What about our Milky Way? It has a black hole. In fact, our galaxy is by far the most convincing case of a black hole in any galaxy. Its mass is 4.6 million Suns. The reason why our black hole case is so convincing is that the center of our Galaxy is only about 27,000 light years away from us. So we can get spectacularly good spatial resolution. We need that because, although a mass of millions of Suns sounds big, our black hole is only about a hundredth of one percent of the mass of our galaxy. It influences the motions of stars only very close to the center. You need good spatial resolution to find it. There's a long history of looking. By now, two groups led by Reinhard Genzel in Munich, Germany, and Andrea Ghez at UCLA in the US have observed the center of our galaxy with telescopes that can correct images for the blurring produced by our Earth's atmosphere. Their measurements of the orbits of individual stars around a dark center of the Milky Way are nothing short of spectacular. They won the 2020 Nobel Prize in Physics for this work. So we are more sure about the black hole in our galaxy than we are about black holes in any other galaxies. But we – Luis and I, at least – believe that the black hole has had no major effect on the evolution of our galaxy. And guess what: Our Milky Way has no bulge component. Of all the things that I say in this interview, this one may surprise you the most.

ZIERLER: Yeah.

KORMENDY: Why do I claim that there is no bulge in our galaxy? We understand our galaxy best by looking at it in infrared light. Then dust in the disk near our Sun does not cover up what there is to see near the center. Then we see a central component of stars that is thicker than the disk – that bulges out above and below the disk plane. It looks boxy. And the important observation is that the box is thicker on one side of the center than it is on the other side. Both observations tell us that this component is not an elliptical-galaxy-like bulge – such a bulge would have an elliptical shape. Instead, the boxy central component is a galaxy bar that is seen almost end-on. Galaxy evolution simulations show convincingly that bars buckle vertically and heat up into vertically thick boxes. Then, when one end of the box is several thousand light years closer to us than the center and the other end is several thousand light years farther than the center, the closer end looks thicker than the farther end and the combination looks like a parallelogram, not a box. So, what looks like a bulge is really part of the disk. And observations closer to the center – including those by Ghez and Genzel – show that there is no smaller bulge, either. Our galaxy is as much a pure disk as is Messier 101. And that matters a lot.



This shows our Milky Way galaxy as observed in the infrared by the COBE satellite. Notice that the boxy part looks taller on the left side of the center than it does on the right side. This is a sign that it is a vertically thick bar seen almost end-on, with the left side closer to us than the right side. Bars are parts of disks. These observations and others show that our galaxy contains no bulge part.

So before we get back to the mystery of pure-disk galaxies, let's focus on black-hole—galaxy coevolution for a bit longer. Luis Ho and I suggest that coevolution applies only to elliptical galaxies and to those "classical" bulges of disk galaxies that look like ellipticals. Why? People believe in coevolution because of the tight correlations between black hole mass and the stellar mass and velocity dispersion of elliptical galaxies and the classical bulges of disk galaxies. But there is no correlation between black holes and disk properties. If we only saw disks and their black holes – and some disks manifestly contain black holes: witness our Milky Way – then there never would have been a tight correlation that caused us to invent the idea of coevolution. And pure disks outnumber ellipticals and bulges by a lot. For example, the Virgo cluster of galaxies contains many thousands of galaxies, but it only has about 30 ellipticals and maybe 100 bulges. The rest are disks. Or objects that evolve from disks when zooming galaxies have too many near-misses with other galaxies. Those relatively few bulges and ellipticals are the only objects for which the coevolution folklore is right. For all the rest – the pure disks – the coevolution folklore is barking up a non-existent tree. The community doesn't like that conclusion, and I don't think that we have convinced them. Even though Luis and I wrote a detailed, 143-page *Annual Reviews* article on the evidence. People still believe too much in coevolution.

ZIERLER: Why? Why do they cling to this folklore?

KORMENDY: There are several reasons. Scientists like Occam's razor. It's simple to have just one process. Also, the argument made by Joe Silk carries weight. It should. It's compelling. But that doesn't mean that it always happens. It's a plausibility argument that it *could* happen. And the third reason – maybe the biggest reason – has to do with numerical simulators of galaxy evolution. So, one of the big revolutions in the history of astronomy over the last few decades has been progress from a time when we had only observers and theorists to the present time when we also have a kind of theoretical experimentalist who makes numerical simulations of galaxy evolution. We now have three cultures, not two. And the simulators more and more have wonderful technical prowess as they try to simulate everything, not just gravity. They include the astrophysics of gas – collisions of gas clouds with each other, shocks, star formation, winds from young stars, winds from supernova explosions, winds and particle jets from AGNs: all sorts of stuff. The physics that goes into these models is mind-boggling. But the simulators find that they can't make galaxies that look real unless they include some sort of continual energy input. So they write a computer program subroutine that they call "AGN feedback" and that feeds a controllable amount of energy into the gas that's trying to form a galaxy and its stars. They need that feedback. But we don't know that it is AGN feedback. It's just energy. It is energy that they need, never mind where it comes from. People are motivated by arguments that involve black holes, and quasars and AGN jets certainly put out a lot of energy. So they call the feedback "AGN feedback". But in reality, they just adjust a "control knob" in their subroutine that injects energy, and they adjust it until the program makes galaxies that look right.

But there are a lot of adjustable control knobs. And success in adjusting them does not tell you where the energy comes from. For example, galaxies also have energy feedback from star formation. But – and this I can't explain – feedback from young stars doesn't satisfy. So they ascribe some feedback to AGNs. And they forget that there is no correlation between disk properties and black holes. I have a hard time believing that black-hole—bulge correlations imply that black hole feedback is important, but black-hole—disk noncorrelations also imply that black hole feedback is important.

I think that black-hole—galaxy coevolution happens during the formation of elliptical galaxies. And not even all ellipticals. Because if you make small ones via coevolution, you automatically preserve the correlations when you make giant ellipticals by merging smaller ellipticals. And the smaller, normal ellipticals form, I believe, in events that are like ultraluminous infrared galaxies or ULIRGs. These are collisions in progress of two galaxies, each of which contains a lot of gas and dust, and the two galaxies are now merging into one elliptical. Their important feature is that they are three-dimensional – they are not disks. Early on, these merger remnants are opaque and have enormous starbursts going on inside their gas and dust. The starbursts inject energy into an opaque medium that is fundamentally three-dimensional. If it were a disk, energy feedback might punch out perpendicular to the disk. Then you lose much of the effect. But in a ULIRG, all directions into which you feed energy always hit a "brick wall" of dust, because the dust shroud is three-dimensional. That's the environment in which Luis Ho and I think that the black hole correlations are created. Their remnants – classical bulges and ellipticals – are the only objects with which black holes show tight corrlations. So that's where I disagree with the current folklore.

ZIERLER: What about disks? You said that their formation involves a mystery.

KORMENDY: Right. That's why I showed pictures of very flat edge-on galaxies. OK, let's back up and consider the early stages of galaxy formation. What happens depends enormously on whether those initial times involve only gas or whether there are particles – we now call them "dark matter" – that pull on each other with gravity but that can't otherwise collide.

Before we knew about dark matter, we tried to understand how galaxies form from primordial gas. High-density regions attract gas toward them with their gravity. Under suitable conditions, gas clouds can collapse to form what we hope will be a galaxy. But different protogalaxies pull on each other a little, so they torque any elongation in them and make them rotate a little. Then the protogalaxy contracts, in part just because of gravity and in part because collisions between subclouds of gas slow them down and let them fall toward the gravitating center. As the cloud collapses, conservation of angular momentum makes it rotate more quickly. And as gas clouds collide, the collisions gradually wipe out random motions. Losing vertical motion while increasing rotation makes the gas cloud get flatter until you have a rotating disk of gas. When that disk has a galaxy-size mass and makes stars, you have a good prescription for forming a pure disk like Messier 101. (Alternatively, if the gas cloud has a stellar-size mass and contains dust, then you have a scenario in which a central star forms surrounded by forming planets. Our Solar System is like that – by and large, a spinning disk.) At this stage of investigation, in the early 1970s, we had a scenario that might succeed, once we worked out the engineering details.

Then came dark matter. And lots of evidence that the dark matter particles – whatever they are – don't interact except via gravity. In fact, we learned even more: the dark matter particles have to move at speeds which are much smaller than the speed of light. This combination is what we call "cold dark matter" – "cold" because the particles move slowly. And by the late 1970s, as I've said, we understood that dark matter outmasses the visible matter, then still gas, by a lot. The good thing about cold dark matter is that it is relatively easy to run numerical simulations to see how it evolves. This work was pioneered by people like Simon White, who is now at one of the Max Planck Institutes in Germany. Many generations of improvements have turned this into a spectacularly detailed and successful industry. What happens is well understood. Measured tiny fluctuations in the temperature of the cosmic background radiation tell us that there were tiny fluctuations in dark matter density early on. Then gravity caused dark matter particles to fall from places with lower density to places with higher density. Eventually, blobs of dark matter that were gravitationally bound collapsed and made dark galaxies. Primordial gas went along for the ride. Eventually, it made stars and created visible protogalaxies inside dark halos. OK, here's the "crunch point": After that, initially small protogalaxies – a LOT of them – grew by colliding and merging. In many generations of merging, this "hierarchical clustering" built todays galaxies.

Now you begin to see the difference between these pictures. The all-gas picture is inherently gentle. Making disks is easy. The many planetary systems that we have discovered show how easy it is. But hierarchical clustering is inherently violent. Over and over, galaxies collide. They come from random directions. So a major collision of two similar-sized galaxies scrambles up their disks and turns them into an elliptical. I've already said that this is how many astronomers think that ellipticals form. And if more gas rains onto an elliptical, or if it swallows a gas-rich galaxy with the right geometry, then you can grow a new disk around an old elliptical. That's how you get a galaxy like the Andromeda galaxy, Messier 31, which is about ¼ old bulge and about ¾ younger disk. OK, so far, I haven't said anything mysterious.

The trouble comes when you look at numbers. The cold dark matter picture produces an enormous amount of substructure. The formation of a big galaxy like our Milky Way involves a cluster of small protogalaxies that is not unlike a small version of today's clusters of galaxies. When you look at simulations, there is so much dynamical violence, and it happens so often, that it is hard to make *any* pure disks, let alone *a lot of* pure disks. And the mystery is that, in environments other than rich clusters of galaxies – in other words, in environments like those where our Galaxy and Messier 101 live – by far the majority of galaxies are pure disks. I pointed this problem out in a paper in 2010. Even in the Virgo cluster, there are too many disks. The biggest galaxies are ellipticals, and they are what we expect. But there are still many pure disks, and I already said that ellipticals are outnumbered by disks by a large factor. And even if you grow a new disk around an old elliptical – now called a bulge – you can't get rid of the bulge. In non-cluster environments like our Local Group, there are too few bulges for such a violent formation picture. For me, this is the biggest mystery with our picture of galaxy formation. I don't know the solution. It seems to me that we have to do something to our picture to reduce the amount of dynamical violence. One of the people who is most intrigued by this problem is Jim Peebles at Princeton. He works on possible solutions, such as changing the spectrum of density fluctuations in the early Universe. Another piece of the puzzle may turn out to be that dark matter – or maybe only some of the dark matter – is less cold than we think. If you make it warmer, then it makes fewer small dark galaxies and the amount of gravitational violence is reduced. Anyway, my job is to study visible galaxies, and that revealed the puzzle.

ZIERLER: Turning back to the 1990s, did Texas recruit you? Were you putting out feelers? Were you specifically looking to leave Hawai'i?

KORMENDY: Texas had the Vaughan Chair open for more than ten years before I was hired. It had at least two rounds of searches for a person to fill it. They didn't succeed, for reasons that I don't know. I think I was hired in the second or third round. And during that round, which got its start in about 1997 or 1998, I noticed and I applied. Texas didn’t recruit me. I applied to them.

There are two aspects to your question. One was the situation in Texas, and the other is why we might want to leave Hawai'i. Let me talk about the Hawai'i part first. Hawai'i is a wonderful place to visit as a tourist. It was more difficult to be a resident. The main reason was that it was enormously expensive. We arrived in 1990, when housing prices had a huge peak. We could buy only a small condo apartment, and it was a financial strain. Salaries were not bad, but they were not adequate for the early-1990s housing market. So Mary had to work, too, in order to make ends meet. She was a travel agent in downtown Honolulu. I was a professor at the University of Hawai'i. I worked in an intellectual island surrounded by a not-so-intellectual sea. But Mary had to work in the real world, and that wasn't easy. She loved the islands – loved sitting on the beach, watching the sun rise – but work was less pleasant. Still, the physical beauty of living in Hawai'i made it hard to leave. But by that time, there was another factor, which we haven't talked about. In 1996, Mary and I became dedicated birders. And the bird population in Hawai'i is both wonderful and terribly vulnerable. We always felt enormously privileged to see the few endemic forest birds. But they are going extinct as we degrade their habitats. We exhausted Hawai'ian birding. Meanwhile, Texas is one of the richest places in the US for birds and one of the best places from which to make trips to other places that are wonderful. And we could buy a house in the country, surrounded by nature and by birds. So our non-work-related interests were well served by moving. Plus, if we divided the cost of living by my salary, our financial situation improved a lot.

ZIERLER: Yeah.

KORMENDY: So now, we have a house out on 21 acres of Texas hill-country forest. We have birds all around us and armadillos in our back yard. It’s a delight. We miss the beach, but we like what we gained. So that was a part of our decision. And on the professional side, another factor was important to me. People in the Astronomy Department emphasized to me that they were overwhelmingly the same age. A time was coming when a large number of people would retire at almost the same time. So the Department necessarily was going to have a major rejuvenation. And I could participate in that. I felt like I had trained for exactly that job in Hawai'i, where I was active in recruiting. So that was a major attraction, too.

ZIERLER: So you were essentially a young senior scholar?

KORMENDY: Yes. I was the first hire in – I think – 12 years. And I came supercharged with enthusiasm and loyalty, because Texas bailed us out of a difficult practical situation. So we wanted to come here in part because of personal reasons and in part because I hoped to help the rejuvenation of the Department. And I helped it some. Still, the hiring process was colorful, and in the end, they made offers to several people. I was the one who said yes.

ZIERLER: In this new environment, what opportunities did you see to start new research?

KORMENDY: The opportunities were not mostly about research. They were mostly about leadership. Research had some pluses, but it also had negatives. I lost access to Mauna Kea, especially the CFHT and Keck telescopes. I hoped to make an instrument for the Hobby-Eberly Telescope, which collects a lot of light. It was for dark matter studies. But I am not an instrumentalist. I would have had to collaborate with McDonald instrumentalists, who are excellent but whose passions turned out not to be the instrument that I was thinking about. It could have been built, but it would have been monstrously complicated. I'm not sorry that I didn't go that route. And I wanted to write an *Annual Reviews* article, which meant that two or three years of my life were going to be spent on that project. It's one of the most successful papers I've written. I'm glad I wrote it. I have done projects with McDonald telescopes – ones that had good impact. But it is fair to say that the most important thing that coming to Texas did for my research was to provide first-class financial support and freedom from the infrastructure cost and uncertainty of applying for grants. I supported Mark Cornell, a very talented full-time research associate, for years. That enabled a paper that required a huge amount of work by both of us. But it is my highest-citation-rate paper other than my reviews. That paper and others took several years of full-time work each. "Full time" means time in addition to teaching, but with a mature undergrad course finished, this meant about 10 months of research per year. Without Chair support and without the research time that the Chair engineered, I could not have written those papers in Hawai'i. So I am happy with how I used the resources of the Chair.

ZIERLER: Now I'm curious about the term "secular evolution." How did that get started and what does it refer to?

KORMENDY: Yes, that's exactly the 2004 *Annual Reviews* article that I just mentioned. "Secular" a weird word for non-astronomers. If you google it, you get a definition that it refers to things that are not about religion. You don't get the definition that we use in astronomy.

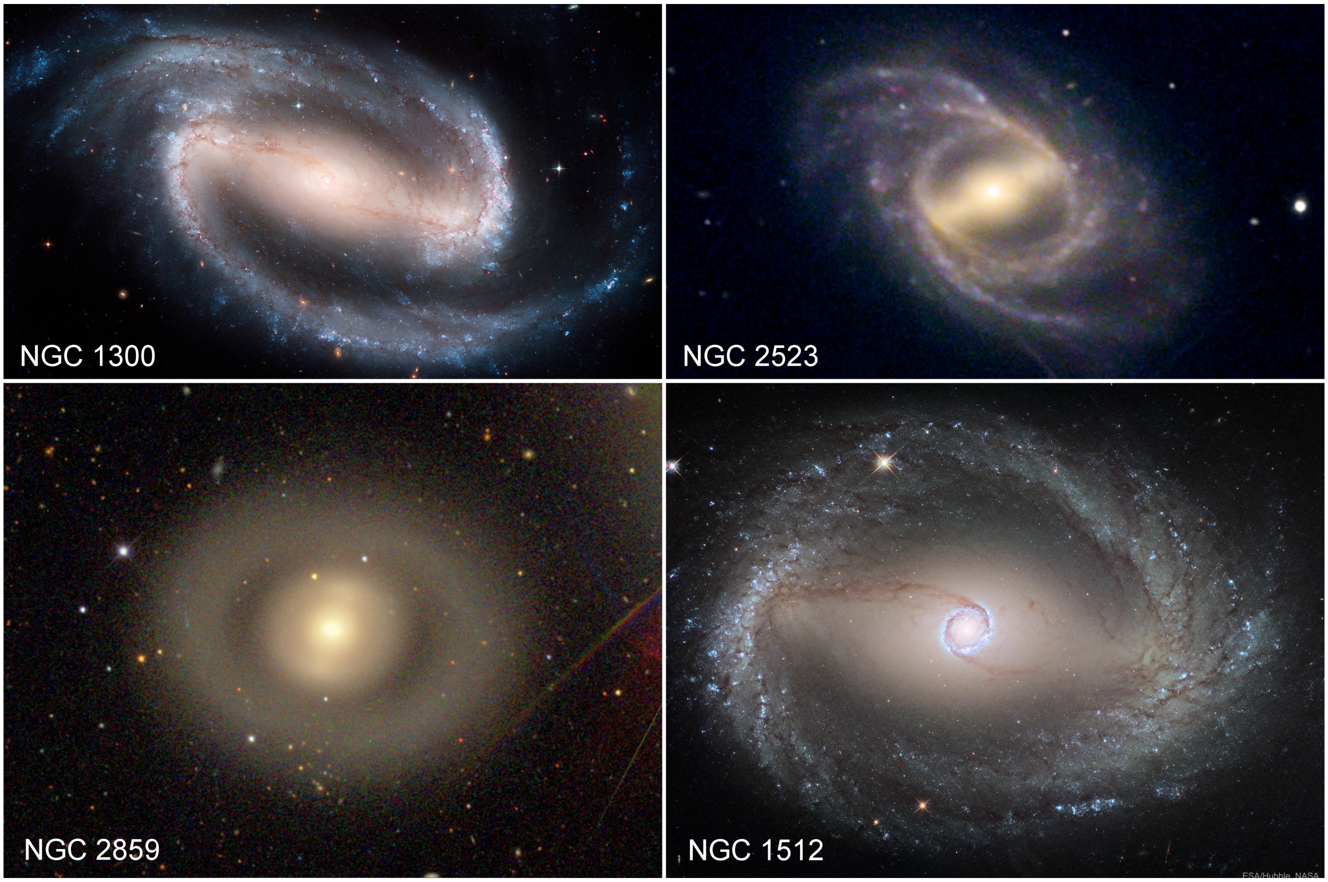
ZIERLER: Right.

KORMENDY: In astronomy, it refers to a process that has a long time scale. Secular means slow. Slow with respect to what? Well, slow with respect to the "dynamical time" – the time that a system takes to rotate or, if you remove pressure, to collapse gravitationally. There are two characteristic times scales for stuff that happens in a galaxy or a star. One is the dynamical time. If you whack a body, how quickly does it jiggle? You can whack a galaxy by colliding it with another galaxy. How quickly does it respond, in this case with tidal effects? The answer is the rotation time, more or less. For our Milky Way, that's about two hundred million years. The other time is the secular time, which is long compared to the dynamical time. It is the time scale for processes that are slow. When I talk about the secular evolution of galaxy disks, I mean the slow rearranging of energy and angular momentum that is driven by non-axisymmetries. Bars are the most obvious driving agents. They change a galaxy fundamentally if you give them enough time. Enough time means billions of years.

ZIERLER: And what was exciting about this field?

KORMENDY: I said that a goal of my career has been to develop an understanding of all aspects of galaxy structure that people like morphologists see and encode in their classifications. For example, many galaxies have a bar, often with an "inner ring" of stars around the end of the bar. In other cases, spiral arms start at the end of the bar. Sometimes there is an inner ring around the bar and a separate "outer ring" at twice the radius of the inner ring. Sometimes there is also or instead a tiny "nuclear ring" of young stars way inside the much longer bar. Inner and outer rings have designations in Gerard de Vaucouleurs' famous galaxy classification; nuclear rings are understood to occur but don't have a designation. Other features that you see in many galaxies get encoded in the classification, too. When I was a grad student, we didn't understand any of them. We were beginning to understand that spiral structure consists of a density wave. We began to understand the difference between global spiral structure – usually two arms that stay coherent as they wind through the disk – versus flocculent spiral structure, where there are many little spiral wisps but no coherent spiral structure. We partly understood this, with some help from the paper I wrote with Colin Norman. And we had glimmerings of an understanding of inner rings. But we didn't understand much more. I wanted to understand it all. As well as possible, anyway.

Now in 2021, we have made a lot of progress. Even by 2004, when I wrote my ARA&A review of secular evolution with Rob Kennicutt, we had come to understand that all the features that are regular – all three rings, dust lanes, spiral structures, and some star formation properties – are consequences of this slow internal evolution. Getting there from an observer's point of view has been a thread of my career ever since 1979, when I wrote my first paper on the subject. Many theoriest helped enormously, too. An especially important one is Lia Athanassoula, who made computer simulations of how bars rearrange disk gas into some of the features that I mentioned. My job has been to figure out the observational side of the story.



These pictures show some of the structures in galaxies that our picture of disk secular evolution now explains. All four galaxies have bars – bars are the strongest and most common engines that drive the evolution. In NGC 1300, two beautifully coherent spiral arms trail out from the ends of the bar. This happens early in the evolution. You'll recall that Colin Norman and I wrote a paper that helped to develop the observational case that bars drive spiral structure. NGC 2523 and NGC 1512 have "inner rings" around the end of the bar. Farther out, NGC 2523 has spiral arms; NGC 1512 does, too, but the field of view of this Hubble picture is too small to show them. NGC 1512 also has a nuclear ring of star formation that is building a "fake bulge" or "pseudobulge"; I've spent a lot of time in my career studying pseudobulges. The dark lanes of dust absorption in NGC 1512 and NGC 1300 are signatures of secular evolution, too. This is where disk gas – including dust that goes along for the ride – undergoes a shock that compresses it, slows it down, and forces it to drop toward the center, feeding star formation there. NGC 2859 is a barred S0 galaxy – "S0" means that it has lost its gas and no longer forms stars – but it has a beautiful example of an "outer ring". Back when the galaxy had gas, secular evolution pushed some gas in the disk outward into the outer ring, where it formed the stars that we see today. So all three rings, the common occurrence of dust lanes in the bar, and central pseudobulges (not classical bulges that are like elliptical galaxies) are all products of disk secular evolution.

So the "bottom line" now is that we have at least a heuristic understanding of every structural component that is encoded in classifications, plus several more that are not encoded. And, separate from our discussion here, we know that the many examples of peculiar galaxies, all different, are galaxy collisions and mergers in progress. OK, plenty of engineering details still need to be worked out. But what I care about is that we understand the fundamentals of how these features are created.

The funny thing, as a postscript to my answer, is that research on galaxy evolution has evolved since my student days in a different direction than what I just said. I have become an outlier, because my interest is understanding structure. That's not what most people do nowadays. Most people now try to understand the history of star formation. It's funny how that happens. OK, I'm an outlier. I am what I am.

ZIERLER: John, when you started to get interested in gravitational waves, was that more as an observer, or because you recognized the value to some of the broader questions you were asking?

KORMENDY: I presume you mean the LIGO detections?

ZIERLER: Exactly.

KORMENDY: I got interested in part because of supermassive black holes. The LIGO results inform all work on black holes. We want to know the origin of supermassive black holes. And we are surprised to find that billion-solar-mass black holes already existed and shone as quasars very early in the history of the Universe. How did black holes get so big so fast? Because there is thought to be a limit on how quickly you can feed a black hole. It's called the Eddington limit. If you throw too much stuff at a black hole, then the intense radiation from hot gas that's about to be swallowed puts so much pressure on the infalling gas that it blows that gas away. When you do the calculations, you find that you cannot grow a black hole from a mass of a few Suns – the kind of black hole that forms now when the biggest stars die – you can't grow one of these up to billions of solar masses in the short time that the most distant quasars had since the big bang.

This was a puzzle. The fundamental insight on how you solve it has been around longer than LIGO detection. But to me, the LIGO detections nail it. Here's why:

The people who work on the first stars in the universe – Volker Bromm in our UT Astronomy Department is one of them – conclude that the first stars that formed in the Universe were more massive than stars can be now. Because there were no heavy elements in the gas immediately after the big bang. So stars were more transparent, because heavy elements in stars like the Sun absorb a lot of radiation. Being more transparent means that the radiation pressure that prevents you from collecting more than about 100 solar masses together to make a star was much smaller.

This evades a constraint on how big you can make a star – a constraint that's like the Eddington limit. So in the early Universe, you can make stars that are much more massive than 100 Suns. If you make a star out of just hydrogen and helium, you can grow it up to a mass of several hundred solar masses. Such stars die quickly and can, at least in some cases, make black holes with masses of order 100 solar masses. That's a lot bigger than the black holes that form now. They have masses typically of order 5 solar masses. OK, that's a start.

The other part of the solution to our puzzle is that the first gravitationally bound protogalaxies were tiny. And there were a lot more of them than we have big galaxies now. Because today's big galaxies grew gradually as their small progenitors hierarchically clustered and merged. So I don't see how you can avoid the notion that lots of small fragments that later merged to make, say, our Milky Way, had at least a few 100ish-solar-mass black holes each. If you hierarchically merge these fragments and they still have some gas in them, then the 100-solar-mass black holes arguably merge. So it seems nearly inevitable that you grow black holes from roughly 100 solar masses to a thousand solar masses or 10,000 solar masses by hierarchical clustering and merging the black holes that you made in first-generation stars. This idea has been suggested by many people, including Marta Volonteri, who works on the engineering details. The point then is that you can grow, say, 10,000-solar-mass black holes up to a million or even a billion Suns a lot faster than if you start from five solar masses. That has been the picture in my mind of how supermassive black holes got their start. For decades. Thirty years ago, we didn't know about the first stars. But we knew about hierarchical clustering, and that's enough for this discussion.

Then LIGO started to discover black hole mergers and to measure their masses. And by now we have seen about 30 black-hole–black-hole mergers in which the products are black holes with masses up to about 160 solar masses. That was a surprise. People thought that the masses would be smaller, more like 2 x 5 = 10 solar masses. But from my point of view, it is a big bonus. Because we just made the jump from 10 to about 100 solar masses. So now, when these merge in a tiny protogalaxy – one of hundreds that will build a giant galaxy of today – you quickly get interestingly big masses. And a big help that you get for free is that these protogalaxies still contain lots of gas. That helps mergers to happen more quickly. So it becomes easier to believe in the scenario that I described. The LIGO results nailed down the early part of what used to be just a theory of the origin of supermassive black holes in a way that is quite wonderful.

ZIERLER: Were your interests in searching for life on exoplanets more of a hobby or did you see that integrated into your overall research agenda?

KORMENDY: It's a hobby. We're now getting away from my research and talking about research that I don't do but am interested in. Biology is important to my life. I said earlier that I'm a dedicated birder. Most trips that we've taken these last – wow: it's already 25 years – have been for birding. We became birders in 1996. So the complexities and beauty of nature and of birds and jungles are central to my lifestyle. The idea that there is life elsewhere in the Universe has always been a fascination. And it connects up with this biology part of my life that isn't part of my profession. I've always liked it. I always liked nature.

ZIERLER: Maybe it's a silly question, but in all of your star gazing, in all of the ways that you've thought about the Universe, do you feel like you have any unique insight into the fundamental question of whether or not there's life out there?

KORMENDY: It don't have insight that others haven't articulated. If I have a strength, it is to pull lots of apparently disconnected ideas together and notice something new. It's almost like emergent behavior, isn't it? Something that's more than the sum of the parts. What speaks to me is the observation of extremophiles on Earth and the realization that some of these environments are similar to extreme environments in places like Mars. But any conclusion is only an educated guess. Still, it's a slightly educated guess. I can't avoid the thought that there are environments on Mars that are good enough now and that used to be better. If you ask me where in the Solar System I think it is most likely that we will find life, Mars comes first. Especially underground, maybe in lava tubes. What comes second? For me, the answer is probably not, say, Europa, although there are good reasons to like the fact that Europa has liquid water under its ice surface. My answer might be the middle atmosphere of Jupiter. All the right chemicals are present. The temperature is comfortable for you and me. Lightning provides energy to ionize atoms and drive chemical reactions. I find it difficult to believe, as some suggest, that life needed a solid crystal surface to get started. I suspect that floating in an ocean of water on Earth or a mixture of gases on Jupiter, with plenty of liquid water around, is good enough. I admit that it is probably easier to concentrate dilute chemicals in an evaporating pond on Earth, not in the atmosphere of Jupiter. Still, I would not be surprised if, 50 years from now, we commonly teach in our intro astronomy courses that there is floating life on Jupiter. Sort of like jellyfish.

ZIERLER: To look beyond our Solar System to exoplanets that might be more hospitable to complex life, do you think that long-term searching for biosignatures or technosignatures is going to be the easier way to detect the possibility of intelligent life?

KORMENDY: I'm very strongly in favor of looking for biosignatures. I am not going vote against technosignatures. But if you have a limited budget and you want to place your bets where the chances for a payoff are highest, I think that the targets are biosignatures. I'd rather see a big effort going to biosignatures than an equal effort going to bio- and technosignatures. If or when biosignatures pay off, as I hope they will, then we can revisit the question of how much effort to put into technosignatures.

People who work on this subject or discuss it for the public like to base their discussions on the Drake equation. It tries to summarize our best guesses about the probabilities involved – the probability that stars have planets, the probability that life will form if there is a suitable planet, and so on. I don't like to use the Drake equation. When I teach this subject to undergrads, I never mention the Drake equation. What I like is evidence, and the evidence that I tell students about centers on extremophiles, on conditions on Jupiter that don’t sound bad in the context of Earth’s extremophiles, and conditions on Mars, including seasonal methane blooms and caves. That stuff speaks to me.

ZIERLER: Just to play devil's advocate about looking for biosignatures over technosignatures, doesn't that run into the possibility that there are intelligent life forms for which the kinds of biosignatures that we look for simply are not needed?

KORMENDY: Sure. I ask myself: What are the biggest dangers for humanity long-term? One danger is that we will turn into electronic beings via an intermediate stage when virtual reality becomes dangerously seductive. Electronic beings might have no detectable biosignatures other than radiation of a temperature that I can't predict. So sure, you're right. But in a constrained world where any search is like buying an expensive lottery ticket, I prefer to buy a lottery ticket where the chances of a payoff look best. Because you need that first. I had a similar experience in searching for black holes. In the long run, you want to search in a representative collection of galaxies. But first, you “stack the deck” in your favor – you look in galaxies where the chances are biggest and the detection is easiest. Because you get lots of telescope time more easily if you first demonstrate some success.

ZIERLER: This raises the question, the step two and step three question: If we do detect life, how do we say hello? Should we say hello?

KORMENDY: Hard question. I have a lecture for my undergraduate classes in which I point out that we humans have a very bad history when technologically advanced groups of us met less technologically advanced tribes of us. I don’t know of a single case in which this worked out well. So … what should we wish for, in terms of meeting extraterrestrial intelligence? Another worry is this: If we know that they know everything that we could hope to learn in the next 1000 years, what does that do to our psychology? To our motivation? In contrast, if we find out that something can be done that’s close to what we already know, then that can be an impetus to do it. For example, if a spy learns something from another country, then it is easier to replicate it in his or her home counry. But if the gap between what we know and what they know is orders of magnitude bigger, then finding this out could be discouraging. So this is a question about which I don’t have a well-formulated answer. I'm sorry about that, because I don’t like to be negative, and it may some day be relevant. But it is how I feel.

ZIERLER: To reverse the question, do you think that we are known to other beings, if they are out there?

KORMENDY: Well, it's a lot harder than people think. I have somehow gotten connected with a blog that circulates people's questions and answers. It gives me a window on how nonscientists think. People have no idea about how hard it is to use the signals that a planet emits. They have this idea that aliens 58 light years away from us could build a telescope that's big enough to see the assassination of John Kennedy. But you get a few photons, that far away. If any. Light is quantized, so you can't build a detector that's big enough to amplify what you see if what you see is grainy and you have almost no grains. And the image is hopelessly blurred. It is hard to believe that you could reconstruct an episode of I Love Lucy out at the distance where that signal is traveling right now. And a separate question is whether we want them to think that that's what we are like.

ZIERLER: (laughs)

KORMENDY: So this subject is delightfully suitable for speculation, but a lot of speculation is naive.

ZIERLER: Do you see any value in bringing our conversation up to the present time and discussing possibilities in quantum computing and simulated astronomy? Seeing things in simulations that we might not be able to see by actual, physical observations?

KORMENDY: That's a good question and a hard one to answer. I'm not an expert in quantum computing, so I can't comment on it. But I can comment on the similations that astronomers make now. About those, I have mixed feelings:

Simulations are now an enormous and welcome part of astronomy. They contribute so much! So why do I have mixed feelings? Well, the people who do simulations have become a culture unto themselves. But there is some tendency for the practitioners to talk mainly with each other. Contact between their culture and observational astronomy could be better. Also, the simulations are very powerful and very clever and truly remarkable in how much hard physics they include. But this can make simulators be overconfident in their results. You can get a culture where the feeling is that, if it happens in my models, it must happen in the Universe. And if it doesn't happen in my models, it can't happen in the Universe either. And that can be true even if some of the "trees" they are "barking up" are wrong. An example was AGN energy feedback, right? People need feedback to make realistic galaxies, but they might not have the right interpretation. Maybe it isn't feedback from AGNs. Maybe it's something else. Such a modeling program may have dozens of interlocking "control knobs", and if you adjust them all in the right wrong way, you get realistic results. But maybe nature didn't do that. Maybe nature adjusted 14 of those knobs in a different way and got the same results. Or better results.

So simulations are very powerful and a little dangerous. It is very good for astronomy that they are being developed. They have gotten enormously – really *enormously* – better in recent years. And they are still getting better all the time. But it is easy to talk yourself into believing more than you have proven. I suspect that this is more or less the theme on which my name came up in your conversation with Jim Peebles, who also worries about such things. I discussed that context – the surprising abundance of pure-disk galaxies – earlier.

Some of my worries come in part from observations that I have made. I'll give you an example, sort of the opposite end of the spectrum from the puzzle of pure disks. It also involves mergers.

In the 1980s, there was a revolution in our thinking about galaxy evolution. We realized for the first time that evolution doesn't happen in isolation. A lot of galaxy evolution happens via the interactions between galaxies, and in particular, via galaxy mergers. The person who started this revolution is Alar Toomre. The crunch moment came when he gave a talk at a conference on galaxy evolution at Yale University. This was in 1977, when I was a young postdoc. The talk was pedagogically dazzling, and by the end, you realized that you just witnessed history. Alar's theme was that, when galaxies collide, their mutual gravity distorts them both, warping and stretching and scrambling their disks. The energy that this requires is taken away from the orbital energy, and the result is that the galaxies don't separate much after the collision. A few dynamical times later, they merge into one remnant that's no longer flat. And that, he suggested, is how you make classical bulges and ellipticals. It took a while for the idea to catch on. But by the middle 1990s, it was a giant bandwagon. We overdid it. By that time, galaxy mergers dominated our thinking about galaxy evolution.

In contrast, now, in 2021, mergers have all but disappeared from the evolution story. They are viewed by the simulation culture as a messy detail that doesn't control much that's important. Meanwhile, the evidence for mergers has not disappeared. Instead, what happened was that the simulators started "from scratch" – from a beginning that was defined by observations of initial fluctuations in the temperature of the microwave background radiation that's left over from the big bang. They put into their simulations cold dark matter and baryons, and they started to calculate. Hierarchical clustering happened, but major mergers of similar-mass galaxies – mergers like those discussed by Toomre – were not a big part of the story. For a long time, I didn't understand why this change happened. But I watched their simulations, and I talked with the simulators – especially to Joel Primack. We looked at the simulations together. And I realized: What you see isn't just two extremes, either nothing going on or major mergers. It is true that collisions of fragments and minor mergers are happening almost all the time. I mentioned this earlier, when I said that there is so much dynamical violence that it is hard to understand how you form a pure-disk galaxy with no classical bulge. There's stuff going on all the time. Because the simulations always show the dark matter. Maybe they add visible matter with particles of a different color. But mostly, you see hierarchical clustering. And there isn't a lot of quiet time punctuated by the occasional major merger, then with more quiet time, and then another major merger. Violence happens all the time. So minor mergers become the main thing that people think about, and major mergers become ... a distraction. I think I understand how the simulators feel. But now, you are talking with an observer, and I see galaxies differently.

When I look at galaxies, I don't see all this busyness. I see lots of galaxies that sit by themselves, maybe with one or two companions. Of course, I don't see dark matter and its enormous amount of substructure. Instead, I see lots of evidence for secular evolution – signatures which show that it is happening. And its products, such as inner and outer rings, are fragile. It is hard to see how these fragile features can form – and harder still to see how they can survive – if every galaxy is being pummeled by dark matter subhalos all the time. Secular evolution can’t be the dominant process that shapes galaxies if there is too much violence. They present us with a puzzle that’s similar to the puzzle of pure disks. So I see galaxies as much more isolated and slowly evolving, with occasional violent events that make a small number of mergers in progress. Ellipticals and bulges look like the outcome of those major mergers. Why? Well, when we measure observed mergers in progress, they satisfy the scaling laws for bulges and ellipticals.

And, when people simulate individual merger events and include gas and star formation, they get specific signatures of gas-rich mergers that I see in my observations of elliptical galaxies. Some of these signatues also are sensitive to too much violence. So I – and, historically, lots of other people – see very specific evidence that major merges happen and produce objects that we can identify via specific merger signatures. So we now have two cultures – the simulators and people like me – that are immiscible and that don’t talk to each other enough. Each culture runs its own conferences and invites people mainly from its own culture. I sometimes get invited to meetings dominated by anti-merger people, but my contributions don’t have much influence.

It’s not good that our cultures are immiscible. At least, we tend to be friendly toward each other. If I go to U C Santa Cruz, I have very enjoyable conversations with Joel Primack. Also with Jerry Ostriker – he is a valued friend. But I don't change their minds about anything, and they don't change mine, much. Not zero. At least I think I can articulate the problem now. But it isn't solved. So how do we solve it?

This is an example of how simulation has gone off in a direction and into a culture that, to me, feels partly compelling and partly alien. It would be good if we could fix this. I don’t know what would work. What I wish would happen for a start is easy to suggest. We need a workshop like the Weizmann Institute workshop that I mentioned earlier, where the main proponents of each culture get together, not for a meeting of 5 days with 20 short talks every day but rather for a meeting where each side presents its point of view in depth and we confront each other’s strong and weak points. It is hard to arrange such a meeting. People are too busy to want to invest what probably would be about a month. Covid makes it still harder. But right now, we are stuck in a world of several different cultures, even in a subject as restricted as galaxy evolution. Each side makes progress in part by ignoring the other side. It is time to do better.

ZIERLER: Yeah. On a related subject, in spending much of your career thinking about black holes, what was your reaction, even your emotional response, to the image of a black hole from the Event Horizon telescope?

KORMENDY: Oh, it was wonderful. But it wasn't as revolutionary as many people think. Because the evidence for black holes had gotten so good that the EHT telescope image felt like a wonderfully detailed confirmation of a picture in which we'd already come to believe. Why? Partly because of LIGO. LIGO was more revolutionary. LIGO allowed us to believe in black holes at a level that we didn't dare to have, before. When I think about the precision with which LIGO measured the separation of its detectors, well: to call it mind-boggling doesn't come close. So LIGO had already done something to my mind. The observations by Genzel and Ghez of the black hole at the Galactic center made a convincing case that black holes exist. A lot of work on stellar-mass black holes did the same. My work and the work of others to look for black holes in other galaxies and to understand their demographics in the big-picture context of galaxy evolution created a compelling picture. So we were in a situation where, if the EHT picture *hadn't* looked like that, we would have been enormously shocked. But the fact that it looked as predicted was a vindication of a lot of other, already pretty convincing work. And of course, you have to admire the technical prowess that made it happen. It was a very difficult observation. Still, by the time it happened, it was more of a confirmation than a surprise.

And it provided a bonus that's important to those of us who use stellar dynamics to measure black hole masses. The EHT measurement of the mass of the black hole in Messier 87 agrees with the best stellar dynamical measurement. It doesn't agree with a Hubble measurement that uses spectral emission lines from gas. We had reasons to suspect the gas-dynamic measurement. EHT confirms that stellar dynamics give the more accurate mass. That's helpful in a business where you always have to look for systematic measurement errors that you may not understand.

ZIERLER: I'll just note editorially that this line of comment [EHT provided confirmation, not a revolutionary new result] is remarkably similar to how many particle theorists felt about the discovery of the Higgs particle.

KORMENDY: Yeah, I could imagine that. (laughs)

ZIERLER: John, now that we've worked our way up to the present, for the last part of our interview, I'd like to ask some broadly retrospective questions about your career. And then we'll end by looking to the future. So my first question is about collaborations and work styles. Your publication list seems to show a theme about when you work solo and when you work with collaborators. I wonder if you can explain a little bit about your decisions about the kinds of projects that you do on your own and the kinds where it's beneficial to have more heads than one at work on the problem.

KORMENDY: The drive to collaborate broadly, which I have done only in the Nuker team, was driven by the sociology of how Hubble as a resource would get used. It was also driven in part by the expertise that we needed in order to make the next step, from the first proof-of-concept black hole detections to a time when black hole measurement is part of our daily arsenal. It was inevitable that a change in collaboration style had to happen, because no one person has all the necessary expertise. It was also inevitable because people connected with Hubble advertised broadly that users would not get a lot of time unless they pooled expertise. They told us that they would prefer not to see lots of individual proposals that address small questions. Instead, they wanted big and powerful teams to address big-issue questions that require lots of telescope time.

So I have not mentioned this yet, but this is how the Nuker team got started. At the time, I was working on galaxy centers independently of anybody else, using the CFHT. Tod Lauer was doing the same, using other telescopes. Tod came to me and said, "We really ought to get together for Hubble." I realized that he included Sandy Faber, too; she had been his thesis advisor, and now, they both were involved with Hubble as insiders in the Wide Field/Planetary Camera team. So I said that I would agree to be part of a team, provided that we also invite Doug Richstone and Scott Tremaine. Because we need their dynamical modeling expertise. They agreed. That's how the Nuker team got started, with 5 core members. After that, it grew much bigger, over the years, as we broadened our expertise. For example, we later included AGN people like Luis Ho and Alex Filippenko. And we added excellent young postdocs.

This is typical of what happens more generally now. Some astronomy subjects nowadays can't be addressed except in big teams. Black hole teams like LIGO and EHT are examples. Another example is studies of the microwave background, either using satellites – which always require big teams – or using ground-based microwave facilities, for example in Antarctica. Because of work on the microwave background, cosmology has made a transition from almost science fiction to spectacularly quantitative astrophysics. This happened in part because of advances in detectors and in part because of giant collaborations where a spectacular amount of expertise is put together. So far, the ultimate step is LIGO, which has more than 1000 collaborators. I personally cannot imagine working in such an environment. Because I don't enjoy being a small piece of a big machine, no matter how important the science is. It's just not fun. That legislates me out of Nobel-Prize-quality work. But for me, research is enjoyable if I understand that I can answer the questions that I ask. I'm happy if I have an answer when somebody asks me, "What have you discovered lately?" I want to have an answer, or I don't feel good about the last few weeks of my career.

Psychologically, I am a loner. I work best alone or in collaboration with one or two people. I collaborate for two reasons. One is that I enjoy working with someone. Ralf Bender and I have written lots papers together, because our expertise is complementary and yet similar, and because we are good friends. Ralf is my best friend in the world after my wife. We have a lot of fun. Discovery is fun in the context of our interactions with each other. In contrast, the Nuker team is less about having fun, though it is a heady experience to work with so many superb people. But that collaboration was motivated by what it takes to address an important problem at the level that the problem needs, given resources and constraints. So in that case, I made the jump from two or three people to around 20 people. The need to make that choice was partly practical and partly driven by sociology. It was not negotiable if I wanted to work on black holes in the Hubble era. And the jump from 20 people to a LIGO collaboration is equally necessary for a growing number of subjects. But I would not enjoy it, and I don't have any expertise that such a team would need. So now, as I reach an age when most people retire, I choose to do what's fun, and I work on problems that I can study in small collaborations. Thankfully, there are enough important problems so that I can continue my career. But I am increasingly an anachronism.

ZIERLER: Absolutely.

KORMENDY: I have to say: I'm sorry that I give you such long answers. Before we started, I told myself, over and over, to resist the temptation to give long answers …

ZIERLER: (laughs) No, no, no, no. No, that's what it's all about, John. And I want another long answer for this one. It has been 50 years since you got to Caltech, right? The improvements in instrumentation in your field – they boggle the mind. What stick out in your memory as some of those advances that were really a quantum leap? Both in the kinds of things that you could see and the kinds of questions you can address.

KORMENDY: Yeah, very good question. There have been a number of revolutions, and some are so obvious that they won't surprise you. The number of big telescopes with powerful instruments has increased enormously. We no longer have a situation where only the top-level astronomers at Caltech and Lick Observatory can address the most important problems. Early on, Kitt Peak and Cerro Tololo observatories, together with counterparts at radio wavelengths, already enfranchised many people. Now, people are doing really first-rate science everywhere. Public facilities define the state of the art. They are available to everybody.

Another obvious answer is the development of detectors that are more sensitive and linear than photographic plates. CCDs changed my world. CCDs changed everybody's world. And now we have detectors for essentially the whole spectrum of wavelengths that nature produces. We have exotic telescopes – none more exotic than LIGO! We have telecopes in space. All this was just getting started when I was a student. Then, astronomy was mostly optical with the relatively recent – but important – addition of radio astronomy. But for example, a vital part of why the dark matter revolution caught on was the development of X-ray astronomy. X-ray-emitting hot gas in clusters of galaxies could be understood only if dark matter provided a lot more gravity than the visible stars that we had been studying for decades. Infrared astronomy got started at about the same time. And by now, we have branched out beyond just using the electromagnetic spectrum to "multimessenger" work. We observe neutrinos and gravitational waves. All this is transformative in ways that are just superb. That's the easy part of my answer.

There have been other revolutions, ones that may seem minor by comparison but that add up to a transformative change in my life style and the life style of all astronomers. One is the availability of word processors. Now, the time that it takes me to write a paper is no longer controlled by a secretary who is always too busy with all the people who depend on him or her. Now, I have the luxury of not being finished with a paper until I have examined and tweaked every word. Until I can defend every word. That changed my life fundamentally. Because I can write papers in a way that wasn't practical before. Because I never failed to exhaust the resources of typists and other support people.

Another similar revolution was access to digital images and the software to analyze them. They let us turn digital signals from a telescope into photons and then into science in transformative ways whose importance is hard to overstate. This, too, changed my life. I experienced it first during my postdoc at KPNO and then later as a staff member at the DAO. Now, we do all this with laptop computers. A young grad student can do more powerful analysis in the first month of study than any Caltech faculty member could do in 1976, the year I got my PhD.

Also, programs like Powerpoint make it so much easier to communicate science. As I say in my book on metrics, the mantra for the 2020s is not "It isn't finished until it is published." Today's mantra should be: "It isn't finished until you have convinced them." Convincing has to do with how you communicate science. The archival part of this is done via papers. But we shouldn't kid ourselves: a lot of communication is done through colloquia, oral papers at conferences, and private talks. And the technology that makes this easy is programs like Powerpoint. They make it easy to include photographs and movies. No more 35 mm slides.

When I look at my undergraduate days, I can't help but laugh at the magnitude of the changes. I remember the first time I had access to a desk calculator that had four memory registers. (laughs) Almost nothing that I do now could be done then. And the science progressed accordingly. The standards for the next advances have progressed, too. We learn things now that we couldn't dream of learning then. Imagine a cosmologist from the 1970s confronted with today's results. How otherworldly that is. If you recall our conversation about communicating with aliens and think about communicating with ourselves as we were only 50 years ago, you can see why I said what I said about communicating with a civilization that's the equivalent of 1000 years ahead of us. What would that be like? The mind boggles … in ways that are both good and bad.

Anyway, these are some of the transformative events that I can think of.

ZIERLER: I know that you downplay the distinctions that we discussed earlier between astronomy and astrophysics and cosmology, but to the extent that we categorize the most fundamental, the most existential questions along those disciplines, however you might choose to answer this, over the course of your career, what have been some of the most satisfactory moments that your research has contributed to those fundamental questions about how the universe works?

KORMENDY: The biggest surprise was the realization that spheroidal galaxies are defunct disks. This brings into our conversation some physics that we haven't discussed. What are you studying when you make that discovery? The answer is environmental evolution. Because we now believe that the transformation of spiral galaxy disks and irregular galaxies into spheroidals happens mainly by the ram-pressure stripping of the cold gas in a galaxy by the X-ray-emitting hot gas (say) in the Virgo cluster. That's why the Virgo cluster is dominated in numbers by galaxies that are ram-pressure stripped. Now, these gasless galaxies are S0s if they have high masses and spheroidals if they are dwarfs. There are more spheroidals in Virgo than all other galaxies put together. By a giant factor. That's environmental evolution. That's another major theme of my research. And it got started when I discovered that bulges and ellipticals satisfy one set of scaling laws and spheroidals, S0 disks, and spiral galaxy disks satisfy another, different set of scaling laws. That's the thing that I contributed that was the biggest surprise.

And let me say, please, one more thing that means a lot to me. My biggest hero for much of my career, especially in the early days, was Allan Sandage. Besides being a role model, he did a lot to further my career, for which I am very grateful. He had an expansive personality. But it was also tortured, not least by controversy about the distance scale. He did not welcome outrageous new suggestions that threatened his work or Hubble's work before him. Here's the important point: One of the great satisfactions of my life is that he never disagreed with me on the distinction between spheroidals and ellipticals. In fact, he was persuaded, after I had said it, by an observation that he was struggling with himself and that is now part of the evidence that I use. He realized that the luminosity function of dwarf ellipticals and the luminosity function of ellipticals are distinct and different from each other. In saying this, he was in a totally surreal situation that I'm not sure he ever articulated. Because he was able to recognize and distinguish "elliptical galaxies" and "dwarf elliptical galaxies" with the same luminosity. Why did he call one galaxy an "elliptical" and another a "dwarf elliptical" if they have the same luminosity and the same morphology? Isn't that crazy? Well, the reason is that they look different. A dwarf elliptical is much lower in surface brightness and much larger in size than an elliptical with the same total luminosity. This is just a statement in words of what the different scaling laws show. And it is easy to see in photographs, which is how Sandage classified galaxies. So he agreed with my conclusions and even credited me with the discovery. I feel good about that. So I was friends with Sandage in a slightly weird way for all of my career. He also did not disagree with the picture of disk secular evolution that I have been developing all through my career. When I was a postdoc, he even let me use his office after he went home for the day, so I could look at the original photographic plates of the *Hubble Atlas of Galaxies*. That was a remarkable privilege. I was very careful with those plates. And he wrote letters of recommendation that helped me to get jobs. So I owe him a lot. I take seriously the responsibility of trying to understand physically the galaxies that he and Hubble classified qualitatively. And to do this sensibly and with rigor.

I'm sorry, I think I got off the track.

ZIERLER: No, that's it. That's it.

KORMENDY: You asked about my contributions. The other one was the scaling laws between black holes and host galaxies. It was not just the discovery of black holes. I was also the first person to illustrate the correlation between black hole mass and bulge luminosity, in 1993 and again in 1995. That subject has blossomed beautifully since then.

ZIERLER: John, for my last question I'd like to gently push back at some regret that you expressed earlier, that you never had a graduate student that you see on the academic tree as picking up the mantle for the next generation. That may be true insofar as you see it, but it fails to consider the generation of astronomers who learned from your research. In that sense, looking to the future, what are the ways that the next generation of astronomers will take your research in directions that by dint of your own life and retirement, you won't be able to go yourself? What are you most optimistic about as the next generation builds on what you did in your career?

KORMENDY: I hope you’re right about the next generation of astronomers.

More generally, I'm not sure that my answer will be about the followup on my career, but I can try to answer in the context of where galaxy research goes next. You remember the immiscible cultures of simulators and observers. I would like to see such divisions and self-identifications disappear. I’d like to see us all get together to thrash out a compromise picture that combines the best elements of both cultures into a new synthesis that is better than either. But it’s a hard thing to realize. One reason is psychological: each culture has interests vested in many publications. I, at least, am willing to be persuaded that I’m missing important points that the simulators and theorists got right. Other problems are practical – the cost in time, effort, and money of getting together for (say) a month-long workshop. And in a larger context, everybody finds it hard to cope with the mountain of work that has been done in recent decades. It’s such a lot to absorb.

It’s still harder to prune what turns out to be the good stuff from what turns out to be wrongheaded or irrelevant. And to put it together in what has to be be a complicated picture. Especially since there’s a lot that we still don’t know. We don’t even know for sure that all dark matter is cold.

So the body of what has been learned is getting so big that it is hard to assimilate. When I was a student, my best teachers at Caltech and Carnegie and Berkeley knew everything worth knowing in astronomy. Now, people know stuff much more deeply, but they are much narrower in what they know and what they do. I used to teach UT’s graduate course on galaxies, and I was always disappointed at how little of what I covered the students absorbed. Then I realized: I lived a more-than-50-year history of the subject. I was immersed in these developments every day. I saw the histories of failures and successes. Now, students listen to me for 1½ hours about 25 times, all while handling other courses and getting their research lives started. I should not be surprised if they have trouble learning in a short time with fragmented concentration what I spent 50 years learning. Subcultures fragment astronomy as it gets bigger. People don’t know and aren’t interested in what we did 30 years ago. Papers from 2000 are ancient to today’s students. Astronomy is getting a “feel” like the culture in “Game of Thrones”. There’s a lot of prehistory that young people don’t know. Sometimes they rediscover it. Sometimes they head off on tangents that look inappropriate to me. It is hard to cope with the practicalities of many decades of research. And harder still to distill new insight from the combination of all things past and the results of my observing run or simulation from last week.

Still more broadly, astronomers are trying to integrate into our world view the most recent big surprise, which is fast radio bursts. Research into transient phenomena is booming. That’s how work on exoplanets got started. And exoplanets are now one of the most important and exciting part of mainstream astronomy.

And surprises. It is the nature of surprises that you can't predict them. If you can predict them, they are not surprises. So what I look forward to most are the discoveries that I cannot predict.

ZIERLER: Well, we'll just have to hang on and find out.

KORMENDY: Yes.

ZIERLER: John, it has been a great pleasure spending this time with you. I'm so glad we connected, and that you've shared your marvelous insights over the course of your career. I'm so deeply appreciative.

KORMENDY: And I’m deeply grateful to you. Thank you *very* much.