Photo by Bob Paz, February 2008

RICHARD ELLIS
(b. 1950)

INTERVIEWED BY
HEIDI ASPATURIAN

January – February 2014

ARCHIVES
CALIFORNIA INSTITUTE OF TECHNOLOGY
Pasadena, California

Subject area
Astronomy

Abstract
Interview in eight sessions (January–February 2014) with Steele Professor of Astronomy Richard Ellis, whose life has taken him from a small coastal town in Wales to the edge of the universe. He recounts that trajectory in this oral history, starting with his upbringing and education in Wales and his youthful enthusiasm for astronomy, which he pursued through studies at University College London (B.Sc. 1971) and Oxford University (D.Phil. 1974). Having the good fortune to begin his career at the dawn of the "golden era" of British astronomy, he describes his years on the faculty of the University of Durham, where he worked with physics department head and future UK Astronomer Royal A. Wolfendale to develop the "Durham group" into an internationally recognized astronomy program. He talks about his work at the Royal Greenwich Observatory, his galactic and extragalactic studies carried out at British observatories and elsewhere, most notably the Anglo-Australian Telescope, and his involvement in mapping the future of British astronomy.

In 1993, he became the Plumian Professor at the University of Cambridge and director of Cambridge's Institute of Astronomy, and in 1999 he joined the faculty of Caltech, where he served as director of Palomar Observatory/Caltech Optical

Observatories (2000–05), carried out pioneering observations at the W. M. Keck Observatories and Hubble Space Telescope, and was centrally involved in still-ongoing efforts to build the Thirty Meter Telescope (TMT).

Ellis details his years of research in observational cosmology, probing galactic evolution and distribution at ever-higher redshifts, and his work on gravitational lensing and dark matter, the cosmic "dark ages" and cosmic dawn, and the pursuit of the most distant objects in the universe. He recalls his role in the 1987 discovery of the first cosmologically distant supernova and subsequent involvement in the supernova cosmology project, an investigation that won the 2011 Nobel Prize in physics for three of its principal scientists. He talks about his collaborations and interactions with numerous colleagues and students, including D. Axon, R. Blandford, A. Boksenberg, G. Efstathiou, D. Lynden-Bell, J. Peebles, M. Rees, W. Sargent, D. Saxon, B. Tinsley, and T. Tombrello, and shares his perspectives on the science and sociology of the astrophysical communities in Great Britain and the United States. Recaps of his election to the UK Royal Society and his designation as a Commander of the British Empire (CBE)—the latter formalized at a Buckingham Palace reception with HRH Prince Charles—also form part of this oral history.

Note: Occasional allusions in this manuscript to a Royal Society memoir or biography refer to an autobiography that Ellis was asked to prepare for the Royal Society at the time he was elected a Fellow in 1995. A copy of the bio is appended to this oral history.

Administrative information

Access

The interview is unrestricted.

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Contact information

Archives, California Institute of Technology
Mail Code 015A-74
Pasadena, CA 91125
Phone: (626)395-2704 Fax: (626)395-4073
Email: archives@caltech.edu

CALIFORNIA INSTITUTE OF TECHNOLOGY ARCHIVES

ORAL HISTORY PROJECT

INTERVIEW WITH RICHARD ELLIS

BY HEIDI ASPATURIAN

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Interview with Richard Ellis by Heidi Aspaturian

Pasadena, California

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SESSION 1

January 15, 2014

ASPATURIAN: This is January 15, 2014, and this is interview session number 1 with Professor Richard Ellis. Let's start by talking about your family.

ELLIS: Okay. I was born in a small village in North Wales, into a Welsh-speaking family. The town where I was born, where the maternity ward is, is Colwyn Bay, which has a railway line going through it to Ireland. But the village in which we lived is Old Colwyn, which is a very small village even now. It's a very pretty place, on the coast, with lots of mountains nearby. My sister was six years older than me, so she was out of the house by the time I was a teenager, and I didn't really overlap with her except in the very early years. My father was a sailor, then a captain in the Merchant Navy. He was away on trips most of the time, four to six months, and then suddenly there would be a father in the house for a month or so on leave.

ASPATURIAN: What do you know about your dad's family?

ELLIS: Oh, quite a lot. My father came from Anglesey, which is an island off the coast of Wales. It was a religious family. His father was a minister so the family moved around Wales. My grandfather—my father's father—was a very famous Baptist minister, Humphrey Ellis, who preached fire and brimstone from the pulpit. He died giving a sermon.

ASPATURIAN: Really?

ELLIS: He had a heart attack giving a sermon in his eighties. I knew him; he died when I was about twelve, I think. This was a period—in the 1910s, '20s, and '30s, and even when I was a small boy—in which Wales was a very religious society, so there were lots of nonconformist chapels. Every village had lots of chapels—Baptist chapels, Methodist chapels, Congregationalist chapels.

ASPATURIAN: All Protestant?

ELLIS: All Protestant. So I had to go to church every Sunday, and I was brought up pretty strictly. My grandfather didn't believe in using the telephone on a Sunday, and I wasn't allowed to play with my toys on Sunday.

ASPATURIAN: Do you think that may have been partially why your dad went to sea?

ELLIS: [Laughter] Yes, my father clearly was obsessed with getting away, and his brother was a sailor captain, too. They both ran away from home, my father when he was fourteen. He ran away, and then he was collected by his father. And then eventually, very reluctantly, his father, the minister, agreed that my father could go to sea. I think he was fifteen when he finally signed up as an apprentice on a ship leaving Cardiff; and I have a copy of his contract. It says that he shouldn't go to pubs and all this kind of stuff. My grandfather then went home and burst into tears. Apparently it was the only time his

wife ever saw him cry. And then to cap it all, my father's younger brother did the same. And so they both were sea captains with the same ship line, going to South America.

ASPATURIAN: Was your father in the navy during World War II?

ELLIS: Yes, he was. He was in the Merchant Navy, but his ship was commandeered by the armed forces, and then he was ferrying people across the Atlantic.

ASPATURIAN: He was on the North Atlantic run?

ELLIS: Yes.

ASPATURIAN: Oh, my gosh!

ELLIS: Yes, it was pretty dangerous. Then he got stranded in New York for maybe three months and got a job in Manhattan. He was on the first ship to enter Singapore after the Japanese surrendered.

ASPATURIAN: Oh, that's a pretty dramatic event.

ELLIS: Yes. He was an interesting man. Very understated. He never told me anything. He didn't sit me down and say, "Let me tell you, son, I did this and that." You had to get it out of him. Unfortunately he died of cancer pretty early; he was seventy-eight. I was, let's see, in my thirties. He saw me become a professor at Durham, but he didn't see me go to Cambridge. He certainly didn't see me move to the United States. Whereas my mother went on for another sixteen years. She died when she was ninety-four.

ASPATURIAN: Tell me about her family.

ELLIS: My mother's side is not so distinguished, although there are some famous people in the family. She lived in Old Colwyn all her life. Her father was a gardener in one of these stately old homes outside Old Colwyn. I don't know that her mother worked at all.

Anyway, they had seven daughters, and the sisters were all my aunties, and they were all very religious and very strict. Some of them were softer than others; but, you know, I had to be on my best behavior with the aunties.

ASPATURIAN: Was your mother's family also Baptist?

ELLIS: Yes, Welsh Baptists. If you trace the family back, there are some famous people. There was a professor of philosophy at Glasgow University—Sir Henry Jones. He would have lived in the nineteenth century. He comes from my mother's mother's side, I think. And then much earlier, and this is where my middle name [Salisbury] comes from, another ancestor on my mother's mother's side traces back to a family from Denbighshire, which is the old name of the county where Old Colwyn is located— the county has since been reorganized**.** That ancestor, William Salesbury [also spelled Salusbury, c. 1520– c. 1584], was the first person to attempt to translate the Bible into Welsh from Latin. So he's quite a famous guy. He went to Oxford, but this was a time of religious upheaval in Britain, and so he had to go into hiding.

ASPATURIAN: Under Mary [Queen Mary I, reigned 1552–1558], he went into hiding?

ELLIS: Yes. And then when he finally came to print the Bible he fell out with his printer, and the printer that he eventually used didn't have the letter "K," so the "K" was eliminated from this translation. It is no longer in the Welsh language as a result of this. So he's responsible for eliminating a letter from the Welsh alphabet, which I think is quite hilarious. [Laughter]

ASPATURIAN: That's quite a lineage.

ELLIS: Yes. I haven't achieved anything quite so dramatic.

ASPATURIAN: Was your family Welsh all the way back?

ELLIS: Yes. No English at all. Welsh was my first language as a child, but it was a time of great transition. There were a lot of English children in the schools, because it was a good place to move for English people from Liverpool and Chester. Most of my playmates were English, and of course English was the language of play. I would not impress my school friends if I spoke Welsh. So it became unfashionable, except at home, to speak Welsh.

ASPATURIAN: Your parents must have been bilingual?

ELLIS: Oh, we were all bilingual. You couldn't get by without English because there were so many English people in Wales. English was the language of teaching, but we were also taught Welsh language and Welsh literature as two different subjects in school. The language went through a minimum in, I think, the '50s when I was a young boy. And then eventually in the '60s and '70s, there was a huge campaign to revive it, and one of the Welsh MPs went on a hunger strike insisting on a Welsh TV channel. And eventually the BBC created a Welsh TV channel, and the language recovered. And now actually there are more people speaking Welsh than when I was a boy.

ASPATURIAN: I remember a couple of classmates of mine in high school who were of Welsh descent carrying around Welsh dictionaries, so it must have been a global phenomenon.

ELLIS: Yes. But although I considered myself Welsh and was proud of being Welsh, I wouldn't hesitate to speak English with all my school friends. Particularly as a teenager.

ASPATURIAN: How did you react as a youngster to the strict religious upbringing? Were you an ardent believer or were you a skeptic?

ELLIS: It was part of the family, and so when I was a small boy I didn't think about it too much. I think I just went along with it. There was Sunday school, there was singing. You know, it was the society. These villages had chapels, and that's where you met everybody. When I was a little boy, all those women—and it was mostly women who

used to come up to me, aunties and everybody—would be the people I would meet. These were the adults I grew up with. Everything was focused around the chapel. There was a sermon on Sunday morning; then there were events during the week, and social gatherings and clubs in the evenings. Eventually, probably when I was about eleven, I started to realize that there was a bigger world, and that's when I started doubting. In the Baptist faith, you're baptized as a teenager as a testament of your belief, and it's total immersion in water. So my chapel in Old Colwyn has a pulpit where the minister speaks, and the deacons sit around it—they've been elevated to be important people. They're all men, of course. And then, in the center, where the deacons are, if you lift up the carpet where they're seated, there's a swimming pool underneath. It's a huge cavernous pool, and they fill it with water, and they sing. And the young kids come in, and they're totally immersed in water. And, of course, the time came for Richard to be baptized, and I refused. I said I wasn't convinced that this is what I wanted to do, and my mother was bitterly upset. Fortunately my father defended me, and said that when he was baptized, he and his boy mates all regarded it as a bit of a joke, and that he never thought twice about it. So in the end, I never got baptized. [Laughter]

ASPATURIAN: You mentioned that your father was out of the house a lot, so your primary parent was your mother. And your mother's sisters were kind of auxiliaries?

ELLIS: Yes. And my sister, of course, was still there when I was a small boy, and she played a key role. But my mother was amazing. She was not narrow-minded. My father was a captain, so by the standards of Colwyn Bay society, we were pretty wealthy compared to most of the other families. I remember that every Saturday, my mother would take us for lunch at a fancy restaurant in Colwyn Bay—and we also went to London. She would book a hotel, and we went on the train to London. And occasionally my father's ship would be in dry dock somewhere like Antwerp or Amsterdam or Hamburg, and she would take either my sister or myself—never both of us together—to the ship, and we would stay on the ship with my father. So I did that two or three times, I think—I was seven or eight. That was fantastic. I mean nowadays everybody goes

abroad, but in those days, in the 1950s, to go abroad and then to have a bedroom with a porthole on a ship was special. I wrote a diary of everything I did on those trips.

ASPATURIAN: Did the ship have a name that you recall?

ELLIS: The line that he was in was called the South American Saint Line. It was a Welsh line, operated out of Cardiff, and owned by a very wealthy guy called Lord Howard de Walden, whom I met once in Cambridge after my father died. He's dead now. These lines were bringing refrigerated meat—corned beef and stuff like that—from Argentina. My father's route was mostly Liverpool, Antwerp, Hamburg, Rio de Janeiro, Santos—which is the port of Sao Paulo—Montevideo, and then up the River Plate to Rosario, also Buenos Aires, Bahia Blanca. When he'd come home, he'd bring tins of awful corned beef for us, and he'd bring presents. I remember he brought a watch for me when I was eight or something. We'd traipse to the railroad station to see the train coming in, and I was nervous because my father I hardly knew.

ASPATURIAN: How often did you see him as a child?

ELLIS: Maybe every four months for two weeks or three weeks. Sometimes we would have a holiday, so I'd get dispensation to take time off school. He'd rent a car—eventually we had our own car, but in the '50s we usually rented one—and we'd drive to Cornwall or the south of England or south Wales for a couple of weeks.

ASPATURIAN: It sounds like along with the reasonable income your family had good social standing?

ELLIS: We did. We were lucky. North Wales is not an affluent part of Britain, and it's dreadfully poor now. In those days, there were some wealthy people, of course, but in terms of normal Welsh families like the people I was with at school, I would think we were probably in the top twenty percent of income. But then unfortunately my father had a nervous breakdown.

ASPATURIAN: When was this?

ELLIS: This was 1961. I was eleven or twelve, and suddenly—well, both he and his brother had traumatic events. His brother suddenly collapsed. It turned out he was diabetic, and so he was whisked home—flown back, actually—from South America. And then maybe two years later, my father suddenly was released from his ship, and he was in hospital in Hull; and my mother would go there, and eventually he came home.

ASPATURIAN: What had happened?

ELLIS: It was never clear. After he died, I looked at the letters from Lord Howard de Walden, the owner of the line, and they just say, "Clearly the work was too much for you." And so whether there was some specific event, I don't know. I think he recovered, but I think what happened then was that my mother wouldn't let him go back. She'd been so lonely all these years. You know, she had to do everything, find people who would come and do repairs, and so forth. The entire time that my father was a sailor, my mother would say, "It's all right for Mrs. Jones; she's got a man to fix things in the house." I think she took the nervous breakdown as a good reason to insist that he come home for good. But—what could he do in this small town? It was very sad. My mother refused to move. Normally what retired sailors did was become harbormasters, and of course we lived on the coast, so it would have been an obvious thing to do. His brother got a fabulous job as harbormaster of Caernarfon, which is a famous town along the coast, with a nice castle. It overlooks an estuary—there's a strip of water between Anglesey and the mainland, and it's quite a treacherous piece of water. So my uncle was in charge of all this, making sure that vessels came in and pilots were handy, and stuff like this. My father was offered a similar position towards Liverpool, but my mother refused to leave our small town. So he had to lower his sights, and he was a humble man so he eventually got a job working for a milk company as an accountant. And then he went to high school and got a certificate in accountancy. Then he worked for a lighting company. After being in charge of all these men and ships and seeing the world, it was a huge comedown. You could see it in himself. But he was a very willing guy. Never

expressed frustration. A very humble and decent person who maybe should have stood up for himself a bit more. My mother was much more powerful than he was.

ASPATURIAN: As a small boy, what were your interests? What did you do?

ELLIS: I was interested in geography, and I was really keen on maps. I had an atlas as a small boy, and I knew all of the capitals of the world and all this stuff. I remember when I was seven, there was a quiz in school about capital cities, and I was streaks ahead of everybody—I knew them all. I had two encyclopedias, which I managed to find copies of in a second-hand bookshop a few years ago, and it all came back to me. In those days, you would sit and look at these encyclopedias over and over again, and you would know which pages followed the ones you were looking at. You just devoured everything because there was no television—well, there was television but it wasn't really developed. We were one of the first families in our community to have a TV, but it was entertainment, not education. So geography was the first real interest, I think, and that got me to the library. Colwyn Bay had a very good library. In fact, I found out recently it was one of the Carnegie libraries in Britain. It had a children's section, and my sister and I would go to the library every week and take books out. In those days, it was not computerized; you had a little card.

ASPATURIAN: I remember that too. It was very exciting when you were very young.

ELLIS: So we went into the library, and I picked up two books. One of them was a book about the earth's atmosphere, the troposphere, the stratosphere.

ASPATURIAN: A children's book?

ELLIS: Yes. It was about rockets and space, but it talked about the layers in the atmosphere, and I remember thinking, "Gosh, there's all this stuff out there beyond the earth." And the other book—and I don't know why I picked it out because it didn't have any pictures or anything—was a little blue book called *Into Outer Space*, by Patrick Moore. This book was inspirational because it was about a boy and a girl who visit their

eccentric uncle—obviously modeled on Patrick Moore—who has a telescope. I can still remember the chapters where the eccentric uncle would say, "Well, we're going to get up very early tomorrow at four in the morning," and the children would go, "Why do we have to get up at four in the morning?" "So that we can see Mercury." "Oh, okay." I remember thinking, "This is amazing." It was just an adventure to see things beyond Earth. As soon as I read that book, I just started devouring everything I could find on astronomy.

ASPATURIAN: How old were you?

ELLIS: Six.

ASPATURIAN: You were reading by age six. Your parents must have placed a lot of value on education. And I assume this was mostly your mother because your dad wasn't home.

ELLIS: Yes. Education always has been for the Welsh nation a very important thing. So I went through all the astronomy books in the children's library. I got through those in no time.

ASPATURIAN: Were there very many?

ELLIS: No. But then the adult library had, and I kid you not, a whole section on astronomy. I don't know why it was so well stocked. But a junior ticket wouldn't let you take adult books out. There was no limit on the number of books you could look at there—I remember you could pile up lots of books—but you couldn't take them out of the adult library. But there was one librarian—I can picture her now, a sort of short, roundish woman—who was so stupid she didn't know the difference between a junior and a senior ticket. So I would sit in the adult library with my books and wait for her shift. I remember that one time she was on the desk as I approached it, but then another woman came and took over and said, "Oh, these are adult books. Sorry, you can't have

them." I'm sure now that if I'd said, "Well, I'm interested in astronomy," they'd have said "Okay." But I had to go back and try again next time.

ASPATURIAN: When you started reading the astronomy books, was your response to go outside at night and look up?

ELLIS: No. I was still trying to find things out by reading about them. It wasn't until I was ten or maybe even eleven that I thought, "Well, I should find these things out myself as well and get a telescope."

ASPATURIAN: I found online the interview you gave to a BBC Wales journalist several years ago. One of the things you talked about there was building your own telescope and your experiences when you first looked through it.

ELLIS: Right. I must have been about twelve. The story there is that the father of one of my friends had a 4-inch mirror. And he very generously gave it to me and said it needed a bit of attention and helped me sort of shape it up. Then I had to find a tube in which to put a flat mirror to make a Newtonian focus. One of my older cousins by marriage, Bob, said, "Oh, go to a carpet factory and ask for the tubes that they wrap the carpets 'round," and by a miracle, this cardboard tube was exactly four inches in diameter. The mirror had little attachments to it that you could screw in. So I screwed the mirror onto the end of this cardboard tube, and then this uncle said, "What you've got to do is paint this tube with yacht varnish." I went round to all the shops in Colwyn Bay—and there aren't many, believe me—and I said, "Do you have any yacht varnish?" But everybody said, "No, we don't have yacht varnish." I remember being terribly disappointed. Eventually, I thought, "This is ridiculous; why do I need yacht varnish?" And, of course, it didn't have to be yacht varnish; it just had to be varnish. So eventually I painted it with varnish and set it up. I can remember trying to get it in focus, and when I did, the first thing I looked at was Jupiter and the moons of Jupiter.

ASPATURIAN: Like Galileo?

ELLIS: Yes. And that was just great. I mean I had achieved it myself. The other thing that I think is great about a telescope is that you can see the colors of the stars. If you've got very good eyesight, you can determine that the stars have colors with the naked eye, but it's much more evident when you observe a rich field of stars through a telescope. My father, because he was a sailor, had a fabulous pair of binoculars, and I used the eyepiece, which was a very nice wide-field eyepiece. I managed to put that together with the mirror, and the combination worked really well. But, you know, it's Wales, and it's raining a lot of the time.

ASPATURIAN: The visibility was not great.

ELLIS: No, and I had it in our back garden. I didn't take it into the countryside or anything like that. The logistics were just too difficult.

ASPATURIAN: Did your parents and friends come and look through it as well?

ELLIS: No. It's incredible. Nowadays, you know, mum and dad would be there, but there was nobody. Maybe my father came out a couple of times. But basically I was out there on my own in the cold weather. I survived despite no support, really. I did it all myself. Then one day, years later, I came back from university, and my mother had junked it. She didn't even ask. It just got in the way, and out it went.

ASPATURIAN: Something else you mentioned in this interview was that many years later you met Patrick Moore and told him—

ELLIS: —About the little book. That is worth mentioning. Patrick had a television program called *Sky at Night*. It started in the 1950s, and it's the longest serving television program anywhere. When I was a boy I used to watch it all the time. When I became a professor, I think I went on the program three times, and the format used to be that you would meet Patrick at the BBC and have a dry run and then do the real thing. It was not broadcast live, but it was all recorded in one go. Afterwards there was a dinner, and at the dinner, I told him that his book *Into Outer Space* had inspired me to become an

astronomer. He said, "Yeah, I remember that little blue book about the boy and the girl going to see their uncle." About two weeks later, he sent me a signed copy, which I think was his copy because "Patrick Moore" had been written in it and crossed out. He wrote, "To Richard, strange to reflect this book set you on your career."

ASPATURIAN: What a lovely story.

ELLIS: And then I reread the book. This was in 1995 or '96, so it would have been about forty years since I first read it, and I found I remembered the chapters, including the chapter about everyone getting up early in the morning to look at Mercury. He was a great guy. He died just a couple of years ago. I've still got the book.

ASPATURIAN: I went to Mauna Kea once in 1988, and I remember there was talk up there about how Patrick Moore had come up to film a special show, and the lack of oxygen had gotten to him, and he nearly passed out. I guess they took him down to the Ellison Onizuka Center, where he revived.

ELLIS: He was a big guy. He wasn't fit at all.

ASPATURIAN: Stepping back, was there any science at your elementary school?

ELLIS: Yes. The upper classes had pretty good teachers. There was a woman in the top class in the junior school—this would have been when I was ten—who knew that I was interested in astronomy. And I remember that when Yuri Gagarin [Soviet cosmonaut, the first astronaut to fly in space –*Ed*.] went into space, she brought it up as a topic and asked me to say something about what this meant. In junior school there were no science lessons. Obviously there was mathematics. But I was always very happy at school. Some people have unfortunate experiences, but I was always happy to learn, and I never felt threatened by teachers or anything like that. It was always a good experience.

ASPATURIAN: Did you have classmates who shared your interest in astronomy?

ELLIS: No, none at all. In the junior school a good friend of mine was interested in science, and there was also a guy who was interested specifically in chemistry. We did have chemistry sets. My mother wasn't very keen on the chemistry set. But, no, there was nobody to talk to about astronomy.

ASPATURIAN: You mentioned Gagarin. Was there any interest for you in becoming an astronaut?

ELLIS: No, no. I think by the time I was ten the interest in astronomy was intellectual; it wasn't about becoming a space man or anything like that.

ASPATURIAN: More about the subject itself. What other kinds of books did you enjoy as a child?

ELLIS: As a child under the age of, say, ten, I would read adventure books mostly. There was a famous British authoress, Enid Blyton, who wrote these books about a group of children called the Famous Five. They would stumble across some crime somewhere in the countryside, and they would eventually sort it out themselves and then call the police in. It was a bit farfetched, really. But I devoured all of those books. When I got to be a teenager, then I started expanding. I remember that as a fourteen-year-old I wanted to be an intellectual so I started reading Hermann Hesse and Camus.

ASPATURIAN: Sartre?

ELLIS: Yeah, Sartre. I read *Nausea*. I don't think I understood it at all. Russian authors—Dostoevsky and stuff like that. So I remember in the summers—and this is probably like fifteen, sixteen—sitting in the garden, being an important person, trying to understand all this stuff. But I don't think there was any obvious theme. I tried to read Welsh literature in the original Welsh, but it was very hard going. As I got older and played with English children, it got even harder.

ASPATURIAN: You were taught Shakespeare and the classic English authors in school?

ELLIS: We did. In school, we did George Orwell. An English teacher was very good, and she did point us to *1984* and Camus and a few other books. I remember reading *1984* and thinking it was really amazing. I remember reading *Keep the Aspidistra Flying*. I went through all of Orwell's books. I tried James Joyce—that was hard going. Oh, and as a teenager I read the *Lord of the Rings* trilogy—one of my school friends told me about it when I was probably fourteen. I thought it was amazing. Do you know the Mervyn Peake trilogy, *Gormenghast*?

ASPATURIAN: No, I do not.

ELLIS: Mervyn Peake was a British author and writer, and this was an amazing trilogy of books. In some ways they are much more thought-provoking than *Lord of the Rings*. *Lord of the Rings* is like a big adventure with a whole system of mythology surrounding it. *Gormenghast* is about a fictional castle with a lot of ritual and adventure and oppression and people who are not very sane or stable. But it's beautifully written. I can't remember how I discovered them, but eventually they became cult books, too. I think if you look at it all, the great classics I tried and didn't get very far with, although I did read many of them. But it was more the adventure literature.

ASPATURIAN: With kind of an epic spin to it?

ELLIS: Yes. Then, as a university undergraduate I remember reading a lot more, because suddenly I was surrounded by students, and I had a lot more spare time. I remember going to Camden Library in London and going through lots of interesting novels. There was an Irish author who used to work for the *Dublin Times*, Flann O'Brien, who wrote a series of really weird books about fictional characters. This became a cult.

ASPATURIAN: So, returning to your early school years, you passed the Eleven Plus?

ELLIS: That's right. In those days, there were two streams of high school. There was what was called the grammar school, which was the advanced level, and then there was what was called the secondary modern, where you went if you didn't pass the Eleven

Plus. I remember sitting the Eleven Plus, which I think was the first time I took a multiple-choice exam. We did a dry run because this was quite a new experiment, a novelty. You had to tick and fill in and circle answers. My sister had gone through a much more traditional exam. So anyway, we both passed this. Now there was a choice of which secondary school to go to. The default would be to go to Colwyn Bay, which was literally half a mile away. But, no, my mother insisted I go to Abergele Grammar School, six miles away, where my sister was, because it was a more Welsh-speaking grammar school. So every day I had to do a half-hour bus ride, and we had to pay. I had to leave the house at eight o'clock, get on a bus at half-past-eight, get to Abergele by nine o'clock. Fortunately there were several boys from Old Colwyn whose parents also thought the quality of Abergele was much better than Colwyn Bay. It was a smaller school, and my sister had been very happy there, so that's where I went.

ASPATURIAN: Was the instruction in Welsh?

ELLIS: No. There were then and are now junior schools and secondary schools in some parts of Wales where the language of instruction is Welsh, but not where I was. But it was a good school, and the science teachers were very good, especially the physics teacher, who was named Williams and who was very impressive. That's where I realized that there was more to astronomy than the Patrick Moore programs and planets and stuff. I would have been twelve, thirteen, when I started learning physics.

ASPATURIAN: They taught it to you that young?

ELLIS: Yes, we did physics, biology, and chemistry as separate subjects from age eleven. And chemistry and physics greatly impressed me. Physics, particularly.

ASPATURIAN: What did you like about it?

ELLIS: I thought it was just great to have all these definitions like force, work, area, pressure. You use these words in common language, but here was a rigorous way of defining what these things are. Suddenly I realized what these things meant. Pressure.

Force. Energy. Power. And you could do these calculations. You could work something out. What would happen if you drop this? Or if you push that? The predictive nature of it impressed me enormously. The homeworks were not a chore; they were a pleasure. It was like finding out the world. It was just great. By now my father was at home. But pretty soon we were doing stuff that was beyond what he could do. If I got stuck, he'd look at my homework and say, "This is beyond me." And yet he had had to do all kinds of calculations, like, for instance, when cargo was loaded in the ship, what was a good balance of dense objects and light objects. He knew a little bit about astronomy because he had to learn navigation. He understood spherical geometry and how to calculate the positions of objects on the celestial sphere.

ASPATURIAN: How about your other subjects in school? Did you like history, English?

ELLIS: Yes, I did. So this is moving on a bit; we did parsing.

ASPATURIAN: Oh, yes, diagramming.

ELLIS: Of English sentences. I loved it. It's unfortunate—I've forgotten how to do it now. We did *précis* with a fabulous teacher who taught us a class called "Use of English." He would give us a newspaper article from *The Guardian* or something and he'd say, "I want you to do a *précis* of this, make it one third the length." That has come in so handy. I've seen so many times in research—

ASPATURIAN: —People who can't get to the point?

ELLIS: People who can't get to the point. So we did this every week. We'd take something and make it more succinct. That was such useful training. Biology was too qualitative for me, but we had great fun. The biology teacher was a woman who was not very controlling of the class. We used to have great fun when we got to sexual reproduction and all this kind of stuff. It was enjoyable, but it was not useful.

ASPATURIAN: Not for you?

ELLIS: Right. Chemistry was great. Welsh was terrible, because I realized that by now my Welsh was not up to the standard. So let me put this in context. The catchment area for the school included really remote mountain areas.

ASPATURIAN: Catchment? The district?

ELLIS: Yes, sorry. The district from which the pupils were coming included remote hill villages where everyone spoke Welsh. So these children were in my class, and their Welsh was impeccable. Whereas I was already basically speaking English with all my friends, and my Welsh was slowly evaporating. When I was a teenager it wasn't really a big deal, but now it's terrible. So I didn't enjoy Welsh because I was struggling. It was hard to follow it, especially literature if you can imagine, because Welsh language and Welsh literature were taught separately. English was okay; we had an innovative English teacher. We did Shakespeare of course. Music was great. I like music. The one thing I didn't take to was physical education. I mean I tried to get out of it. We hated it.

ASPATURIAN: Not a favorite with a lot of people. Were you still speaking Welsh at home, or not so much?

ELLIS: Not so much. It's difficult to say when the transition occurred. It was probably when I was fourteen, fifteen; the conversations and discussions were more complicated, and English provided a better medium, I suppose, so I just switched to English. I wasn't told off or anything for speaking English at home.

ASPATURIAN: Did your life change much when your dad came home permanently?

ELLIS: We were poorer.

ASPATURIAN: Did he get a pension?

ELLIS: There's a story here. It was not a happy time for my mother, or him, probably. He was very frustrated. I was only eleven so I didn't really understand it at all. But I

spoke to my sister about this later, and she reminded me that we used to go on these long walks where he wouldn't say anything. So he was depressed, I think. His life was the sea, and now he was stuck in this house with this woman, his wife, but he didn't know what he was going to do. Suddenly, our standard of living plummeted. I can remember I bought a Rolling Stones LP, and I remember my mother shouting, how could I possibly be wasting money. It was eleven shillings, I think, and I saved up for it. But she was very annoyed that she was scrimping and saving, and I was spending. But eventually things settled down. He did get a pension, a very generous one, from Lord Howard de Walden in person. That continued for many, many years, which made a big difference for him. My parents were a very happy couple. There was never any tension between them, because he was so accommodating to her. She was very determined. She had her way. She insisted that they stay in that house and that he find a job locally. So he did.

ASPATURIAN: You mentioned the Stones. You grew up at a time of fairly major transition in Britain. The 1950s, postwar years, the national economy still struggling to come back, and then it morphed into "the Sixties."

ELLIS: It was a fabulous time to be a teenager. I saw it all.

ASPATURIAN: It reached Wales as well?

ELLIS: Oh, yeah, and so we decided to form a rock and roll group.

ASPATURIAN: In high school?

ELLIS: Yes. The leader of the group was one of my friends. He was a pretty forceful guy, and he said, "We've got to go to Liverpool and buy you a drum kit." I had a post office savings account, and I went to the post office—I must have been thirteen or fourteen—and took all of my money out of it. We got on the train and went to Liverpool and bought a drum kit, and I brought it home. And I just announced to my mother that I had done it. She was livid. Firstly, I'd taken all of my money out of the savings account,

and now I had this drum kit. But she went along with it. We had a great time; we formed a group; we played in school.

ASPATURIAN: How did you became a drummer? Was it an admiration for Keith Moon [the drummer for The Who], or did you have an innate feel for rhythm?

ELLIS: Oh, it was before The Who. This was the time of the early Beatles. I think it was by elimination. They wanted a singer, and the other two guys in the group were a bass guitarist and a lead guitarist. I started out as the drummer, and then I became the organist. And that was much better. I learned drums and I was pretty good at it, but I far preferred playing the piano and organ to playing the drums. Then when I was around fifteen or sixteen, we formed a group called Shades of Blue, and I was the pianist in that. I got quite good at it. I picked it up by ear.

ASPATURIAN: You did not have lessons?

Ellis: I had lessons as a child, but they were hopeless. I hated it when everybody else was out playing on the beach, and I would have to go with my little bag to my lessons. When I was a teenager, my mother got me back again, and I had some more piano lessons, but basically I learned playing the piano entirely by ear. I've lost it unfortunately. One thing I want to recover when I retire is playing the piano again.

ASPATURIAN: What was the original name of your group?

ELLIS: There were several; it morphed. It's like all of these groups; people move from one group to another, and they all change their names. There was never one name. Eventually it became the Omegas, which I thought was rather nice.

ASPATURIAN: Were you all physics students?

ELLIS: No we weren't. Nick was interested in chemistry, and Ian Ward was a mathematician.

ASPATURIAN: Ah, that explains it.

ELLIS: We were all in science. And I was playing the drums and singing. We eventually got a gig in town.

ASPATURIAN: At the age of thirteen?

ELLIS: No, this would have been like fifteen.

ASPATURIAN: Oh, you were much more mature then.

ELLIS: Fifteen or sixteen, playing in the village hall. And there I was with drums, and I had a little jacket, I remember.

ASPATURIAN: Did you write your own music?

ELLIS: We tried to. We didn't do very well, I don't think. Mostly played Chuck Berry and stuff like that.

ASPATURIAN: Did you all grow Beatles haircuts?

ELLIS: This was a time when growing your hair was essential to be hip, and this got you into great trouble in school. I was sent home once for having long hair. You got very good at concealing it—I used to roll my shirt down, so that it looked as if the hair was shorter than it really was, and you walked past the headmaster with your head bent and your shirt down. Because the rule was your hair must not touch the collar.

ASPATURIAN: I see.

ELLIS: We had all kinds of ways for getting around this. Yeah, you're right, this was a fabulous time. The music, the excitement. There were Mods and Rockers. There was The Who. Even in North Wales, everybody was caught up by it. And if you went to the big cities, of course, there was a lot of action.

ASPATURIAN: Do you remember when you first heard about the Beatles?

ELLIS: Yeah, I can remember, this would have been '63, I would have been thirteen. I remember "Love Me Do" ["Love Me Do" was the first single released by the Beatles as a group in Britain. *–Ed.*], and all the excitement. I remember they came through North Wales and played in Llandudno or Bangor, and I heard that they drove through and stopped at our local fish and chip shop. And everybody said, "Do you realize the Beatles had fish and chips at such-and-such a place?" I remember thinking, "Oh, that's amazing." It was a great time. It was impossible not to be interested in music as a teenager. The Top 20—young kids today, do they take an interest in the Top 20?

ASPATURIAN: I think the iPod and iTunes have basically put an end to all of that.

ELLIS: The question was, what was No. 1? On Sunday night, there was a radio station called Radio Luxembourg, and at ten o'clock it would run through the Top 20. We had one transistor radio, and my sister and I would fight over it so we could listen to the Top 20. Sometimes she would storm into my bedroom while I was listening to it and grab the radio. At other times when she didn't do that, I would fall asleep before they got to No. 1 because it was always done in reverse order.

ASPATURIAN: While we're talking about popular culture, I would like to get it into the record that you recently found out you grew up not far from Terry Jones of Monty Python [Flying Circus].

ELLIS: Terry Jones was born in 1942, so he's a quite a bit older than I am. But for the first four years he lived on the same street as me, and I know exactly where he lived—I know the house. We never met until quite recently when we had a dinner together in London about a month ago and found we have a lot in common. Near where I grew up there's a little wooded area with a river running through it that we called the Fairy Glen. I used to ride my bike through it and do all kinds of stunts on my bike there. Terry Jones moved before he got to that age, but he knew the Fairy Glen really well and used to go

for walks there with his mother. So we had a lot in common and had interesting stories about growing up in Wales.

ASPATURIAN: Speaking of bike riding, were you one of these kids who had an opportunity to independently explore and go off with your friends?

ELLIS: Yes, it was a great time. Everybody had a bicycle, and my mother was very kind. During the summer holidays—there were none of these camps or organized anything—she would just say, "Go out." So at 9 a.m. I would just go out on my bike, and I'd be out all day with my friends. I'd come home for lunch and again in the evening; and then if it was a summer evening, I'd go out again. So we were just getting fresh air in this fabulous seaside town. We would just be exploring; we would be doing stunts in these little parks; and sometimes it was a group of six or seven of us with bikes doing things. Occasionally somebody would fall off a bike. Fabulous fun and total freedom! The roads were not crowded. It was a very nice place to grow up.

ASPATURIAN: So you were a bit of a rebel in school, but it sounds like you were also a very good student.

ELLIS: Yes. In those days there was no continuous assessment, but every year there would be several exams. There'd be a Christmas exam, an Easter exam, and there would be a summer exam. And the students were ranked, starting from the top of the class. I remember my grandfather, the minister, saying, "C'mon, you've got to get to top of the class. Who's top of the class?" And I said, "It's this girl." He said, "C'mon, you've got to beat this girl." He died; and then I did—I did beat this girl. [Laughter] I wasn't always top, but I don't think I ever fell out of the top three. I remember meeting this top girl later—in fact, we met about five years ago. She said she never quite understood chemistry. In a chemical equation, you know, copper sulfate plus something equals something-something. I've forgotten how to do this now, but to balance the equation you need to balance the various components. And she could never figure out how this was done, so she just memorized all the equations. She said, "I'm just not going to learn how

to do it." I remember telling her "That's so stupid. What's the point? Why not spend ten percent of that time trying to figure out how it works?"

ASPATURIAN: What did she end up doing?

ELLIS: She ended up as a teacher. So, I was a good student. I don't know if I wanted to be naughty or a rebel or just wanted adventure, but I used to escape school. A friend of mine had a car. It's hilarious, he had an old Jaguar Mark 7 that he got for five pounds from a scrap yard, and he brought it to school. It was a huge car with leather seats. He would park it in school—it was amazing that he was allowed to park it there—and we would sneak out of school.

ASPATURIAN: You would just cut classes?

ELLIS: Yes, and I would lie down on the front seat while he drove out. It was not as if we would do anything ridiculous; we'd just go to the seaside, have a coffee or whatever, just have fun together. And of course we got caught, and I lied. I remember the headmaster came in and said, "Where were you on Friday afternoon?" And I said, "I went to the dentist," and the class went totally silent. They knew that I hadn't. And the headmaster said, "We can check, you know." And I thought, this is just getting worse and worse. [Laughter] And I just said nothing. In the end, he didn't probe; he didn't say, "Which dentist?"

ASPATURIAN: It must have been tough for the school. You were one of their top students.

ELLIS: The headmaster did call my mother in at one point. This is a bit of a story. This is now in what we called Sixth Form.

ASPATURIAN: Which grade would that be for us?

ELLIS: That would be age sixteen.

ASPATURIAN: Okay, tenth, eleventh grade.

ELLIS: School was not obligatory at that point. You could leave school at the age of sixteen, or you could stay on another two years at what we call an advanced level, the A Levels. So I took nine subject tests in the Ordinary Levels [O Levels], the first exams that everybody takes, did very well, and specialized in physics and mathematics in the Sixth Form. And that was great because suddenly the teachers regard you as—you're not kids anymore; you're intellectuals. They're training you to apply for university, and it seemed ridiculous that we should be contained in school. I felt already mature enough to be an undergraduate.

ASPATURIAN: What I'm also noticing in your history is a taste for thwarting petty authority. I mean with the librarian and the hair length.

ELLIS: True. In the Sixth Form, you could become a prefect. They'd give you a little badge to put on your school uniform blazer, and this enabled you to be a prefect and maintain order with the younger kids. You could tell them to go and do this; you could get them in lines for school dinners and things like this. You had the authority to maintain school rules. I think this is all very petty, but a group of us were asked to be prefects, and we said no because we didn't believe in the school rules—it was the Sixties. I think in the end, the headmaster—I didn't like the headmaster anyway; he was a bit of an idiot, really—called my mother in, and I remember her saying, "Well, I'll go and tell him that you're probably one of the best students he's got." Sure enough, my mother went in there and defended me: "You're telling me all these things about my son, but what about his school work? Aren't you proud of what he's doing?" "Oh, yeah, yeah." "Well, what's the problem then?" So then it came time to go to university.

ASPATURIAN: Had you had any formal astronomy instruction up to this point?

ELLIS: No, none at all. Even in the physics class there was nothing—no astrophysics, not even the structure of the sun. We did optics, but it was all lenses and mirrors.

ASPATURIAN: Newton.

ELLIS: Yes. We did practical [i.e. laboratory] classes. It was not very imaginative, but it was all classical physics. Newton's cooling law, frictional experiments, magnetism, electricity. But we didn't do circuits. Miniature electronics hadn't really hit schools at that time. Transistor wasn't taught in school at all in the '60s.

ASPATURIAN: Were you continuing to read astronomy independently through magazines?

ELLIS: Oh, yes. I also joined the British Astronomical Association, which is an association for amateur astronomers, when I was about fourteen. But I was so far from everybody. In North Wales there was no society I could join. I was interested in chess, so I joined the local chess society. That was good fun.

ASPATURIAN: Were you any good?

ELLIS: Pretty good. I was not too bad. What else did I do? Of course, I was interested in girls at fourteen, fifteen. And we had this music group. Starting when I was fifteen, I also fell in love with hitchhiking. So I started deciding to explore Britain on my own, sleeping out rough. I'd get up at six, unannounced to my parents, have breakfast, and leave a note on the kitchen table saying, "Gone out for the day. Richard." I'd go down to the main road and then I'd hitchhike and see how far I got. It was trivial to get to Liverpool; that was about an hour away. Manchester was not too far. Then I became more ambitious, and of course I had to get a backpack, a sleeping bag. So then I'd go to Scotland. Then I became interested in nuclear disarmament, so I hitchhiked down to London to join the protest marches. I was like sixteen now, so I got pretty good at hitchhiking and I met lots of interesting people. You walk out of a town like Aberdeen at ten o'clock at night with no idea where you're going to sleep and then find a field, and in the morning there'd be all these cows around you. [Laughter]

ASPATURIAN: Did you go alone?

ELLIS: Occasionally there were two of us. We tried to break the hitchhiking record from John o' Groats to Land's End, the two of us. We got to John o' Groats [a village on the northeastern tip of Scotland, colloquially known as "the start of Great Britain." *–Ed.*], and we got as far as Bristol and gave up. And I went to London, and he went back home.

ASPATURIAN: So hitchhiking was safe?

ELLIS: Oh, yes, and fabulous adventure. And when I met [my wife] Barbara, I said, "C'mon, let's do it." So probably until our first child, Hilary, was born we hitchhiked around Europe. We were more organized then. We had a tent, you know.

ASPATURIAN: A little more mature. So you took your A Levels?

ELLIS: I took my A Levels in physics, pure mathematics, and applied mathematics.

ASPATURIAN: I saw where you noted that your marks were a little disparate.

ELLIS: Yeah, yeah. Very strange. I thought I was really good at pure mathematics, and the exam paper was extremely easy. So I was confident that I got the top mark. And so was my classmate, who was even better at pure maths than I was. We both got grade C. But it was just enough, fortunately, because I got an A in physics and an A in applied maths.

ASPATURIAN: Had something gone wrong on the grading with the third paper?

ELLIS: Well, the mathematics teacher complained to the examinations board for both of us, and it didn't lead anywhere. But fortunately it was above what I needed to get to London University.

ASPATURIAN: And how did you decide where you wanted to go?

ELLIS: Okay, so in Britain, students apply to university using a clearing house called UCCA [University Central Council on Admissions]. You filled in a form, and you ranked your university preferences. You could choose six. I only wanted to do astronomy. There was no question.

ASPATURIAN: Was that unusual—to want a career in astronomy?

ELLIS: Yes. The headmaster was very puzzled and tried to talk me out of it. He said that he knew somebody who had gone into astronomy and was now working at the Royal Observatory in Greenwich, and it was a dead-end job. I said, "Okay, well, it's what I want to do." I did think that this was a risk, to specialize so early. I could have done a physics degree. Nowadays I recommend to people that that's what they should do—but I was just fascinated by astronomy, and I never wanted to do anything else. And my parents never pushed me out of it. My mother was disappointed that I was leaving home; she wanted me to be a medical doctor in the area or a banker—safe job. But she could see that this was my passion, and my father was keen. He could see I was interested in it, so that's what I did. There were only four universities that offered a degree in it. There was St. Andrews and Glasgow, both in Scotland, and two in London—University College London and Queen Elizabeth College London. I didn't want to go to Glasgow, so I put UC London top, Queen Elizabeth College second, and St. Andrews third. I was invited to interview at University College and Queen Elizabeth College on the same day. I remember that very well. I got the train from Colwyn Bay—my mother made me sandwiches—and I went to London and was interviewed by the professor of astronomy at University College.

ASPATURIAN: There was only one professor of astronomy?

ELLIS: Yes. His name was C. W. [Clabon Walter] Allen. He's very famous. He's dead now, of course, but he was a solar astronomer, an Australian, and he held a named chair at London called the Perren Professorship. He wrote this very famous book called *Astrophysical Quantities*, which is like a compendium of everything an astronomer or astrophysicist might want to know and which is still published; it's now in its fourth

edition. He interviewed me at the University of London Observatory, in a place called Mill Hill. I'm seventeen, and here is a real observatory, and this is the first professional astronomer I've ever met. He interviews me, and he asks me very simple questions. He asked me to do an integral. He was very gentle. And I get an offer. Then I got to Queen Elizabeth College, and I'm interviewed by a mathematician. I didn't go to St. Andrews—they just offered me a position straight away, without an interview. So I decided to accept University College. I remember going back to my school and saying, "Is University College London a good place?" And they said, "Oh, yeah, one of the best schools in the country."

ASPATURIAN: You didn't consider Oxford or Cambridge because they didn't have the programs?

ELLIS: That's right. One of my teachers said, "Why not have a go at Oxford or Cambridge?" One of the guys in our music group did apply; and then you had to sit a special exam, and he didn't get in. I was amazed because he was a bright guy. But, no, I didn't want to go because they didn't do astronomy. So that was the end of that. I was just over the moon. By now I really liked London. I'd been there; I'd hitchhiked there. It was the opposite of where I'd grown up. It was this huge city, and so much was happening there. So going to London was the right decision. I don't regret it.

ASPATURIAN: University College had a department of astronomy that consisted of one person?

ELLIS: No. A professor in England at that time was the senior person in a department, a titled professor, and then there were lecturers and readers who were called doctor. The department was quite small, maybe six or seven academics, but just the one professor. He held an endowed chair, but he was close to retirement. I was in his final class. Many years later they made me a fellow of UCL, so I go back there quite a lot.

ASPATURIAN: Compared to astronomy and astrophysics in the U.S. at that time, what was Britain doing? I'm trying to get a sense of the community.

ELLIS: Britain's strength in astronomy—now we're in the late 1960s—was in theory, cosmology. There was Fred Hoyle at Cambridge, and the [Geoffrey and Margaret] Burbidges, and the collaboration here at Caltech with Willy Fowler [William A. Fowler, Institute Professor of Physics, Emeritus; 1983 Nobel laureate in physics; d. 1995]. So there's cosmology, nucleosynthesis, problems of the elements and how they're formed. Solar astronomy was very strong in Britain. C. W. Allen had been a solar astronomer at Oxford. [Donald] Blackwell, who was eventually to be my PhD supervisor, was a solar astronomer. But there were no big telescopes, none at all. This was, compared to California, hopeless. This was a big postwar problem in Britain—how to get into optical astronomy? There was the Anglo-Australian Telescope [AAT], a collaboration that began in the mid-1960s between Britain and Australia to build a 4-meter telescope in Australia, comparable to the 200-inch telescope at Palomar in size and computer controlled for the first time. I remember Allen telling me as an undergraduate that this telescope was under construction. It was eventually opened by Prince Charles.

ASPATURIAN: Of course.

ELLIS: Of course. In 1974. So while I was an undergraduate, this was on the horizon. I didn't really think much about it. Later it became my bread-and-butter telescope, probably where I built my career. What else? Britain had just finished building the Isaac Newton Telescope. This was a 2.5-meter telescope, not as powerful as the Anglo-Australian Telescope would be, but it was finished in 1967. But you know where they put it? The Pevensey Marshes on the south coast of England.

ASPATURIAN: It wouldn't seem like a very good place.

ELLIS: Well, it was the sunniest place in Britain, but every night the fog rolled in from the English Channel. So the number of clear nights was pitifully small, and in the end, they physically moved the telescope to the Canary Islands. They actually took it brick by brick, or whatever the phrase is for a telescope, and reestablished it in the Canary Islands. It was so funny.

ASPATURIAN: Can you talk about the atmosphere that you found at the University of London in the late 1960s?

ELLIS: Well, of course it was just great to leave home and be an undergraduate, and I got into a hall of residence, which is like a student dorm, in Camden Town. Here's a big city; there are buses, and the underground; there are student societies; there's all these girls; and there's subjects to get interested in; there's society. It was fabulous! Absolutely amazing time! And you had so much free time. I mean, you never find that kind of time ever again in your life as when you're an undergraduate. Then there were all these great rock groups every Saturday night, which a lot of these universities could afford. I went to see Pink Floyd for five shillings.

ASPATURIAN: Did you get involved in the left-wing politics at all?

ELLIS: Yes.

ASPATURIAN: The splinter groups splintered from the other splinter groups?

ELLIS: You got caught up in politics because you couldn't ignore the fact that elsewhere in the world students were asserting themselves. So the LSE was on strike, and I remember that I thought this was exciting. I don't think I ever asked myself, Why were they on strike? Then there was the student union. We were all members of that. We'd go to the student union, and some guy would stand up and say, "Do you realize that the university is keeping records on us?" Well, viewed now, of course they were. They were responsible for us. If something happened, and a parent rang up and said, "What's happened to my little Johnny?" the university would have to look at a file. Well, the idea that somebody had a file on *me* when I didn't know what was in it was abhorrent, so we all marched. [Laughter]

ASPATURIAN: This was during your first year?

ELLIS: Yes. We marched to the central University of London building, shouting and chanting, and we broke in and occupied the building; and then one guy's spectacles got smashed so we got even angrier. It now seems totally out of proportion to the provocation, but we were only nineteen. What can you expect? But academically, it was satisfying. The course was good. We had practical classes with telescopes—not big, big telescopes but 50 centimeters. It was great to start using them. I started looking at the sky and making measurements of proper motions of asteroids and things like that.

ASPATURIAN: How did this compare to the telescope you built when you were younger?

ELLIS: My telescope at home didn't track. It was a 4-inch telescope, not sophisticated. So this was much more rigorous. Physics I didn't like so much. The practical classes were not creative. You went in and took readings and afterward you figured out what it was all about.

ASPATURIAN: Did you learn any quantum mechanics?

ELLIS: Oh, yes.

ASPATURIAN: Did you enjoy that?

ELLIS: Yes. I also liked atomic physics. I loved very much the idea of all these energy levels.

ASPATURIAN: Which comes in handy with spectroscopy for astronomy.

ELLIS: That's right. In the first year, you had to do a heavy amount of physics and math. Thermodynamics I found tough. The teacher wasn't very good. Pure mathematics I found very difficult. University mathematics is at a very much higher level than at secondary school. So there were all these concepts—whether series converge and stuff like that. I found that very difficult to get my head around.

ASPATURIAN: I have a note here from your [Royal Society] memoir where you say, "I was surrounded by physicists of apparently greater intellect and lost confidence. Slowly I recovered." I wanted to ask you about that.

ELLIS: Okay. So when you've come from a school where you were obviously in the top set, you're feeling very confident. And then you go to university, and you meet a lot of people just like you. I think there were only six of us doing astronomy, and I could see that I was in the top two of that small group of students. I was on top of the homework and I was managing, and I was still interested so that was fine. But then we'd go to these enormous physics and maths classes, which would have people working toward their physics or chemistry degrees. In that large a group, I was lost. I would say that physics was not so bad, but in mathematics, particularly pure maths, I thought, "Gee, this is really tough stuff." It was clear that there were mathematicians who loved it and were lapping it up. So first year was tough. I remember going home at Christmas and telling my mother that I didn't think I was going to cut it. She thought this was ridiculous: "How could you make your mind up so soon?" I said, "I'm not sure; it's really tough, Mom. I'm good at this, but it's much harder than you know." But at the end of the first year, I got very good grades in everything, and I remember the tutor calling me in, saying, "Your first-year performance is equivalent to a first-class degree. So keep it up." [Undergraduate degrees in the UK are graded first, second, and third, and the second class degree is split into the upper second, and lower second. *–Ed*.]

ASPATURIAN: Were you startled to hear this?

ELLIS: I was.

ASPATURIAN: Those aren't easy to come by, those first-class degrees.

ELLIS: When I think back, although I can get concerned about things, often when the crunch comes—an exam or a crisis or some focal point—then I rally 'round and succeed. Even now in my career, I can think, "God this is going to be really tough. I'm not sure I'm going to cut this." But in the end, somehow it comes together.

ASPATURIAN: What were you studying in your astronomy classes besides the initial observations? How early did you know that what you wanted to do was observational astronomy?

ELLIS: Oh, I think right at the beginning. I remember my very first practical class, where I wrote up the experiment the way I'd been taught by my sixth-form physics teacher, Williams, who was a fabulous guy. He was very clear and strict about how an experiment should be written up—the apparatus that you used, the purpose of the experiment, the procedure, the data, the analysis, the conclusions. Much like an *ApJ* [*Astrophysical Journal*] paper, you know? So when I went to university and did my practical class and wrote it up exactly as I had done in school, the lecturer for the class called me aside and said, "You've written this up so well." I said, "Well, that's the way we were taught in school." He said, "That's great." So I realized that my sixth-form training had been unique. I was very lucky. And applying that to doing an observation—how do you summarize the result for somebody else to see?—was really great. And it's just great fun using a telescope. So I realized this was something I wanted to do. But there was no research yet. The first two years, there's just so much course work. It wasn't until the third year that we were allowed to have a project of our own.

ASPATURIAN: You were at the top of the class. Did any of the physics faculty try to talk you into bailing from astronomy and moving into physics?

ELLIS: No, nobody did; and I think migration from one to the other was not so common as it is in universities now. It's so funny when you see students here at Caltech. They're constantly coming to me—the ones that I'm responsible for—and asking, "What do you think I should do? Should I do this or that?" We didn't have any of that choice. This was the syllabus; these were the courses; there was no "Should I do this or that?" There was no choice at all. The only choice we ever got was in the final year, when we could choose a few courses and had a project of our own. And you must remember, the physics teachers were confronted by these enormous classes, and I was just one of six astronomers, and they didn't know me personally at all.

ASPATURIAN: Did you have any professors who stand out as particularly memorable? You mention this Professor Allen; were there any others?

ELLIS: Yeah. The guy who taught atomic spectroscopy was a guy called Bill [William B.] Somerville, who still comes into work. Very nice guy, a Scotsman. He was very rigorous, but a sympathetic guy, soft-spoken. He was very helpful to me in many ways, and I remember thinking, "This is somebody who really understands his subject," and I paid a lot of attention to him. Allen, the professor, was very eccentric, a weird guy. The guy who was like the tutor to the class was called Derek McNally, and he started out as rather a formidable guy. He taught us a subject called spherical trigonometry, where you're presented with the celestial sphere and asked how do you work out where things are. Precession, mutation, how do you predict an eclipse? I've done all this. It's so tedious. He taught this class in the first year, and he taught it brutally to sort of frighten everybody with the idea that if you didn't get through the course, then you didn't have it in you. So I was trying very hard to please him, and I did well in that course in the end. In my third year, he was very helpful in advising me where to go for my PhD.

ASPATURIAN: What did you do in your third year when you had some options?

ELLIS: I can't remember how on earth I chose it, but I decided to look at absorption lines in the spectra of quasars. It was about as far as possible from anything I'd ever been taught, and how I came across it I don't know, but it inspired me to do research and I did a project all on my own. I got no help from anybody; I just devoured the literature in the library. I went through all of the *Astrophysical Journal* papers—a large fraction of them from Caltech.

ASPATURIAN: Of course. [Maarten] Schmidt [Moseley Professor of Astronomy, Emeritus], Wal [Wallace L.W.] Sargent [Bowen Professor of Astronomy, Emeritus, d. 2012].

ELLIS: Schmidt, Sargent. [John N.] Bahcall. Jesse Greenstein [DuBridge Professor of Astrophysics, Emeritus, d. 2002]. Artie [Arthur M.] Wolf. And others. The big debate

was, are the absorption lines in the quasar itself or are they from material along the line of sight to Earth? So there were these two theories: intervening or intrinsic. The quasar itself has a redshift, which is the redshift of the emission line. And then there were these myriads of absorption lines, all at different redshifts. Today we know that they're formed in intervening gas clouds along the line of sight. This gives you unique insight into the intergalactic medium, which is now a big subject.

ASPATURIAN: Yes. I remember talking to Wal Sargent about this years ago.

ELLIS: So this was just fabulous. This opened up research to me, and I discovered this topic myself. You had to give a presentation as part of this course.

ASPATURIAN: Did you have an advisor while you were doing this project?

ELLIS: No. We just went and chose a topic. I think there must have been a lecturer in charge of that particular topic. I told him the title, and he just said okay.

ASPATURIAN: Was there any emphasis on extragalactic astronomy?

ELLIS: No. I was the only one doing extragalactic. Let's think what the other students chose. One chose the helium problem—whether the cosmic helium abundances are consistent with our understanding of the Big Bang. Another guy was doing radio astronomy studies of molecular clouds in the Milky Way. We each had to give a talk about our topic.

ASPATURIAN: I am struck by the fact that you and these classmates chose what turned out to be very important problems.

ELLIS: Yes. It was a good atmosphere in the class, but we didn't see the bigger world of research as undergraduates. Which is a shame, really. There must have been colloquia, but I don't think I ever went to one as an undergraduate. The Royal Astronomical

Society [RAS] meetings were in London, and we could have gone, but we didn't. We were not looked after very well; it was very traditional.

Some material in this session was originally recorded during interview sessions 2 and 5.

RICHARD ELLIS

SESSION 2

January 17, 2014

ASPATURIAN: When we broke off last week, you said you wanted to talk next about meeting [your future wife] Barbara while you were studying in London.

ELLIS: Okay. It was in the second year, 1969. In the first year, we were all doing general courses in physics, mathematics, and astronomy. But then when we got to the second year, we started a course called Astrophysics and went into real detail about the processes in stars and between the stars. It was absolutely great, and suddenly there were other students taking the class who came from other universities in London. At that time, the University of London was like a federal structure. There was the University of London, and then there were the colleges. The degree was from the university, which is not the case now; today all of the universities in London are independent entities. But in the 1970s, it was like a federal system, and so there were students from Queen Elizabeth College taking this astrophysics course. And two of them were girls, and one of the girls, Susan, was pretty attractive. There we were. Pretty soon after the term began—this was November—there was a party in London. And you won't believe this, but the residence hall where the party was held was right smack in the center of Oxford Street. Must have been prime real estate—it's now been torn down and replaced by some massive skyscraper. But it was such an incongruous location—like having a hall of residence in Times Square. So I went to that party with a mind to chatting up Susan, but when I got there, there were all these men around her, and it was practically hopeless. But Susan had brought her roommate, Barbara, who was doing biology at Queen Elizabeth College. I just made a beeline for Barbara, and that was it. We hit it off really well—I think Susan was a bit offended. So that began, and now Susan, who married somebody doing a PhD and now lives in Scotland, is a good family friend. Barbara and Susan have kept in close contact.

ASPATURIAN: You and Barbara were both in your second year?

ELLIS: Yes, both exactly at the same stage.

ASPATURIAN: You mentioned this astrophysics course. Was this like contemporary topics?

ELLIS: It was pedagogical; and it was taught by a guy called Foster. He was an interesting guy, and that was one of the two courses that really got me going. The other was atomic physics, which I mentioned last time, which Somerville taught. Then there was a professor in physics, [Alexander] Boksenberg, who taught a thermodynamics course. He later became director of the RGO—Royal Greenwich Observatory—and a great friend. He worked with Wal Sargent and used to come out to Palomar in the late 1970s, early 1980s. So the second year was great fun, and in the third year we had a practical class with telescopes and much more ambitious projects. Allen, the professor whom we've talked about, was in charge of the class that year, and when there was a solar eclipse in February 1971, he said to me "Why don't you take some photographs of the eclipse, and we'll analyze them for a phenomenon called limb darkening." If you look at the surface of the sun, you'll see that it's slightly dimmer at the rim than in the center. And that's because there the light from the deeper layers of the sun is coming from higher up in the atmosphere, so its path length through the atmosphere is less before it gets absorbed. So when you look to the edge you're seeing upper levels of the atmosphere where the temperature is cooler. Whereas when you look straight into the center of the sun, you penetrate farther down where the temperature is hotter. So what this measurement tells you is how the atmosphere in the sun changes its temperature as you go to deeper and deeper layers. I said, "Yeah, that's a great project," and I decided to take the photographs with a large refractor telescope they had. And then at night I took photographs of the Andromeda spiral. Now this telescope, although impressively large in a big dome, is right next to the M1 motorway in the center of London. Every time a truck went along the freeway, the telescope would vibrate.

ASPATURIAN: Not an optimum situation.

ELLIS: Not an optimum situation. But it was just great fun to do practical work with telescopes.

So I had a great time at UCL, feeling *this* is what I want to do. The subject was interesting, research was great. Barbara and I were together. Especially in the summer, London is a fabulous city to be a student. I think those were a happy three years.

ASPATURIAN: I see that you won a couple of major prizes as an undergrad.

ELLIS: Let's think—at the end of the first year, there was the Huggins Prize, which was given to the best astronomy student. Well, there were only six of us, so that wasn't—

ASPATURIAN: Still.

ELLIS: But the Faculty of Science silver medal at the end of my three years was a big surprise. That *is* quite an honor because that was for the student with the highest marks in the faculty of science. It is a genuine silver medal, and I've got it somewhere. You know, they didn't tell me! One day in the summer after I'd left University College, I got a letter saying "Congratulations, here's your medal."

ASPATURIAN: There was no presentation?

ELLIS: No. It was as if they weren't quite sure who got it, and then when they realized, oh, Ellis got it, they said, "We'd better send it to him."

ASPATURIAN: You know, that's quite possible.

ELLIS: It was hilarious. It was the middle of summer, like July; I had a summer job and was working when it arrived. When I got home, my mother said, "This little box has come in the post from London." And I said, "I wonder what it is?" And I opened it, and it was a letter congratulating me, and inside was a lump of silver. [Laughter]

ASPATURIAN: Your parents must have been pleased about your academic success.

ELLIS: My mother was very proud. She was obsessed with the idea that I'd got a first-class honors degree. You know, in Wales, education is a very important measure of pride in children. So she marched to the newspaper and said, "My son's got a first-class degree." Then we'd go to shops like Marks & Spencer, and meet old ladies who would say, "Well done, Richard." So my mother was very pleased. And then as we'll discuss, I went to Oxford, and she thought that was great.

ASPATURIAN: How did you decide where to apply for graduate school?

ELLIS: I knew that astronomy was what I wanted to do, and I knew I wanted to do research, so the question was where to apply. Since I had worked on quasar absorption lines and the only place in Britain where that work was being done was Cambridge, I applied there. I was very disappointed that I didn't get anywhere, even though I had a first-class degree.

ASPATURIAN: Why in the world did they turn you down?

ELLIS: I don't know. I just got a letter, and it was a pretty nasty letter. To do a PhD you applied to a central clearing house in Cambridge that dealt with your application to both a particular college and the Institute of Astronomy. I sent in my forms and got a letter back from this clearing house saying my papers had been forwarded to the Institute of Astronomy, which was not interested in my application, which therefore "necessarily fails." And that was it. But for some reason, I wasn't upset, I just thought, "Oh, yeah, okay." So I applied to Oxford, Edinburgh, Manchester, and Sussex. Sussex took me very seriously, and I remember going down there on the train. The great thing about England was how easily you could visit all these places. No flying around the country. I didn't go to Edinburgh; I can't remember, but I think they may have turned me down. Sussex was very interested, but they didn't take you straight for a PhD. You had to get a Master's first and pass an examination to go on for a PhD. I didn't want to do another degree and have more written exams. I wanted to get on and do research. So Sussex was not attractive. I also went up to Manchester, but they monkeyed around. They interviewed me, and they seemed interested but never seemed to come to a decision.

Oxford made me an offer soon after I went up there. It was in March or April, a beautiful day, and I turned up at Oxford and met the head of the department, Professor Donald Blackwell, a solar astronomer. He was a very charming man, very pleasant, very much like an uncle, and he showed me around the department. When I got back a couple of days later, there was a letter with an offer, saying that as long as I got an upper second degree, that would be fine. But there was no discussion about what I would be doing. This was the mistake I made—neglecting to discuss what topic I would do my research on.

ASPATURIAN: You had simply assumed that because they had done work in this extragalactic area—

ELLIS: Well, I had met various people while I was there, including John Peach, their only extragalactic astronomer. I chatted with him, and he showed me what he was doing, and it never occurred to me to be assertive and say, "Okay, if I come here, what would I do?"

ASPATURIAN: How old were you? Twenty-one, twenty?

ELLIS: Twenty. So I accepted the offer, assuming that I would be doing extragalactic work, but when I got there, Blackwell said it wasn't possible because Peach wasn't taking any students that year. By now, it was October, and Barbara and I were looking for somewhere to live. Blackwell said that he had a vacancy for a doctoral student. So I had no choice. It was a ridiculous situation, and such a lesson for a young person to be so misled.

ASPATURIAN: Did you consider applying elsewhere and moving?

ELLIS: No, I mean, it was too late. It's not as if I was against working with Blackwell, who was regarded as the main observer in the department. These departments were tiny; there were probably five people there who would take students, maybe fewer. There was a very famous theoretical cosmologist arriving the same year as me—a professor, Dennis Sciama.

ASPATURIAN: I know that name.

ELLIS: He had been Martin Rees's supervisor and Stephen Hawking's, and the people at University College London said, "Oxford's going to be a great place. Sciama's moving there, Blackwell is an observer. You'll have no problem doing great projects." So viewed from outside, working with Blackwell didn't seem in any way a mistake because there was going to be this atmosphere of theory and observation. It was only after I arrived that I realized that Blackwell was a rather narrow guy who just wanted to study bright stars and the sun, and that Peach had nothing to do with Sciama and was not taking students.

ASPATURIAN: Even in this tiny department, everything was siloed like that?

ELLIS: Yes. In fact, there was some slight tension between Blackwell and Sciama, which I realized after I'd been there a few months. So it wasn't a very well-functioning department. But, it's amazing how when you're very young, you can turn anything around, even what would seem now a disastrous situation. It's like little kids—they bounce back, they don't get upset about things. So I realized that I had to make the most of it. There was one aspect that Blackwell was interested in working on with me, which was stellar nucleosynthesis. This was an interesting time in the subject because Burbidge, Burbidge, Fowler, and Hoyle in the late 1950s had produced the concept that the nuclei of the chemical elements were produced in stars.

ASPATURIAN: Right, their three seminal papers.

ELLIS: Yes. But in order to test the idea of how heavy elements were produced in stars, you needed accurate observations, and those measurements of the abundances of the nuclei depend rather sensitively on the atomic constants—what we call the oscillator strengths of the various spectrum lines of each element. Blackwell sat me down and said, "They're just very poorly known. My job is to measure them more precisely." I said, "Okay, well, how are we going to do this?" And he said, "Well, I'm building a big furnace. And we're going to do laboratory astrophysics. Obviously that's not very

exciting for a student. You can work on that, but we will also do observations of spectra of bright stars, and you can combine the laboratory astrophysics with the stellar astrophysics." So in the end, working on my thesis, I did a lot of laboratory astrophysics. I learned a lot about nucleosynthesis and radiative transfer, and I learned a lot of computing. I did observations in Switzerland and in Israel. It took three years—it wasn't a sentence for ten years. But Oxford was a strange place. It still is a rather pompous university.

ASPATURIAN: What was the environment there like?

ELLIS: At Oxford I had to choose a college. I chose Corpus Christi, which is a very small college, and they were very friendly to me because they'd never had an astronomer, I don't think. So they were very nice, but a huge fraction of the graduate students there had been undergraduates at Oxford, so they knew how everything worked. There were so many puzzling aspects to Oxford that newcomers had to learn—the terminology, the way the colleges worked, having lunches and dinners. It was all very mysterious because I hadn't gone through it before.

ASPATURIAN: It sounds like it was heavily ritualized.

ELLIS: Yes, it was.

ASPATURIAN: They had their own nomenclature?

ELLIS: They did. To give you an example, you could go to your college for lunch and dinner. And of course that's nice, the food was great, but you didn't pay with money. The idea that these college servants had to deal with money was dangerous. So you bought books of paper money in little chits, like at a fairground. Scrip, you call it in America. You bought these books of scrip—there was one kind for, say, afternoon tea and other kinds for lunch and dinner. So you would have all these different scrips to keep track of. At the end of term, you got a bill, and guess where the bill was sent?

ASPATURIAN: To your parents?

ELLIS: Yes. [Laughter] My father said, "I just got a bill." I said, "Yes, I'm sorry, it goes to you by default." "Well, I'm not paying it," he said. I said, "I'm terribly sorry; I couldn't intervene." It was amusing. But then Barbara and I got married during my second year at Oxford—I was twenty-two—and one thing we decided was to live out. We moved to the countryside, eight miles out of Oxford, and we lived in a little converted Victorian schoolhouse in a very small village called Waterstock. It was just fabulous to live in the country, and that made up for a lot of the anguish about what I was doing at Oxford and whether this was research that I really wanted to do. We had a great life out in the village. We would cycle to the pubs. Eventually we got a car.

ASPATURIAN: I wanted to ask about the general environment in the astronomical community at that time. Things had kind of exploded in the '60s. You had the nucleosynthesis work, you had the cosmic microwave background; and then in the early 1970s [Anthony] Hewish and Jocelyn Bell discovered pulsars in Britain and won the first physics Nobel ever awarded for astronomy.

ELLIS: I was aware of all this. Sciama gave a cosmology course as soon as he arrived at Oxford, and I went to it. That was great. Here is a famous cosmologist in the department, and he was a nice guy. Of course I was doing something else, but I was interested in his subject. We had some very interesting colloquia. Hewish came.

ASPATURIAN: Where was he?

ELLIS: He was at Cambridge. But the atmosphere was parochial. We didn't move around. Once a month the Royal Astronomical Society had a meeting on a Friday, so everybody went to London for it. And then Blackwell became its president [1973–1975]. Pulsars had become a big thing, and I remember talks there on the theories of pulsars. Hoyle was still around and would turn up occasionally. But I was twenty-two, and what struck me was how few young people there were there. The RAS was run very much like a gentleman's club. You'd see all these old people in the front few rows, and the students

would be in the back and never ask questions. It was after I got to Durham [University] that there was real expansion in funding in Britain for astronomy, and the field really began changing. During the 1960s, we were in this situation where there were no optical telescopes. There were radio telescopes: Radio astronomy and theory were the two leading fields. The area that Blackwell was in, solar astronomy, was declining; people were pulling out of it. So you're right, there were a lot of discoveries; but I didn't notice the British expansion until, I would say, the second half of the '70s. That's when suddenly there were lots of young people going to telescopes. The Anglo-Australian Telescope had opened. There was a new generation of observers. That's when things really took off from my point of view.

ASPATURIAN: So technology drove the expansion in many respects?

ELLIS: That's right.

ASPATURIAN: Now in your memoir for the Royal Society, you mention that at some point you became a bit disenchanted. You refer to an incident that happened at the Wise Observatory in Israel.

ELLIS: Yeah, that was terrible. I regret it very much. Blackwell just had no real social skills as a supervisor.

ASPATURIAN: Even though he seemed like such a pleasant guy.

ELLIS: He was shy, I think. Firstly, he'd been propelled into this famous chair called the Savilian Chair, which goes back all the way to sixteen-something [1619] and had had many distinguished holders in the past. But he was not, in my opinion, a leader. His previous students hadn't done that well, and here was I working with him. I think he was just personally very shy. He didn't come down and say, "Richard, I think you should visit Cambridge," or "Richard, here's a conference; I think you should present your work here."

I remember that one day, one of the world experts on nucleosynthesis, Dave [W.

David] Arnett, from Steward Observatory [University of Arizona], was visiting Oxford. And Blackwell's secretary put a little piece of paper on my desk, typed, that said, "Dear Richard, Professor Arnett is visiting from the United States. Professor Blackwell thinks you should meet him." That was it. It was like an instruction for me to go up to this professor from the U.S. and say, "Hello, I'm a graduate student, can I chat to you?" Today if a distinguished professor from Britain was coming to Caltech, I'd invite my group 'round, we'd all go for lunch, I'd introduce everybody, and I'd say, "Why don't you tell Professor X what you're doing for your thesis? Why don't you go to your office and show him what you're doing?" But this was in the early 1970s, and I think that Blackwell was very nervous. He was often not around, and if I had a problem I'd have to make an appointment to see him. There was none of this sort of dropping in. It was very formal. Anyway, we went to the Wise Observatory on two occasions, and the relationship was just absolutely hopeless. In the end, I thought this is just not something I want to do.

ASPATURIAN: Was there something specific that happened there with him?

ELLIS: Well, there were two things about the Wise Observatory. First, it was a very miserable place. It's in the desert.

ASPATURIAN: Is it in the Negev?

ELLIS: Yes, in the Negev desert. Secondly, we were staying there in a sparely furnished apartment, just the two of us, and he wasn't talking. He was in his room all day, and I thought, "This is just getting nowhere. I can't work with this guy, he's hopeless." So I left, which was a ridiculous thing to do, and irresponsible, because we had reserved telescope time. He had to be there. I left him a note, obviously, and I got on a bus to the airport, and I flew home. It was amazing that I managed to get home because in those days, if you turned up at an airport trying to get a ticket, there were no ATMs. Apparently I had just enough cash to get the air ticket—it was 40 pounds, I seem to remember—and I managed to get a British Airways flight to London. This is 1972, so I was in my second year. I got back to Oxford and said, "I'm done with astronomy."

ASPATURIAN: I see.

ELLIS: Barbara was very supportive. So I wrote Blackwell a long letter saying this was not what I had imagined. He of course didn't know where I was and had been worried that I wouldn't make it back or get lost somewhere in Israel. So I immediately sent him this express letter saying that I was safe in England, and everything was fine, and that I was very sorry, but this was it. Anyway, his secretary tried to find me. In the end, I went back to see Blackwell. He was quite distraught, actually; he said he could see that maybe I wasn't cut out for it. But he desperately pleaded with me to finish the thesis because I was halfway through. Then I realized of course that it was a blot on him if I didn't finish because our studentships were funded by the government, and this would look bad on his record.

ASPATURIAN: It sounds like he was having trouble with some of his colleagues and such?

ELLIS: That's right. So in the end I buckled down, and I finished the thesis. I was quite sincere then in thinking that I would leave the subject. In the final year—this is 1974—I made a concerted effort to get a job outside academia, and I realized I was actually marketable. An Oxford PhD counts for something in Britain. The best job offer I nailed was as an advertising executive in London. It looked fabulous. The salary was a magnificent 2,000 pounds a year. I had a series of interviews—there were something like four of them—and in the end I got the offer. I was all set to go, and Barbara was very keen that I become an advertising executive. She thought that we would earn a lot of money and would eventually move back to London. I could so easily have left astronomy at that point, and then something incredible happened. Blackwell, to my amazement, after all we'd been through, came to my office one day and said, "They want an astronomer at the University of Durham." And I said, "Really? Where's Durham?" He said, "It's in the north of England. It's a famous old university town." I said, "Oh, yeah, I've been there once. It's a very nice city." He said the head of the physics department there was a guy called [Arnold Whittaker] Wolfendale—Professor Wolfendale—and he had made it his mission to expand the department into astronomy,

now that astronomy was getting more money. He said, "He wants a young astronomer. Why don't you apply?" I said, "After all we've been through?" He said, "No, why don't you give it a go; you might like it." I applied, and I know for a fact that Blackwell wrote a very good letter and said "Ellis is exactly the man you want." So I got on the train to Durham, and when I got there, the first thing I realized was that we could afford to buy a house. It was so cheap. Houses were like 8,000 pounds. There was an interview, and there were short-listed candidates. It was like a temporary teaching position, senior demonstrator.

ASPATURIAN: Senior demonstrator in physics?

ELLIS: That's right. So there were six people, and they asked me what I was doing for my thesis. I said, "Can I use the blackboard?" and they said, "Oh, yeah," and I started explaining on the blackboard what I was doing, and then Wolfendale called me afterward and said, "We've given you the job, and you know what swung it? You had the confidence to go to the blackboard. All the other candidates just sat in their chairs. You seemed lively. You wanted to tell us what you were doing."

So I turned down all these other offers. I think there were three—IBM, the journal *British Birds*, and advertising executive—and I said to Barbara, "We're going to Durham." She said, "Wow, are you sure you know what you're doing? I thought you didn't like astronomy." I said, "I want to give it another go."

ASPATURIAN: What swung it, do you think?

ELLIS: Being respected by Wolfendale; suddenly having somebody who wanted to support me. I think what had turned me off Oxford was that no matter what I did, nobody took any notice at all, and my research was not what I wanted to do, and Blackwell didn't nurture excitement. Now when I look at my own students, I think, "That's what a professor has to do." Students go through ups and downs, and the role of professor is to support those students and say, "Look, you didn't get this job, but don't give up because the future is very bright." That was a big disappointment at Oxford—that they didn't engender excitement in the subject and provide support. I think that deep down I was

still very enthusiastic about astronomy, and here was an opportunity to reset the clock. I'm glad I did go to Durham.

ASPATURIAN: Earlier when the recorder was off, you mentioned having left your PhD thesis in a car?

ELLIS: Okay, yes. I had accepted the job at Durham, and I was desperate to get the thesis finished before I left in September. So this would have been maybe June or July, and I was well advanced in writing it. There was no word processing in those days; you wrote the thesis out in longhand and gave it to a typist. Of course, it had to be written out neatly so that the typist could do it. I think I did the equations by hand on the final version with a calligraphy pen. At the time we were without a car. We had smashed the car up. Entirely my fault.

ASPATURIAN: "We" being you and Barbara?

ELLIS: Yes. We were married and still living out in this country house. If the bus was there, I would take it, but the bus only ran every half hour or hour, so what I would do is hitchhike until the next bus came along. And, sure enough, most mornings I caught a ride. This time I got a ride from a guy who was working in Oxford, maybe in medicine. I put my thesis, which I had in a folder, on the back seat, and we drove into Oxford, and he asked if it was okay if we parked where he worked, and I said, "Sure, I can walk from there." I walked all the way to my department before I realized that the entire thesis was still in the back of his car. And so I panicked. I had no idea who he was, where he lived; I couldn't even remember what car it was. I walked back to the car park, which was not an insignificant walk—it's like half a mile—and the car was still parked there. What was amazing was that the door was not locked, so I could just open the door to get the thesis. It's really quite incredible. [Laughter] So then I turned up at Durham.

ASPATURIAN: Were you the lone astronomer in a physics department?

ELLIS: Yes. There was a physicist at Durham, Mike Scarrott, who had built his own instrument. He was a solid-state physicist who had been told by Wolfendale to go into astronomy and do something. So they decided to build a polarimeter. Normally when measuring the polarization of stars, you have a camera and you measure the signal from the star with the polarized filter one way and then the other way, and you look at the difference between the measurements and that tells you the polarization. Scarrott had built a two-dimensional polarimeter that took a picture of the sky in one plane of polarization, then rotated it, and took a picture in the other polar degree. It was a very clever instrument: He was a very talented experimental physicist, but he was not an astronomer. He liked taking pictures but had no tools to interpret them. So the idea was that I would work with Scarrott as a real astronomer—I remember Wolfendale saying, "We've got a real astronomer in the department." Here I was, after all the turmoil of Oxford, and it turned out that Scarrott and Wolfendale didn't get on. Wolfendale had his own view of what I should be doing, and Scarrott got upset, and one thing led to another, and in the end, I decided to detach from Scarrott and work primarily as an independent researcher with Wolfendale.

ASPATURIAN: One of the things you say in this memoir is that Scarrott threw you out of his group?

ELLIS: He did. Because, you know, I had ideas too. There are two ways of viewing this—one is, he was a difficult guy. In the end, he became an alcoholic and died. It was a very sad end to Scarrott. The other way to view it is that, basically, I was under instructions from Wolfendale on what to do, and Scarrott hated this and took it out on me. I was caught in between these two figures, and it was a very difficult situation. But I persisted, and I managed to extract myself from Scarrott.

ASPATURIAN: How far along were you?

ELLIS: Oh, only three months. So, in early 1975, I was still in a situation where I was very worried that this wasn't going to work out.

ASPATURIAN: How large a department was this?

ELLIS: It was pretty big—about twenty lecturers, two professors—a theoretical physicist and Wolfendale—and undergraduate classes of a hundred. So I was teaching in the laboratories, which was difficult. Laboratory physics was something I'd done at university in London, but I'd never taught it. So I was learning the ropes pretty slowly. But I will say that within six to nine months, I'd completely recovered because I met a PhD student, David Axon, a bright guy, and the two of us started working on a project of our own design. He was collecting polarization measurements of stars in the Milky Way, and he'd fallen out with Scarrott too, so the two of us became buddies. We started working together, and he was effectively my first student. We wrote a paper—two papers actually—on the polarization of starlight in the Milky Way, and this got a lot of attention. I gave a colloquium on it and was invited to give a talk at the Royal Astronomical Society. This was all in the space of about six months.

ASPATURIAN: What was it that attracted the attention?

ELLIS: We produced the first three-dimensional map of the magnetic field of the Milky Way galaxy. There had been maps of the polarization of starlight sky before, but we only chose stars whose distances were known. The polarization of starlight tells you about the magnetic field in the Milky Way. As polarization is produced along the line of sight by the effect of magnetism on dust grains, it's possible to dissect the signal at different distances to produce a three-dimensional map of the magnetic field of the galaxy. So this was the breakthrough, and our papers got a lot of attention, and I think Wolfendale suddenly realized, "Hey, this guy can do great research." Suddenly I became, within a year, a respected guy in his department. From there, things really took off. That project really rescued me, I think. Otherwise, I'd have probably left.

ASPATURIAN: I looked into this student, David Axon, and I guess he went on to quite a distinguished career. Head of the physics department [School of Mathematical and Physical Sciences] at Sussex, I believe.

ELLIS: Yes, that's right. He did. Very nice guy. He came to my sixtieth birthday party. Unfortunately, he had a heart attack and died very suddenly two years ago.

ASPATURIAN: So with this paper, you established your reputation within the field and within the department?

ELLIS: Yes. Since my job was a teaching position, I also had to start giving classes. They came up to me and said, "We'd like you to teach an undergraduate class on the earth and its atmosphere." I sat down and I prepared a set of notes for that, and that was great fun. I really enjoyed teaching undergraduates. So suddenly, life was great—a combination of my research taking off, the fact that I enjoyed teaching, and Barbara probably was pregnant pretty soon after that. Our first child, Hilary, was born in 1976. We bought a house. Then I began work on a project about the orientation of galaxies—a bit of a diversion, really—that Wolfendale helped me with a little. Today I would regard it as a crazy project, but it was my first venture outside the Milky Way. Durham was cut off from what I would call the mainstream, the big astronomy departments.

ASPATURIAN: I was going to ask about that.

ELLIS: You know, we were isolated, and Wolfendale was a cosmic-ray physicist who wanted to move into astronomy. So there was nobody saying, "Richard, that's a wacky project; I wouldn't waste my time on that." I had quite a lot of freedom, really. After this magnetic field of the Milky Way map, which was a good project, Wolfendale wanted to know whether there was any evidence for an intergalactic magnetic field—basically, where did magnetic fields come from. One of the arguments was that there was an intergalactic magnetic field that was amplified when the galaxy collapses. There were two people who'd worked on this. One was Eugene Parker, who's a very distinguished professor at the University of Chicago, and then there was a less well-known physicist in Australia called [John Hobart] Piddington. And so I read their papers, and one of Piddington's arguments was that if there *was* a uniform intergalactic magnetic field, then galaxies would be aligned. The galaxies' shapes, their major axes, might show some alignment in the sky. So, I thought, "Okay, this sounds fun." I found that an amateur

astronomer called Brown had sat down in his house, copiously measured the angles of thousands of galaxies from the Palomar Sky Survey, and recorded all this in notebooks, like a catalog. I tracked down this old man, who was in his seventies and living in London, and Wolfendale said, "Go and get his notebooks; let's do some statistical tests on them." So I went down to see this guy; he was dying and not in good shape. His wife was very nice, and they put me up. I stayed with them, I got the notebooks, and I entered all their data into the computer. I didn't see any signal that was interesting, so I didn't write it up. This guy Brown had been using big photographic plates taken by the Schmidt Telescope at Palomar Observatory. But the British now had built a telescope—the UK Schmidt—to look at the southern sky,

ASPATURIAN: Where was this based?

ELLIS: In Australia, at Siding Spring Observatory, on the same site as the Anglo-Australian Telescope. It's part of the overall observatory, but it was entirely owned by Britain, and it was operated by the Royal Observatory in Edinburgh. It had been recognized that plates taken at the UK Schmidt would be a treasure trove for looking at the properties of galaxies, and so they took these glass plates, which are 14-inch square with thousands of little, faint galaxies on them, and shipped them to Edinburgh. And Edinburgh had just built a machine called COSMOS, which would scan these plates and measure the positions, brightness, and shapes of all the galaxies on them.

ASPATURIAN: It could correlate from plate to plate?

ELLIS: Yes, it would make a big catalog. I'd already given a talk on this and said these plates were sufficiently deep that you could look a little way back in time. So we could see galactic evolution as well.

ASPATURIAN: How far back?

ELLIS: Maybe they'd look back four billion years, so a third of the way back to the Big Bang. So Wolfendale said, "Okay, this sounds like a great project; Richard's going to do

statistical astronomy. I'm going to put you in touch with a physicist in the department, Dick Fong—Chinese but born in Britain—who's very good at statistics and a good mathematician. I'd like the two of you to build a research group to work on this." So I go to see Dick. He's a theorist, a very pleasant man. Then Wolfendale says, "We're going to give you students together. Richard's senior demonstratorship is coming to an end in '77, so we'll apply for money to keep him on as a postdoc while we build this cosmology group. And we'll strike a deal with the Royal Observatory at Edinburgh so we can analyze these plates." So, all credit to Wolfendale. We all go up to Edinburgh in his car, and we strike something like an MOU [memorandum of understanding] deal with these guys at the Royal Observatory. Durham's role will be to analyze the data scanned by the COSMOS machine from these Schmidt plates. The group in Edinburgh will do project A, and the group at Durham will do project B. So suddenly we're in big science, really. It was quite interesting.

ASPATURIAN: Wolfendale seems to have had a vision.

ELLIS: He did. But I think it followed from the talks I was giving on what I was finding—for instance, the orientation of galaxies and large-scale structure and so forth. So at about this time, [P. James] Peebles at Princeton was writing papers on the clustering of galaxies, how to measure the galaxy distribution statistically, and what do those measurements tell you.

ASPATURIAN: Where were the Princeton data coming from; were they using the 200-inch here in California?

ELLIS: No. At this time, there was no data really on the clustering of galaxies. Believe it or not, nobody was using the Palomar Sky Survey to do that kind of work. They didn't have a measuring machine. The great insight in Britain was to promote the development of these machines—the COSMOS machine and then later an equivalent machine at Cambridge. This put Britain in the lead on statistical astronomy, based on the data they were getting from these plates. There was one guy at [University of California] Berkeley, Richard Kron, who was counting galaxies, but that was later—1978, 1979. In '76, '77,

Britain was leading in this area. So Peebles was writing theoretical papers predicting what observers would see in the clustering evolution of galaxies. He predicted that as the universe expanded, the clustering would increase because gravity is an attractive force; it's bringing the galaxies together. If we could measure this evolution in the clustering, it would verify the whole paradigm of how structure grows as the universe expands. We approached it empirically, but Peebles' theoretical papers were appearing in the *Astrophysical Journal*, and so it became the mission of the group to make these measurements with these deep Schmidt plates.

ASPATURIAN: Was Durham the only group working on this?

ELLIS: Yes. So we very quickly got the attention of people in Cambridge, and I remember they would come up and talk to us. Then, of course, we gave talks in London on what we were doing. So suddenly Durham had carved out a niche for itself. Wolfendale had been the catalyst that got it together, but really it was a combination of my observational intuition and Fong's mathematical ability. He had the patience to go through Peebles's paper in great detail and explain the statistical tests that Peebles was proposing, and I had the data from these plates. And we had a very good student—Steve [Steven] Phillipps—who later went on to be a professor; he's now at Bristol. Later, because we were now improving our academic visibility, two very good students turned up. One was George Efstathiou, probably the best student that we ever had at Durham. He's now a professor at Cambridge. The other was Tom Shanks, a rather wild Scotsman, who'd done an M.Sc. in statistics at Imperial College London. With those two students, suddenly the place was alight with enthusiasm and bright people. Durham was no longer some backwater. We were suddenly at the center of British astronomy in the space of three years. It was amazing.

ASPATURIAN: Wolfendale's intuition in hiring you turned out to have been correct.

ELLIS: Even more to the credit of Wolfendale, we found a result that showed clustering was evolving.

ASPATURIAN: As Peebles had predicted.

ELLIS: Yes. So Wolfendale said, "Richard, get on a plane and go to America and give some talks." I couldn't believe it! Suddenly I got an air ticket to New York. This was just fabulous. I arrived in New York; it was January and bitterly cold. I went to Harvard and gave a talk. I went to Princeton, where I met the great Peebles. I went to Yale, and then I came back to New York, and then I flew home.

ASPATURIAN: So this was your first firsthand association with American astronomers?

ELLIS: Yes.

ASPATURIAN: What do you recall of that?

ELLIS: It was great. For the first time I saw these departments, and I was taken seriously. I was only twenty-eight.

ASPATURIAN: Yes, you were very young.

ELLIS: And I was accompanied by a guy called Mike [Stephen Michael] Fall, a theorist, who was at Harvard at the time. He had been at Cambridge, but he was very much associated with the Durham group, and we had written some papers together. So he acted as my host, and he took me under his wing, and he was great. He introduced me to Peebles and said, "Richard's got some fascinating results," and so Peebles said, "Let's hear it, Richard." We went to Yale, and Beatrice Tinsley was a professor there.

ASPATURIAN: She died very young, I believe.

ELLIS: Yes, she died. Beatrice said, "These plates are so deep, and they're so detailed that you could think about counting galaxies and seeing the evolution not just of the clustering of galaxies but the stellar populations in the galaxies themselves."

ASPATURIAN: The plates were that good?

ELLIS: The plates were that good. So we began a collaboration with Beatrice, where she would run her computer models to predict what we would see. That was a great period. It reached its climax, I would say, with our research at the AAT. George Efstathiou, for his thesis, studied elliptical galaxies, and he and I sat down one day and decided that we wanted to know whether elliptical galaxies were rotating. There had been various ideas, and Garth Illingworth—he's now at UC Santa Cruz—had written a very provocative paper saying that elliptical galaxies were not rotating. Ellipticals are galaxies that are, as the name suggests, elliptical in shape, and you might think that it's because the form of the object is governed by its rotation, in the same way that the sun is slightly flattened because it's rotating. But Illingworth found that that's not the case, and that their three-dimensional shape might be complicating the picture. They might not be flattened ellipsoids; instead they might have three axes that are completely different sizes—tri-axial objects. So George is a very clever guy mathematically, and he said, "What we ought to do is measure the rotation, not only about the major axis but also the minor axis. That would tell us what kind of shape these objects have." So, having never been to the AAT, we wrote a proposal.

ASPATURIAN: The telescope had just come online?

ELLIS: It had come online in 1974, and this was probably 1977. So we were still quite young, and had no experience with a big telescope. Well, we got two nights, and I flew to Australia. It was clear weather, and we got great data, and we found that these objects are not rotating, and so we wrote it up. That was my first observing run on the AAT. It was fabulously exciting because it's such a powerful facility. Australia is a great country. It was really amazing. At the observatory, we met an astronomer named Bruce Peterson, and he said to me, "I've seen your work on counting galaxies with Schmidt plates. Did you know that the Anglo-Australian Observatory, which is a much more powerful telescope, has a photographic facility? I've taken this very deep plate with the AAT, and it imaged objects something like six or seven times fainter than all of the Schmidt plates."

Now the Schmidt plates are big, but the AAT went deep in a very narrow area. To cut a long story short, we scanned Bruce's plate.

ASPATURIAN: With the Edinburgh COSMOS machine?

ELLIS: We scanned it in Australia with a very slow machine called a microdensitometer. Then we sent the raw data to the group at Cambridge who were building a rival machine to COSMOS, and their software processed all this data and produced a beautiful catalog of all the galaxies within that field. What we found was that there were many more galaxies than we would have predicted if the universe was not evolving, and that in the past, the galaxies were more numerous, brighter, and bluer. And Bruce, who was American although he was living in Australia, said, "This is so important; we ought to send this to *Ap J*." So Bruce and I and the group at Cambridge wrote a paper to *Ap J Letters*—in 1978; I think it was published in 1979. Peterson was the first author; I was the second author; and this was the first paper really to look at the properties of galaxies and show that there seemed to be a strong evolutionary effect, and that there were many more faint galaxies than we had expected. Well, this paper got an enormous amount of attention, and suddenly I got invited to all these conferences. So that galaxy-counting period sort of reached a climax with this 1979 paper. That's when I realized the significance of this for studying cosmic evolution—that the AAT lets us go deep enough to see changes in the universe.

ASPATURIAN: Was this an insight that had occurred to many astronomers, or were you one of the first to make the connection?

ELLIS: Beatrice Tinsley was writing papers saying that in the past galaxies were brighter and therefore we might even see their birth. But there were no data at all. Here we were using this fabulous new telescope, the AAT. I remember talking to Wal Sargent years later, and he said it was just a mystery why Caltech never got into this at the time. So, I suddenly saw the AAT as a fabulous instrument to study the evolution of galaxies in the universe. Because I'd done so much work on statistics on the Schmidt plates, I had all the machinery to predict what we would see, and then I had models from Beatrice Tinsley

on what kind of evolution we might expect. I was well placed to put together the big picture: Not only what are we seeing, but what would we see if we had even more powerful facilities. So in those days one wrote up one's conference talks; and I got quite good at writing conference proceedings up and looking at where the subject was going. I remember going to a very important meeting in Italy where I had to give a series of lectures on the evolution of galaxies, and they got written up. But I still didn't have a tenured job.

ASPATURIAN: With all this traveling in the U.S., did the thought cross your mind that you might like to come here?

ELLIS: At the time, no. We had two children by 1978, and we were settled in England, of course. Barbara was working some of the time. But I loved coming to America. I remember coming to Los Angeles for the first time in 1979 and thinking, "This is just a fabulous place."

ASPATURIAN: Did you come to Caltech at that time?

ELLIS: No. The conference in 1979 was at UCLA. It was called "Objects of High Redshift," and I gave a talk on the first morning of the conference about the Peterson paper. Some of us went to Disneyland, and I nearly got sick on Space Mountain. The conference dinner was at Mount Wilson. I thought, "This is just a fabulous place." The climate alone was impressive.

ASPATURIAN: You certainly crowded a lot of activity into a few days.

ELLIS: That's right. By now I was a regular observer at the AAT, so I was into jet setting.

ASPATURIAN: You were able to get the telescope time.

ELLIS: Every three months. Maybe three or four times a year I was going to Australia. And I was learning how all the instruments worked.

ASPATURIAN: What was happening generally in astronomy in Britain at that time?

ELLIS: The AAT was now the big news, and there was more government money for astronomy. We were building the UK Infrared telescope [UKIRT].

ASPATURIAN: Was that the first one to go up on Mauna Kea?

ELLIS: It was the first British one. There was already a telescope on Mauna Kea from University of Hawaii.

ASPATURIAN: Oh, yes, that's right.

ELLIS: It went up about the same time as the Canada France Hawaii telescope. Later we started building the William Herschel Telescope in the Canary Islands, and the Isaac Newton Telescope—remember the one that was originally in the Pevensey Marshes — got moved there as well. So that was all happening. It's inconceivable now—Britain was building one 4-meter telescope, was on the starting blocks with two more 4-meter telescopes, and was moving a telescope out to the Canary Islands as well.

ASPATURIAN: What a terrific era in which to be a young astronomer.

ELLIS: There was an explosion of telescope time to rival America, without question. Technologically we had the measuring machines for the photographic plates, and then we had this fabulous detector—the Boksenberg Image Photon Counting System—one of which was permanently resident at the AAT. I learned how to use that: It was a very efficient detector, excellent for spectroscopy. Alec Boksenberg, who built it, had been one of my undergraduate lecturers at University College. So the turning point for these galaxy counts was that I realized that to interpret how far we were looking back in time we needed redshifts.

ASPATURIAN: How had the interpretations been done up until then?

ELLIS: Just the brightness of the galaxies.

ASPATURIAN: That can be very misleading, though.

ELLIS: Absolutely. We had done a little survey, called the Durham Anglo-Australian Redshift Survey, which looked simultaneously at the luminosity functions of about 330 bright, nearby galaxies. This was part of George Efstathiou's thesis, so we did a series of papers with George, and Tom Shanks was involved, too. We were looking at a fair sample of the local universe out to maybe a redshift of 0.1 corresponding to 17th magnitude. So we got the spectra of 330 galaxies in various selected areas in the sky, measured all their redshifts, and then used that and the expansion of the universe to calculate how far away they were, get their luminosity, and produce what we called the luminosity function—the distribution of luminosities of galaxies per unit volume of space. We had some competition—there was one U.S. group doing this at Kitt Peak [Kitt Peak National Optical Astronomy Observatory, based in Arizona], and that was Bob [Robert P.] Kirshner, Gus [Augustus] Oemler, [Paul L.] Schechter and [Stephen A.] Shectman. They called theirs the KOSS Survey. So there we were, competing with a U.S. group. Once we had this luminosity function, then I had the machinery to say that if it was the same everywhere in the universe at all times, I could predict how many galaxies I would see in my deep photographs.

ASPATURIAN: You would have a baseline. I see.

ELLIS: So the question was: If there are more galaxies than we predicted on the basis of this, where are they, and are they very, very far away? Beatrice Tinsley said they're probably young galaxies in the process of formation. So we needed to measure their redshifts, but they were very faint. And to study them one at a time would be exhaustive—we'd need four or five hour exposures for each one. Then something very interesting happened. I was in London and—

ASPATURIAN: What year are we in right now?

ELLIS: We are probably in the early ’80s—1979 to 1980. And Roger [James Roger Prior] Angel, professor from Steward Observatory, was in London. He is a Brit, and he gave a talk at the Royal Astronomical Society about a technique called multi-object spectroscopy where you had a brass plate and an array of optical fibers, and you plugged all these fibers into holes that had been drilled into the plate at the precise positions of galaxies in the focal plane of the telescope, and then you got the spectra of, say, twenty galaxies at the same time. I remember thinking, *“This is what we’ve got to do to get these faint objects.”* So I wrote to Don [Donald C.] Morton, the director of the Anglo-Australian Observatory, and said, “This is the future for the AAT. We’ve got to do it.” He was a Canadian and a very powerful guy, but very nice and friendly. He said, “I think Richard’s right. Multi-fiber spectroscopy is something the AAT could do very well.” So when I was in Australia, he introduced me to an engineer called Peter Gray, who subsequently has worked on the TMT [Thirty Meter Telescope]. And Peter Gray and I worked together to create an instrument that would do multi-fiber spectroscopy at the AAT using Boksenberg’s image photon counting system. So in the early 1980s we went into production on measuring the redshifts of these distant galaxies with a multi-fiber spectrograph, and started doing very deep surveys to study exactly how the universe was evolving: How far away are these faint, blue galaxies? Now, of course, these multi-object spectrographs are everywhere; we have them at the Keck Observatory. But this work really started in earnest at the AAT. I met Roger Angel years later and told him, “Your talk inspired me.” He said, “It’s very strange; we definitely initiated it, but we never really took it to its logical mass production. You guys have done well with that idea.” And that’s because Peter Gray made the spectrograph what we call a common user—that is, a facility that anybody could turn up and use. That’s what you need to do with these instruments, make them user friendly. Whereas at the Steward Observatory, the guy who did this, John Hill, did it for his thesis, and then he moved on and did something else. He didn’t make it user-friendly, and he didn’t write up his results. So very few people followed up with it, and it got discarded. Whereas at the AAT we made it a professional tool, and it became very effective.

ASPATURIAN: People were able to use it and extend its applications.

ELLIS: That's right.

ASPATURIAN: Let's step back for one moment to the fact that you still did not have a tenured position.

ELLIS: Okay. At Durham, Dick Fong was a lecturer, maybe even a senior lecturer, and there was a bit of tension because it actually was his research group, and here's Richard Ellis, who doesn't have tenure, whizzing around the world giving talks and seeming to be a law unto himself. I remember him coming down and telling me in the nicest possible way that he had given me infinite freedom, but he did expect me to work on certain things. He was the grant holder, so in effect he was paying for me.

By now we had two children, and I was beginning to get worried—I felt I should have tenure. Since Barbara was half-German, born in Germany, we considered going there. I met a German professor from Göttingen at a conference in Cambridge, and he said, "I could get you a permanent job at the university in Göttingen." I went to Germany on a lecture tour, and I gave talks at Göttingen, Munich, and Bochum, which is in the Ruhr somewhere. They're all famous university groups, and I did get offered a position in Göttingen. I went to Wolfendale and told him, and I also told Martin Rees, who was head of the astronomy group at Cambridge and a very influential guy in Britain. I remember Wolfendale saying, "Oh, God, don't go to Germany." Rees said, "If you want to go to Germany, the universities there are very weak compared to the Max Planck Society." So in the end, I turned down Germany, and I applied for a position at Sussex University. That was a ridiculous situation. I think I was observing in Hawaii when I suddenly got a telex saying I'd been short-listed for an interview with Sussex on such-and-such a date next week. I said, "Well, I'm in Hawaii. Would you cover my airfare to fly back?" and they couldn't cover the expenses. So I said, "Well—" and I didn't get it. I got pretty close, I think.

Then in 1981 a job opened at Durham. There'd been a previous lectureship opportunity in 1979 that I interviewed for and didn't get. The way these positions work is very different from in the U.S. Everybody turns up on the same day, and the panel

interviews each candidate in turn and makes a decision that day. It's very much more efficient than the U.S. process. But Wolfendale could see that I had one foot out of the door, and that if he didn't do something pretty soon, I'd be gone.

ASPATURIAN: I'm puzzled that with the reputation that you had established for yourself and for Durham they gave this 1979 lectureship to somebody else.

ELLIS: Well, it wasn't in astronomy; it was in physics. Wolfendale had an enemy in the department. The other professor there was a guy called [Brian] Bransden, and the two of them were at loggerheads. Bransden had a candidate, and I was Wolfendale's candidate, and believe it or not, they couldn't agree. So what happened was that after I interviewed, I was told to go back to my office and that maybe around 5 o'clock, they'd have a decision. So I'm waiting in my office—5 o'clock comes and goes, then 7 o'clock, and nothing happens. I thought, "Bugger this, I'm going home." So I went home, and at 7:30, I got a call from Wolfendale saying that the committee had been unable to reach a decision and would meet again in the morning. When I went into work in the morning, I met a member of the committee, who said, "You didn't get it." So I went to the office of the other guy, who was Bransden's favorite, to congratulate him. I said, "Well, it seems you must have got it." He looked me in the face, and he said, "No, I didn't get it; I thought you got it." And I said no. And we went, "Who got it?" Do you know what happened—and I only found this out years later. They couldn't agree. We were both equally ranked Number 1. They couldn't agree so they went for Number 3. [Laughter] It was unbelievable! And that guy is still there. In 1981, Wolfendale finally got permission for a lectureship in astronomy, and I interviewed for that and got it and finally got tenure.

ASPATURIAN: As a postscript to this, Wolfendale subsequently became the Astronomer Royal?

ELLIS: That happened while I was there, I think maybe in the early '90s.

ASPATURIAN: Had he made any major contributions himself by that time or was it mostly the result of presiding over the evolution of the Durham astronomy program?

ELLIS: The straight answer to your question is he's not a distinguished astronomer, for sure. He's a famous cosmic-ray physicist. He's a very charismatic figure, and he is very persistent in political circles. People were a little surprised when he became Astronomer Royal—the newspapers came out with the title "the Dark Horse." Then he got a knighthood. And he's really into that territory. But he's been very good to me. I have a real soft spot for Wolfendale.

ASPATURIAN: You're still in touch with him? He's still alive?

ELLIS: He's still alive, and he's still writing papers. All righty. Shall we pause there?

ASPATURIAN: I think so.

Some material in this session was originally recorded during interview session 3.

RICHARD ELLIS

SESSION 3

January 30, 2014

ASPATURIAN: I wanted to step back for a moment and ask about something. You had a religious upbringing as a child and teenager until you decided not to be baptized. I wondered what impact you think this had on your outlook, your thinking.

ELLIS: I think it made me less aggressive than perhaps many people. I think it gave me patience. I had to sit through sermons every Sunday and be a good boy during the first fourteen years of my life. Go to Sunday school—there was no debate about it. Although I did rebel, and I had these signs of being independent, like not being a prefect in school, I think that being brought up for such a long time in that strict environment made me less impulsive, perhaps, and more patient. Now, that may have actually damaged my career. For instance, when I went to Oxford as a graduate student, and I couldn't do the research that I wanted to—in today's climate no student would accept that. They would say, "Hang on a minute; I've come here expecting to work on this, and you've misled me." But I was still fairly junior and felt that I didn't want to be assertive, which may have been to the detriment of my career. Imagine if I had done a thesis on galaxies or cosmology, then I probably would have gone to Cambridge as a postdoc straight away. Instead, I had to go to Durham. But when you look back, these things eventually all work out.

ASPATURIAN: In your case certainly.

ELLIS: I know I don't regret going to Durham at all. But as far as belief goes, I never mixed religion with science, and I'm not a religious person today. I'm an experimentalist, and I'm trying to find what's going on in the universe. I don't have any preconceived view.

ASPATURIAN: I also realized, listening to you last time that suddenly you were doing all this traveling. As quite a young man you went to Australia, you went to America, and it sounds like you hopped around Europe quite a bit. What impact do you think this had on you?

ELLIS: Oh, I loved it. Back then I was regarded as one of the most well-traveled people in the Durham physics department, and that's true even here at Caltech. I remember a student at Durham making a joke about it and saying, "It must be your father. It's the same reason why your father ran away from home."

ASPATURIAN: Wanderlust?

ELLIS: Yes. I think that's right. I just love it. I've always liked traveling. I like the challenge of entering a new country and figuring out how the science there works, and I always value collaborations with foreigners. That's my goal, really, to be a global astronomer. Even though I've been at Caltech now for fourteen years, I think when I go back to Britain that I'm still regarded as a British astronomer, really. So that's nice.

ASPATURIAN: When we left off last time, I think you had just been appointed to a permanent, tenured lectureship at Durham. And as I recall from your Royal Society write-up, you set about building up a robust astronomy program there. Could you talk about the science, the politics, the culture, all of that.

ELLIS: Yes. As I said last time, this was a great era because Britain was still expanding its facilities. In the early '80s, we had the AAT, UKIRT, and the ill-fated Isaac Newton Telescope—remember the one on the Pevensey Marshes? In 1984 they moved that one to La Palma, the western-most island of the Canary Islands, and built the La Palma Observatory around both it and the William Herschel Telescope, which came online in 1987 and was the biggest one of all. So in Britain, this is now the height of the astro-empire. We have telescopes in Australia, Hawaii, and La Palma. And just think of it, along with all this observing time comes the need for instruments on all these telescopes. Durham is emerging as a major player in all of this, alongside the two royal

observatories—the Royal Observatory in Greenwich, which was actually now in Sussex, called the RGO, and the Royal Observatory Edinburgh, ROE, in Scotland. There were very few instrument-building groups in the universities. The traditional model was that the instruments were built in the royal observatories, but the government funding agency, which at that time was known as the Science and Engineering Research Council, SERC—it's like the NSF—was very keen to increase the instrument-building capability at universities. And that's because they ultimately wanted to get rid of these royal observatories.

ASPATURIAN: Why was that?

ELLIS: Well, I didn't realize it at the time, but they felt these were civil-service institutions. RGO goes back to 1670 or something [1675 –*Ed.*]. I think the government felt that because it funded the university groups, it could exert more control over their agendas and activities, whereas the royal observatories' employees were civil servants paid for life, with inflation-proof pensions**.** It was the beginning of Thatcherism in a way. The attitude was, Why can't we tailor the resources we put into research according to the needs, rather than continue to support these establishments with a fixed income regardless of the research program? They felt the budget should be tailored to what the opportunities were and not a fixed thing from year to year. So we rode this wave very well at Durham in developing an instrumentation effort. The first instrument I was involved in, which I think I described, was the multi-fiber spectrograph collaboration with the AAT.

ASPATURIAN: This was the technology Roger Angel had developed? [See Session 2]

ELLIS: That's right, yes. Then pretty soon after I got my tenured lectureship at Durham, we started to collaborate with the RGO on a faint-object spectrograph for the recently moved Isaac Newton Telescope. This was exactly the kind of astronomy center that Wolfendale had wanted to see blossom at Durham, and certainly within the university there were no political problems. It still was a physics department, but I was the lead person running the astronomy show as it were, especially in the observations. There was

generous funding at that time for postdocs and instrumental projects. So we started recruiting good students and good postdocs, and we were building these instruments.

ASPATURIAN: Were the instruments being primarily built by the physicists?

ELLIS: The way it worked was that I would be the project scientist, and physicists who had technical experience and were under orders from Wolfendale to get involved in astronomy, would help me. The faint object spectrograph for the Isaac Newton Telescope involved a solid-state physicist, Mike Breare. He was older than me and his research area was diminishing, so he was very keen to get into astronomy, and Wolfendale was saying "Work with Ellis on this spectrograph." Scientifically, as we discussed, the territory was the evolution of galaxies and the challenge was, how far away are these galaxies, and how far back in time are we looking at them? So getting increasingly efficient spectrographs and measuring the redshifts of many galaxies at once was what was driving me in this. It culminated in an instrument that again I worked on with RGO. It was called the low-dispersion survey spectrograph, LDSS.

ASPATURIAN: What year are we talking about?

ELLIS: That's about 1983, 1984. LDSS was a truly innovative instrument in that it had a very wide field of view. There was a brilliant optical designer at the RGO named Charles Wynne, in his sixties at the time and a somewhat difficult character but a very clever optician. He had no duties other than to think about innovative optical design. So he came up with this spectrograph design that had a 12-arc minute field of view at the AAT. Twelve-arc minutes meant that you could get the spectra of fifty very, very faint galaxies at once, each viewed through little slits. Very much like the instruments we have now on Keck, but this was 1983. So we wrote a proposal to the SERC, with me as the principal investigator, and we got the grant to do that. That was probably one of the most exciting projects at the time, and then we got CCDs [charge-coupled devices] behind it. So there were two techniques: We had the fibers, which were useful for brighter objects, and we also had a student, Ian Parry, who came up with the very clever idea of building a robot called Autofib that would move these fibers one by one. So we were simultaneously

innovating in the fiber area and in spectrograph design. We had both instruments going at the AAT by the mid-'80s.

Then we built two new instruments for the William Herschel telescope, a faint object spectrograph and a new, refined version of the LDSS. This period—1983 to 1988—was, I think, one of the most exciting periods at Durham. The only real competition we had was from Richard Kron and David Koo at Berkeley, and they were using a similar instrument on the Kitt Peak 4-meter. Palomar, as Wal Sargent told me years later, never had anything in this territory.

ASPATURIAN: Why do you think that was?

ELLIS: There was nobody on the Caltech faculty committed to it. A few years later they tried to get into fiber technology, but I don't think that Caltech had the depth of technical experience or the dedication. There wasn't a champion on the faculty who wanted to do it.

ASPATURIAN: Based on what I remember from that period, I think Caltech was throwing an enormous amount of resources into radio [millimeter and sub-millimeter] and infrared.

ELLIS: Infrared, yes. Because it was Neugebauer's era [Gerald "Gerry" Neugebauer, Millikan Professor of Physics, Emeritus, d. 2014]. So two people at Durham stand out from this period: Tom [Thomas] Broadhurst, who arrived as my student at the time these fiber instruments were being built, and Matthew Colless, who came as my first postdoc once I became a professor in 1985. So those two were involved in these major studies. Broadhurst worked on the fiber survey, and we did the first comprehensive redshift survey of several hundred galaxies out to redshifts of about 0.6. It's called the Auto-fib Redshift Survey.

ASPATURIAN: And how far back would redshift 0.6 be?

ELLIS: That's about five billion years—about a third of the way back to the Big Bang. And then Colless and I and later Karl Glazebrook worked on the Herschel LDSS, and that pushed back to a redshift of 1, which at the time was the frontier. That's about 1988.

ASPATURIAN: What were you looking at specifically?

ELLIS: We had found that there were many more faint galaxies than predicted. Assuming that the universe was unchanging, you could take a slice of the local universe and predict what you would see as you went fainter, that is, presumably farther back in time. It's a bit like doing a census of the area where you live, and then predicting how many people live in that region as you go farther and farther out. But we found that what was called the "no-evolution prediction" failed miserably to account for what we saw at fainter magnitudes and presumably greater distances in the following sense: The galaxies at faint limits were bluer than the galaxies today, and there were many more of them. Beatrice Tinsley, the professor at Yale, had theorized that this could well be because we were seeing extremely distant galaxies that were very luminous, maybe observing them at the time that they formed.

ASPATURIAN: Very young, active galaxies?

ELLIS: Yes, what we call primeval galaxies. But how could you tell just from the brightness and colors? You really needed to prove what redshifts they were at.

ASPATURIAN: You needed their spectra?

ELLIS: You needed the spectra. So what Broadhurst and I found was that while the so-called faint blue galaxies were numerous, they were not at great distances. Paradoxically, they were within the same redshift range that would have been predicted from the no-evolution model. So it takes a while to get your head around this. There are many more galaxies than predicted, but we're not seeing them at greater distances. And the explanation for this is that as you go back in time, the number of feeble galaxies increases and since they have higher star formation rates, they're bluer and become more visible.

So what we called luminosity-dependent evolution was the discovery that we made. Firstly, the universe is evolving, the galaxies are changing, but they're changing in a way that depends on how intrinsically bright they are. The most massive galaxies are hardly changing at all. And the feeble galaxies—

ASPATURIAN: When you say feeble, you mean smaller, less massive?

ELLIS: Smaller, less massive, and less luminous; those are changing more rapidly. This result, which dates from about 1987–88, is now an accepted part of the story of galaxy evolution.

ASPATURIAN: Changing more rapidly how?

ELLIS: Decaying.

ASPATURIAN: Oh, they're decaying.

ELLIS: They were shining and blue then, but today, they've faded away or merged together. Now if galaxies merge, then their numbers are not conserved. You start out with lots of these smaller objects, and they merge, and you don't see them today. So the idea of this non-conservation of numbers and luminosity-dependent evolution seems quite natural now when you think about it, but of course in the 1980s, we all saw galaxies as isolated things that never interacted with one another and whose behavior was governed by the way that their stars evolved. Today we know that it's much more complicated. And so the picture of evolution that came out of those surveys was transformative in a way.

ASPATURIAN: Now, this was the mid to late 1980s?

ELLIS: Yes.

ASPATURIAN: The reason I'm asking is because I know a lot of work was being done here on the IRAS [Infrared Astronomical Satellite] data at that time. And they were finding a lot of galactic activity in the infrared.

ELLIS: Yes. We were involved in the IRAS revolution in the second half of the 1980s. But all those were local observations to determine how the galaxies were distributed in space. IRAS showed us that there are many more star-forming galaxies—starburst galaxies—detected at infrared wavelengths via their hot dust than are seen in the optical. So this idea that the cosmic census is incomplete is one aspect of the IRAS story. The more interesting part, which we were involved in, along with a guy called Michael Rowan-Robinson—who actually had very strong connections with Neugebauer and Tom [B. Thomas] Soifer [professor of physics]—was measuring these IRAS galaxies' redshifts. That turned out to be easy because they have so much gas that's heated by their young stars, and that gas glows in emission lines. One of the big surveys that we did with one of my instruments, the faint object spectrograph, was an IRAS redshift survey of several thousand galaxies. That charted the large-scale galactic distribution, which was a key result from the IRAS catalog. So that came from this period, too. But IRAS didn't see typical galaxies to the cosmological distances that we were observing optically.

ASPATURIAN: I see. It was more local?

ELLIS: Yes, it was a more local survey. In terms of large look-back times—redshift 1—our optical surveys were the most effective.

ASPATURIAN: I assume that now you began publishing papers on galactic evolution? What kind of reception did you get?

ELLIS: Oh, great. I remember going to a conference in Beijing, where I gave a plenary review summarizing all the hard work we'd done on these two spectrographs. So I think in that area of spectroscopy, with the exception of Koo and Kron at Berkeley, we were largely on our own. And I think we were more effective. One of the big advantages that I had, especially when I became a professor, was being in charge of the instruments. The

instruments I was using were designed to do what I wanted to do. When one of my instruments arrived at the telescope, I would make an ambitious request for observing time, arguing that the instrument had been specifically designed for what I wanted to achieve. And so I was getting a lot of observing time and a lot of support from the British astronomical community, particularly after I became a professor. I was made a professor quite young. Nowadays that's not so unusual, but at that time—

ASPATURIAN: You were thirty-five?

ELLIS: Thirty-five. Full professor.

ASPATURIAN: That's reasonably young. Do you think the fact that Durham's program was young and hungry benefitted you?

ELLIS: Yes, it did. Everybody knew about Durham. We were called the Durham Group. Even in the U.S., people said, "Wow, I hear a lot about Durham; don't remember Durham before; what's happened?" So it was great. And we hired Carlos Frenk for my lectureship position after I was promoted to professor. I should probably go through the history of how I was appointed to the chair of astronomy. It was a new appointment, a created position at Durham, and had been Wolfendale's dream since the mid-'70s. In fact, he went to the SERC and said that if SERC funded or contributed to the chair's first few years, the university would then take over the rest. He was whizzing around the country trying to get this chair up and running, and in 1983 he succeeded, and the chair was advertised. When I became a lecturer in 1981, I could see that this chair was eventually going to happen. I thought I was too young to get it, but on the other hand, imagine if somebody else was appointed to it who would take charge and redirect things. Maybe I wouldn't have the freedom that I currently had.

ASPATURIAN: It's politically a little dicey?

ELLIS: Yes. So I was very worried about who this might be. I started wondering whether I should have one foot out of the door. When it was advertised in 1983, I didn't

honestly think I stood a chance of getting it. But in the end I got it because of a long, rather interesting story. The way these professorships are appointed in Britain is that there's a committee with members both internal and external to the university. This committee consisted of the vice chancellor of the university—the equivalent of the president—Wolfendale, as head of department; a professor from another department—I think it was engineering—and two external professors: Martin Rees, who was at Cambridge, and Alec Boksenberg, who was the director of the RGO. I think there were three front-runners, including me. The person who I thought would get it—and it would have been fine, probably, was Bob Carswell—an observer in Cambridge, a very nice guy. He worked on quasars—he's older than me and has just retired. We were all interviewed on the same day—so we all bumped into each other—in this grand room in the university headquarters called Old Shire Hall—a red brick building with big tiles and portraits. So in this big room with wooden tables, I was grilled. I remember Barbara coming down to the building at the end of the day to see how it had gone. Then Bob Carswell and I were called back in together, which I thought was very unusual. The vice chancellor sat both of us down and told us that the committee hadn't yet made a decision. They needed to interview another candidate, but both our applications were "very much on the table." I remember thinking, "What the hell does this mean?" What had happened was that Wolfendale, in his enthusiasm, had eliminated a senior applicant called Bernard Pagel, saying, "This guy is sixty; he's far too old, he's not the man we want." And the external members of the committee, Rees and Boksenberg, only found this out on the day. Pagel was a famous guy, a Fellow of the Royal Society, and they protested that such a distinguished person shouldn't be eliminated, and that therefore the committee had to interview Pagel. I certainly wasn't told this; I figured it out independently. So Pagel came up to Durham, and they interviewed him.

ASPATURIAN: Where was he?

ELLIS: He was a senior astronomer at the RGO, where Boksenberg was. You might wonder why a sixty-year old would want to move to Durham at that stage of his career. The answer is that in the civil service, sixty is the retirement age, and he wanted to have

his pension *and* have a salary. But he was not an observer. He was really an analyst, who studied stars, specifically stellar populations. Rather a formidable figure, definitely of the old school. Anyway, he came up, interviewed, and, of course, he shone and was offered the job. So then I remember this amazing day when Wolfendale called me into his office and presented a completely different face to me for the first time. He said he had some great news for me: "Bernard Pagel has accepted the chair of astronomy." I remember thinking, "Oh, okay." He said, "That's good isn't it?" And I said, "Yeah, that's going to be great." So by now, I was myself—this is a long story—working part time at the RGO because this was the Canary Islands era, and this was obviously part of my attempt to have one foot out of the door. So I congratulated Bernard Pagel on his appointment. He was very nice to me; we had dinner and so forth. He started quizzing me about how does Durham work, what do I have to do as a professor, and I realized as the weeks went by that there was some nervousness in his mind about how this was going to work. In the end, it got to him. He started not sleeping, he lost weight, and he had a nervous breakdown. Just by himself. We all tried to help him. Although he accepted the position, he eventually turned it down and never came.

ASPATURIAN: For how long did this go on?

ELLIS: It took maybe six or eight months. But there was no previous chair holder, so it wasn't noticeable. Meanwhile I had one foot out the door, working at the RGO, and now the question was whether the vice chancellor would honor his promise that my application and Carswell's were still on the table. And then to my annoyance, what they did was advertise it again.

ASPATURIAN: Do you think, you were clearly for your age—

ELLIS: Too young.

ASPATURIAN: Well, no, you were becoming fairly prominent in the field as a very young astronomer. Was there a sense of competitiveness to the effect that, We don't want this young colleague to get ahead too far, too fast?

ELLIS: There may have been a bit of that in the department, although I didn't feel that they were against me. I was surprisingly tactful. I didn't go around huffing and puffing. And nobody came to me and said, "Just hang in there, Richard," or anything like that. I tried to work with Pagel; I sat down and explained how you get grants. Since he was in this privileged position of being in the Royal Observatory, his research was funded without ever writing a grant proposal. It's like Carnegie [Astronomical Observatories]. And he'd never had a graduate student. So now he was asking me the most basic questions.

ASPATURIAN: I am surprised that they wanted to hire him. It sounds like it might have been a disaster.

ELLIS: Well, I don't know. He was a very distinguished guy. But why he got so worked up about it, I'll never know. In the end, he accepted a professorship in Denmark, so he did get another job after his retirement from the RGO, and we became close friends. Although we never spoke about his decision at the time, years later he said he was not proud of that part of his academic story. He was always very nice to me. Anyway Durham advertised the position again, and so there I was, back in this room. Same room, same committee, same questions; and I told them at the interview, "You asked me that two years ago," and they laughed and said, "Well, say what you said last time." Anyway, this time I got it, which was nice. Bob Carswell did not apply the second time, and he told me he was pleased I got it. So that was fabulous. Suddenly we were much wealthier as well. The salary difference was enormous, from a lecturer to a professor.

ASPATURIAN: Your parents must have been very happy for you.

ELLIS: Yes, they were very pleased. So that was great.

ASPATURIAN: You mention in your Royal Society memoir that the two years [1983–1985] you spent away from Durham at the RGO was kind of a mixed period. What was it like for you there?

ELLIS: Well, at the time I was still a lecturer at Durham, and the Canary Islands Observatory was opening up. I admired Alec Boksenberg, the director of the RGO, very much; he was a frequent visitor to Caltech—a collaborator with Wal Sargent. He had been one of my lecturers at University College, London, and his Image Photon Counting System had revolutionized British astronomy. So he was like a hero of British astronomy, and, not surprisingly, while he was a professor at London, he got invited to apply for the RGO directorship, which was at the time the most prominent position in British astronomy, other than the Astronomer Royal, which is really a more of a ceremonial title. The director of the RGO was really the lead observational astronomer in the country. At that time, the RGO was in a castle called Herstmonceux in Sussex, because that's where the Isaac Newton Telescope in its wisdom had been placed— on these marshes where it was foggy all the time. It was under Boksenberg's leadership that they had negotiated access to La Palma, the westernmost island in the Canaries chain and a beautiful site. It's a very, very steep volcanic island with the observatory at the top. Boksenberg and I got on very well, and I said, "What if I wanted a two- or three-year period at the RGO?" and he said, "Definitely." He managed to convince the SERC to give me a position there as a research fellow with a pay rise and a leave of absence from teaching at Durham to get involved in the La Palma observatory.

It started out really well because I really had freedom, but I also had duties. I had to support astronomers at the telescope, which meant I had to go to the Canary Islands for maybe two weeks at a time, every few months, and when visiting astronomers came I was in charge of helping them use the instruments. So this was a service role. Initially the whole family moved down to Sussex, but it was much more expensive than Durham, and after several months, Barbara and the kids went back to Durham, and I commuted from there. I would fly down on the Monday morning, stay in a kind of guesthouse, and then fly back on the Friday night. So I did this for about a year, and it was pretty miserable, especially being on my own in the evenings. In some sense I had been foolish to not think this through, although I got a lot done and learned a lot about telescopes and supporting people. I became an expert on the use of the spectrographs on the Isaac Newton Telescope. But of course there was a slightly ulterior motive: I knew that the chair of astronomy was coming up at Durham, and I wanted to have one foot out the door

when the advert came out. So, sure enough when I applied, I applied on RGO letterhead, so it was obvious I was not an internal candidate. In the end, I managed to reduce my time in Sussex to two years, and I spent three months of that on a leave at the Space Telescope Institute in Baltimore because we all thought the launch of the Hubble Space Telescope was imminent. Boksenberg was marvelous throughout. He never said, "This isn't what we agreed," when I went to him and said, "It's not working, Alec. Barbara and the children want to be back in Durham at school." He said, "That's fine, whatever you think makes it work." He was very good about it. But I think many of the more senior people at the observatory thought I had let them down. I was there during the week, and on weekends I whizzed home. We didn't integrate; it was really just a work role. So that was my period as a civil servant. I learned a lot, and of course, I made connections with everybody at the observatory.

ASPATURIAN: So, to back up a bit, at this time you were working on these faint galaxy surveys and studying the evolution of the cosmos, and there was really nobody else doing this except these two fellows at Berkeley.

ELLIS: Actually, there were three groups. The third was at Bell Labs, where Tony [J. Anthony] Tyson was counting galaxies. He was a pioneer of CCDs, so he was doing extraordinarily deep imaging, but he was not doing spectroscopy. Koo and Kron at Berkeley realized the power of spectroscopy, as did I. And then you're right, there was the infrared revolution, and in the infrared region for distant objects, UKIRT was at the frontier. The group leading that was at Edinburgh—Simon Lilly, who was a graduate student there, working with Malcolm Longair, who later moved to Cambridge. So there was the Lilly-Longair effort, which was looking at the infrared flux of radio galaxies, which could be seen to redshift 2. So to find the stellar-evolution content of the radio galaxies, the UKIRT telescope was important. I teamed up with David Allen, an infrared astronomer who also had been at Caltech. He was like the leading astronomer of the AAT and an amazing guy. He had an infrared camera that was superior to the one on UKIRT.

ASPATURIAN: These must have been studies in the near-infrared, though, on these ground-based telescopes?

ELLIS: Yes, some of the studies I did were in the near-infrared. This brings us to what we thought would be the launch of Hubble [Space Telescope]. That was expected in 1985–86, and, as I said, I took a three-month sabbatical at the Space Telescope Institute in anticipation of getting ready for it. Then of course, there was the Challenger disaster [January 28, 1986], and the launch got postponed to 1990. So I think that covers the 1980s, and when I look back, it was an amazing time for me, instrumentally, professionally, and scientifically. We really opened up the universe to redshift 1.

ASPATURIAN: I think that in the late 1980s, you also became involved in a number of telescope committees, nationally and internationally?

ELLIS: Right. There was some embarrassment that in Britain we didn't have a long-term plan in astronomy similar to the *Decadal Survey* in the United States. So in 1985 the Royal Astronomical Society decided to construct a ten-year plan for British Astronomy. Since I'd just been promoted to a professor, I got put on this panel. There were about five of us, and it was chaired by Sir Francis Graham-Smith, who's a very distinguished radio astronomer. I was the youngest member. We wrote a report, and one of the two things that came out of it was that Britain should think about how to build an 8-meter telescope. We now had the William Herschel Telescope in the Canaries, but in Hawaii, the Keck was being built, and California astronomy was once again riding off into the sunset. And we were going to be left behind, so Britain needed to plan its next big telescope. The other project that got a lot of support in the review was developing a multi-fiber spectrograph to study the distribution of galaxies.

ASPATURIAN: Which you had already been doing for a few years.

ELLIS: Which I had been doing through the work with Peter Gray and Ian Parry. So clearly I was interested in both of these projects. Our report then went to the SERC, and they said, "What we need is a panel to investigate how to develop these two ideas." The

idea was to cost them out and figure out whether one or both could be done as an international partnership. So they created a panel called the Large Telescope Panel, LTP, and they asked me to chair it. It had only three members, including myself. Jim Hough was an instrumentalist who studied active galactic nuclei at what is now the University of Hertfordshire. He was a very experienced committeeman, a very nice guy and politically very smart. The other guy they put on it was Mike Edmunds from the University of Cardiff. He was an observational astronomer who studied nearby spiral galaxies, and again a very frequent committee member. So we set about writing a report, and of course the main challenge was, how much does a large telescope cost, and can we afford it by ourselves? And if we can't afford it by ourselves, whom should we partner with? This was new territory for me: I'd never chaired a national committee before, and I had to get support from the community because this RAS report had not been debated within the astronomical community. Unlike the *Decadal Survey* where there's a huge amount of input, Britain hadn't organized itself that way. So I had to visit universities up and down the country, basically giving seminars on what we were trying to achieve and why it was important to get a big telescope and a big multi-object spectrograph. Then of course there was the financial issue of working with both royal observatories to get them to design these two capabilities. Were these new facilities? What kind of design would they have? How much would they cost? And finally, there was the international aspect: Are other countries thinking the same way, and would they like to share in developing such a facility with us? We went to Germany. We went to the national observatory here in the U.S., to Australia, and to Japan, which was perhaps the most interesting, because Japan had never had an international collaboration in astronomy, but they were trying to build an 8-meter telescope too. At one point it looked very promising that there would be a UK-Japan large telescope, but in the end it didn't happen. Japan wasn't ready for an international effort—their ministry had no ability to collaborate internationally. They'd never done it.

ASPATURIAN: They didn't have any infrastructure?

ELLIS: They didn't have any infrastructure for international collaborations and Japan eventually told us a deal with the UK was just never going to work. So the culmination of all this was a report that recommended that Britain explore two options for a large telescope. One was a collaboration with Spain in the Canary Islands, which was natural because we already had an observatory there. The other option was to join the U.S. effort that eventually became Gemini. This involved building two telescopes, one in the north hemisphere and one in the south, and Britain would then be a twenty-five percent partner of two telescopes. Canada eventually joined that project. So you can see the angle here. I set out originally to get Britain a large telescope all to itself, but this was too expensive. So we were told to go back and get the UK half an 8-meter. The Spanish option gave us half of one 8-meter, and the U.S. option gave us twenty-five percent of two.

So that was the large telescope option. The multi-object spectrograph option involved having optical designers, such as Charles Wynne, propose a new corrector for the AAT. This gave it a 2-degree field of view, which quadrupled the area of sky that you could observe at one time. That became what is known as the 2dF, the Two-degree Field instrument. Both the Gemini telescopes and the 2dF eventually got built.

ASPATURIAN: Where are the Geminis located?

ELLIS: The Gemini telescopes are now in Hawaii and Chile. And Britain was a member from the beginning, although the UK pulled out about two years ago due to the economic downturn. That was very sad. And the 2dF project was finally completed in the mid-'90s, and we did the 2dF Galaxy Redshift Survey at the AAT, on which I was the UK principal investigator.

ASPATURIAN: Thinking back on the 1980s, I remember that there was a groundswell of public interest in astronomy in this country. You had Carl Sagan with the *Cosmos* series, and quite a large number of popular astronomy books came out. I probably was exposed to more of this because I was here at Caltech, but astronomy suddenly attracted a lot of interest. Did the same thing happen in the UK?

ELLIS: Definitely. Just at Durham, we put out several press releases on highlights from that time. One was about our team finding the first distant supernova, which led to the idea of finding additional distant supernovae and ultimately to the discovery of the accelerating universe.

ASPATURIAN: Which telescope did you find this supernova on?

ELLIS: We had a collaboration with a Danish group, so we were looking at supernovae with the Danish telescope. We found the first cosmologically distant supernova in 1987 and wrote a *Nature* paper. That in some sense was the forerunner of the [Saul] Perlmutter program at Berkeley. Perlmutter had a predecessor there called Carl [Carlton R.] Pennypacker. Pennypacker read our paper in *Nature* and came to Durham and said, "This is just great. We're going to do this with a bigger camera. Do you want to join our effort?" So I joined the supernova cosmology project in '88, '89. But that was firmly led by Berkeley, and of course it eventually led to Perlmutter's Nobel Prize. [See Session 5]

ASPATURIAN: What were some other big stories?

ELLIS: Gravitational lensing was another big thing I got involved in. One of the things we did with our faint-object spectrograph was to prove that you could use it to measure the redshifts of these distorted objects that we saw magnified through clusters of galaxies acting as cosmic lenses. We did this in '87, '88 with the William Herschel Telescope, and we had a press release on that. Perhaps the most infamous press release—which is partially correct but led to a lot of interesting debate—involved one of my students, Tom Broadhurst. We had done a redshift survey in Australia with the fiber spectrograph, and our Berkeley competitors, Koo and Kron, were using the Kitt Peak Telescope to look in the exact opposite direction. So while we were looking at what we call the south galactic pole, they were looking at the north galactic pole. They had published their data and we had our data, and what Broadhurst did was absolutely hilarious. These were pencil beams—that is, very narrow directions in the sky. He plotted a histogram of our redshifts in the southern sky, and then he plotted the Koo and Kron redshift distribution in the northern sky in the precise opposite direction. So he got a one-dimensional redshift

distribution that went from a redshift of 1 in this direction through the observer to redshift 1 in the other direction.

ASPATURIAN: Was this unprecedented?

ELLIS: Yes. And guess what he found? The distribution was periodic. From north to south, here were all these spikes that had more or less the same separation. So he came into my office and showed me this, and I put my head in my hands and I said, "You know, I can't think of any physical explanation. The universe is not periodic." So we sent this result to David Koo at Berkeley and Alex [Alexander S.] Szalay at Johns Hopkins, and they both got very excited. Szalay said, "We have to publish this, even if we don't believe that we understand what's going on. So we published it in *Nature*, in 1990 I think. *Nature* has this "News and Reviews" section, and Marc Davis, a very distinguished professor at Berkeley, reviewed it and concluded that if this result is true, we know next to nothing about the universe. Of course, it got an enormous amount of publicity, and we got all these theorists trying to explain it. One theory that I remember most amusingly suggested that the universe doesn't expand smoothly but it expands discretely. We got all these crank letters from people saying now I believe in God and all this kind of stuff. Some of my colleagues thought that I'd lost it at this point. When Wolfendale proposed me to be a fellow of the Royal Society, I remember he came back to me afterward and said that the Royal Society committee thought I was very promising but wanted to see a bit more about how this result panned out.

Now why would we find this result? Well, we now know that the universe does on large scales have a coherent scale, which we call the baryonic acoustic scale. And what we were seeing was the first hint of that result. That periodicity was in some sense a forerunner of a genuine result. But of course we didn't know it at the time.

ASPATURIAN: Can you explain a bit more in qualitative terms what was going on that produced this?

ELLIS: When you look at the microwave background, there is an acoustic scale that represents the horizon at that time. So if you think of the universe expanding from the

Big Bang, it expands until the universe is 370,000 years old and then you see the microwave sky. So you look at the microwave sky and say, "What's the maximum scale that could have been causally connected across that time?" and that's what we call the horizon. If you think of how that plasma—the radiation and the matter—behave, then there can be oscillations—sound waves are traveling in that plasma, and they have a wavelength, too. So this sound-wave horizon means that if you look at the microwave background, there is a coherent scale, and this was first discovered by Andrew Lange [Goldberger Professor of Physics, d. 2010] and others in the late 1990s. He measured it precisely with the BOOMEraNG experiment. And that sound scale is imprinted on the matter, and it expands with the universe unfettered. So it is like a rigid scale that expands with the universe.

ASPATURIAN: You're just seeing it at later times.

ELLIS: Yes, and today it would be at about 130 megaparsecs in scale. And this periodicity that we saw was 128 megaparsecs, so what we saw was an echo of the baryonic acoustic feature in the microwave background. Now the problem was that the slice that we saw looked *too convincing*. One reason people were suspicious is that we didn't have any theoretical explanation for it. If you read the *Nature* paper, we're incredibly cautious. We say that this is striking; we don't know what it means; and we're putting this out there for people to think about. I remember there was a cosmology conference in Canada where the audience was asked to vote on whether they thought our result was fundamental or a fluke. Eighty percent thought it was fundamental. So, you know, it got a lot of attention. And it turned out to be the forerunner of an important result.

ASPATURIAN: In your Royal Society autobiographical note, you mention that when you and your colleague discovered this regularity in the redshift distribution, you were surprised that it was the more creative astronomers who seemed skeptical, whereas the more rigorous ones embraced it rather readily. I wondered why you thought that happened because it is an unusual response.

ELLIS: You know I can't remember who supported it and who didn't. I know that some senior people were skeptical, but I also think the result was so striking that many people had their eyes opened. Some people may have thought, "If Richard thinks there's something in it, maybe we should take it seriously." Tom Shanks, a colleague at Durham, whom I mentioned earlier, tended to have flaky ideas, and he was dead against it. I remember thinking, "That's odd because he usually likes crazy ideas." But as an individual he tended to do the opposite of what I did anyway, because he was one of these contrarian Scotsmen. So that didn't surprise me too much. On the other hand, when Jim Peebles stood up at this Canada conference and said he thought that a result like this had to be taken seriously, I remember thinking, "Gosh, he's a member of the Establishment. I would have expected him to be more skeptical." I think I was surprised by the way people reacted.

ASPATURIAN: When was it realized exactly what this was?

ELLIS: I would say that in 2002, 2003, this scale was finally detected. We picked it up in the 2dF Galaxy Redshift Survey at the AAT in 2003, and Dan [Daniel] Eisenstein, who's now a Harvard professor, picked it up in the Sloan Digital Sky Survey. That year AAS [American Astronomical Society] had a press conference that Dan and I gave jointly about finding this feature. In his paper Dan very generously refers to the Broadhurst paper as the first to glimpse this structure. But there were rebuttal papers in between.

So the 1980s were an amazing time. I was blessed with good students, too, both in gravitational lensing and galaxy redshift surveys. It was this period when there was bags of money for facilities and instruments.

ASPATURIAN: And some extraordinary technical advances. You were so lucky in your timing as an astronomer, because fifteen years earlier or fifteen years later—

ELLIS: Fifteen years earlier, there was no telescope to use, and now in the UK we had three 4-meter telescopes. Later, there wouldn't have been enough money. Britain went into the doldrums in the second half of the 1990s.

ASPATURIAN: It's very clear what the intellectual appeal of astronomy is to you. What is the emotional draw?

ELLIS: Well, it's just fabulous fun whizzing around, going to the observatories. In the late '80s I was going to Australia maybe three times a year. What made me buzz at that time was the travel, working with young students, arriving at the observatory, being welcomed by lots of people. You know, the interaction on the mountain—"What are you observing?" "Oh, Richard, I read your paper, very exciting"—all that kind of thing; the observational community is a very friendly one. And I was going to Hawaii as well, so I would meet American astronomers at Mauna Kea. I was always interested in foreign collaboration so I was establishing a reputation in the U.S., and I was keen to have U.S. collaborators. We had a visitor program at Durham, and I invited American astronomers to come. I started collaborating on a study of distant clusters with Gus Oemler, who later was director of Carnegie, but at the time was a professor at Yale. A bit later when Hubble was launched I invited astronomers from the University of Hawaii to come to Durham. We were trying to establish global collaborations as well. So as a professor, I saw myself as an envoy for Durham whenever I went abroad.

ASPATURIAN: You're sitting up in these observatories looking out into these unimaginably vast reaches of space and time, does that affect you?

ELLIS: Yes, of course. And that's why remote observing, which is now so popular at Caltech, is something I try not to do.

ASPATURIAN: It doesn't carry the same quality?

ELLIS: No. It doesn't carry the same quality, and you don't put aside your daily duties. If you go to an observatory, you focus single-mindedly on what you're attempting to achieve, which of course makes it depressing if it's cloudy. But often we would make a discovery in real time. For example, with our periodicity result, one of the next steps was to take this pencil beam and open it out and see whether these shells continue over larger angular scales.

ASPATURIAN: Yes, I was going to ask about that.

ELLIS: Tom Broadhurst and I were at the telescope, and I said to Tom, "When I get back to Britain"—assuming we had the results—"it would be very impressive if I flew straight back with a diagram that showed them." So Tom and I reduced the data, and I flew straight back from Australia. I remember going into the lecture theater in Durham on the day I landed and giving the talk.

ASPATURIAN: With the raw data straight from the source?

ELLIS: Yes, and saying, "This is what we now see." I remember people saying this was just great—the data come straight from the telescope, and Richard comes in and gives a talk in real time. I think it was just so exciting, you could look at the data in real time, and often you would make a discovery during the night.

ASPATURIAN: Are there any other incidents like this that stand out during the 1980s, as you recall?

ELLIS: So in gravitational lenses, emission lines are the key thing to measuring the redshifts of these really thin arcs that you see in clusters**.** There was this very famous arc that puzzled people for a long time, and I remember using the faint object spectrograph on the Herschel and seeing the emission line more or less as the data came in, and thinking, "We got it. This is amazing." It was a redshift of 0.72, I think, which at the time was really far away. So we wrote this up and got a press release. Yeah, we saw that in real time. We tracked Halley's Comet with one of the spectrographs, and we managed to get the comet's spectrum in real time. With the big surveys, of course, you have to digest a lot of information and do some statistical tests, so you don't normally analyze those results until you've gathered all the data. With our survey of the clustering of the IRAS galaxies, we had to finish the survey first, which took many nights, and then produce papers after that.

So in terms of in-the-night discoveries, it's usually clinching a single object or

measuring a particular redshift of a famous object and finding out at the telescope what it is. Yes, you never lose that excitement.

Some material in this session was originally recorded during interview session 4.

RICHARD ELLIS

SESSION 4

February 6, 2014

ASPATURIAN: You mentioned last time that the two royal observatories—Greenwich and Edinburgh—had a different funding structure from astronomy programs in the universities. Where did the British universities get their money? Was it all public also, or was some from private funds?

ELLIS: It was a them-and-us situation. It came to a head when I was director of the astronomy institute in Cambridge. For years these royal observatories' staffs had privileges. They never had to raise their own money for research: It was the responsibility of the director to raise funds for the entire staff of the observatory. They could go to any conferences they wanted. They didn't have students because it was a civil-service institution, but they were very generously looked after. There was a lot of resentment and bitterness about this in the universities because we had to write grant proposals and juggle our research with our teaching. And so this model of building an instrument in partnership with a university was in some ways the beginning of the end for the royal observatories. Because once the SERC saw that universities could build instruments, they started to question the rationale for the royal observatories. And as the science became more international, why did we need a national observatory?

ASPATURIAN: Had those been rooted in royal patronage, historically?

ELLIS: Both of them, yes. The division of labor between the two was that the RGO was really responsible for optical astronomy. It championed the La Palma observatory. The ROE championed Mauna Kea—UKIRT and the James Clerk Maxwell Submillimeter Telescope [JCMT]. I got to know the people at both observatories very well.

ASPATURIAN: It sounds like they did good work, though.

ELLIS: Oh, they did. There was no question that they enabled academic astronomers to have fabulous opportunities. The same criticism of the national observatories was happening in the U.S. In about 1985 I became a member of the American Astronomical Society, and I used to get their newsletters, in which American university astronomers voiced exactly the same criticisms of the U.S. National Optical Observatory, NOAO. The attitude was, "The National Science Foundation [NSF] is strapped for cash, so why do we have to have these generously funded national observatories? The private observatories like Palomar are doing just fine."

ASPATURIAN: So, in Britain, did most of your funding also come from the government except that unlike the royal observatories you had to request it? That was the distinction?

ELLIS: Yes. In those days you would submit a specific grant application for a specific scientific program to the SERC. Later, in the '90s, they evolved into departmental grants, where the chairman of the department had the responsibility of collating all the ideas and presenting one coherent departmental grant. I experienced both procedures at Durham.

ASPATURIAN: On the topic of university and national telescopes, in 1985, Caltech and the University of California started building the Keck Telescope. I don't think that the first Keck had even seen first light when Caltech basically announced that it was going to have another Keck all to itself. Caltech had also had Palomar as a private observatory, but in an era of growing institutional and transnational collaboration in astronomy, this kind of broke the mold. Do you recall what the reaction was in the British astronomical community?

ELLIS: In 1988, as we talked about last time, I was chairman of the UK Large Telescope Panel, and of course we were well aware that California was marching off again into the sunset with the Keck telescopes. We had just managed to catch up with the 200-inch Hale Telescope at Palomar because we had the AAT, and so the motivation for a British large telescope was very much to keep up with Caltech. One of the engineers who assisted me was a guy called David Brown, and he had just retired from a company in Newcastle called Grubb Parsons, which was the manufacturer of pretty well all the

British telescopes up to that point. So he had enormous expertise in every aspect of telescopes, including mirrors and telescope structures. I remember sitting on a plane with him and discussing the Keck telescope, and he was incredibly skeptical. He didn't really imagine that the computer-controlled segmented mirror would operate smoothly. Because I trusted this guy, I remember being somewhat skeptical too. But then I came to Caltech in 1991 for a conference marking its hundredth anniversary, and the atmosphere here concerning Keck I was very much one of great anticipation, and I remember thinking, "Gosh, these guys are going to clean up," and thinking how behind, technically, we were.

ASPATURIAN: Last time you talked about near-infrared surveys that you worked on, in addition to your optical surveys. Will you talk a bit about what the advantages were of working in each of these wavelengths?

ELLIS: Right. So as you go to higher redshifts—that is, to earlier objects—the energy spectrum of a galaxy moves to the infrared.

ASPATURIAN: Because of cosmic expansion.

ELLIS: Because of the expansion of the universe. So I remember thinking that the most distant objects probably would be brighter in the infrared and therefore easier to see that way. At the time, one of the aims of measuring galactic evolution was to measure the colors of the stars as a function of time. It turns out that the bigger the baseline in color, the more accurately you could measure this evolution, and the combination of optical and infrared colors gives you this large baseline. At Durham, in the late '80s, we were in competition with Malcolm Longair, the ROE director, who had oversight of the infrared telescope UKIRT on Mauna Kea and was in charge of instrumenting it. Of course, he was a very busy man, but he had a very bright student, Simon Lilly, and the two of them were looking at radio galaxies, which were known to be very distant. Many of them had redshifts greater than 1 or even 2. But I was very skeptical about using radio galaxies as probes of cosmic evolution because they are extreme—atypical—objects. Most galaxies are not radio sources. I imagined that the sample was biased in some way. I wanted to

look at galaxies that were not radio sources, and I chose galaxies in clusters. We had a sample of distant clusters, but unfortunately they only went to redshift 1. Jim [James E.] Gunn, who at the time was at Caltech, had another sample of clusters, also out to redshift 1.

So I had a very good student from Spain, Alfonso—grand name—Aragon-Salamanca. He and I set out to measure the colors of galaxies in all of these clusters, in competition largely with Lilly and Longair. That took me to Hawaii and Mauna Kea for the first time. In those days, of course, you could only observe from the summit. UKIRT was the biggest telescope there at that time, and the Keck was under construction. I have photos of Keck being built.

ASPATURIAN: I was up for a few days in '88. It's quite amazing.

ELLIS: So I learned a lot of infrared astronomy, and then fairly soon after that I had this second terrific student, Ian Smail, and we started looking at gravitationally lensed objects. These are even more distant, and we started looking at them with UKIRT as well. We were the first really to look at what I would call typical objects that weren't radio sources at infrared wavelengths out to redshifts beyond 1. Alfonso's thesis is a beautiful piece of work. He found very clear evidence that at redshift 1 the galaxies were bluer and younger—as they should be—than they are today. It was a very nice demonstration of the rate of evolution in the stars that make up galaxies. So that was a very satisfying outcome.

ASPATURIAN: You also mention in this bio that in your first meetings with the gravitational lens community, with the exception I think of Roger Blandford and one other person, you were not terribly impressed. What was going on in that field?

ELLIS: Lensing is just an amazing phenomenon, and it's unfolded in a very interesting way. In the 1980s there had been the discovery of these distorted arcs of light and—this was before Hubble—nobody quite knew what to make of them. Were these shells of gas, or what? There was a group of astronomers—very similar to Durham's in that it was a small group, all very young—in Toulouse, France, and one of their students, Genevieve

Soucail, got a spectrum of one of these arcs with the Canada France Hawaii telescope, and proposed gravitational lensing as an explanation. I got so inspired by this; I thought, "This is just beautiful work." Then I got invited to be on the thesis committee of some of the students in Toulouse, and so I became great friends with the Toulouse group, and we invited one of their young stars, Yannick Mellier, to Durham for a couple of months. So in 1988 or '89, the Toulouse group decided to have a conference on gravitational lensing. It was a beautiful conference in Toulouse. [Roger] Blandford gave the conference summary; and that was the first time I met Roger, I think, because even though he was British, he was here at Caltech. I really liked his summary. He said that many of the gravitational lens practitioners were not very rigorous. If they saw something that was slightly distorted, they clapped their hands and said, "This is a lensed object." If they saw two objects on the sky, they said, "It's a multiple image, it's a mirage." He said, "You guys have got to wake up and be a bit more rigorous." But what I realized was that when you look through a gravitational lens, you have the capability to do two things. You can see a distant object that's magnified and hence brighter than it would otherwise be, and that's great—it's like a free telescope. And you can look at the nature of the lens itself, and the focusing properties of that lens depends on how the dark matter in it is distributed.

ASPATURIAN: Had dark matter emerged as a concept by that time? I guess the idea had originated much earlier with [Fritz] Zwicky [professor of astronomy, emeritus, d. 1974].

ELLIS: Yes. It didn't have anywhere near the prominence then that it does today. But it was obvious that these lenses were more powerful in their focusing properties than was possible just from the starlight alone. So I had this great graduate student, Ian Smail, and we decided to focus on lensing for his thesis, and that was a nice piece of work, too. And then in 1990 Hubble was launched, and I think that's when lensing really took off because in order to recognize these distortions, you need high quality imaging. Hubble did go through this rocky first couple of years with the spherical aberration in its mirror, but even in those days you could see the potential. So Smail and I found several truly amazing objects, multiply imaged. I remember I flew to Baltimore for a press conference

to describe the promise of Hubble for lensing, and we got a lot of attention. So I became a big fan of lensing after Hubble. [See also Session 8]

ASPATURIAN: Were there other projects you were involved with on Hubble??

ELLIS: Yes. One of the reasons why I was very keen to get into Hubble was the morphology of galaxies. Most of my work in the 1980s was concerned with trying to understand why there were more faint galaxies on the sky than this no-evolution model had predicted. From our spectroscopic surveys, it looked like most of these excess early galaxies were feeble little things. They're not around today, but they were there then, and we wanted to know what had happened to them. What kind of galaxies were they? Were they elliptical or spiral? And it turned out that the bulk of these very high redshift objects—the feeble ones—were very irregular little things with no clear form. So these objects had presumably merged and assembled into more regular looking galaxies like the spirals that we see today. So what we did was to count how many there were, because, for the first time with Hubble, we could resolve them into their forms. There were a few other groups working competitively in this area, but we managed to get time for a key project called the Medium Deep Survey with the Hubble Space Telescope, and so we had more data than most people. So in the early 1990s, I was doing two things with Hubble: discovering these gravitational-lensed objects and hence demonstrating the telescope's unique capabilities for making maps of dark matter; and counting these little irregular galaxies, measuring their morphologies, and trying to come up with a synergistic picture of how these systems merged to produce the bigger galaxies that we see today.

ASPATURIAN: During this period I believe you were on some type of research fellowship?

ELLIS: That's right. In those days, Britain had a scheme called the SERC Senior Research Fellowship. It's a bit like the Guggenheim. I had tried once before to get it and failed, and then the second time, in 1989, I got it. And it was just fabulous—five years of research, with no teaching. They paid your salary to the university and then the

university could hire somebody to do your teaching. It was very competitive—only three or four a year across all of science were awarded. So I was very lucky to get one.

ASPATURIAN: Was there any substantial brain drain from Britain at this time in the astronomical community? For example, Roger Blandford had come over here.

ELLIS: I think most of that was during the 1970s before this beautiful expansion in facilities. One guy came back. This was Roger Davies, a Cambridge PhD, who left in the ’70s and eventually went to the University of Arizona. When we finally got funds to design the large telescope we hoped to build for Britain**,** we looked for somebody to be the project scientist, and he was hired and came back to Oxford. So there was a bit of movement the other way. I was tempted—I mean I got a bit frustrated as it became clear that there just wasn’t enough money for one telescope of our own. I got a bit disillusioned thinking that if California was going to have the Keck, Britain’s half an 8-meter telescope was not going to be very competitive. I got a phone call from Simon White in Tucson, asking if I would be interested in a professorship at Steward Observatory at the University of Arizona. I went there and found it a fabulous department, very exciting. But Barbara said that she didn’t want to live in Tucson, and that was the end of that. I also got invited to visit Johns Hopkins. I knew Baltimore because I’d visited many times for the Hubble Space Telescope programs, and we’d lived there briefly in ’85 when I spent three months with the Space Telescope Science Institute. Barbara said Baltimore might be okay. But in the end I turned down Johns Hopkins too and stayed in Durham.

ASPATURIAN: You had written a number of very significant papers during this period. Did you like preparing papers for journals?

ELLIS: Loved it.

ASPATURIAN: Some scientists just find that an agonizing side of their profession.

ELLIS: Well, I don't like writing reviews. I've done one big review in '97, and it nearly killed me. It was awful.

ASPATURIAN: What do you mean by reviews?

ELLIS: It's a summary of the state of the art in a particular topic. There's an astronomy journal called the *Annual Review* [*Annual Review of Astronomy and Astrophysics*], and you get invited to contribute. [Allan R.] Sandage, the famous astronomer at Carnegie, invited me to write an *Annual Review* article on faint galaxies, and I thought, "Yeah, I ought to do this; it's the culmination of all the work I've done, spectroscopically and with Hubble." So I agreed to do it, and it took two years.

ASPATURIAN: Is it like a survey of developments in the field?

ELLIS: Yes. I think if you're trying to do research as well as reviewing, it's very hard to find the time. At the time I wrote this review I was director of the Institute of Astronomy in Cambridge, and so I would have to physically go somewhere else to work. I'd have to go home and surround myself with preprints and rigorously try to write. I didn't enjoy that. But I do like writing focused scientific papers, especially with students. I tend to get frustrated if we don't churn them out fast enough, actually, because it is a competitive business.

ASPATURIAN: What were some of the courses you taught at Durham?

ELLIS: So I got dumped in the deep end, because of course I'd done a degree in astronomy. Some aspects of physics, like solid-state physics, I'd never even studied. So I was always a bit fearful of being given a course that I'd never done as an undergraduate. Fortunately it never happened. When I became a lecturer, the first course I gave was atmospheric physics. And it was a short course to non-specialists. I really enjoyed it. I put a lot of effort into it. It was twelve lectures, maybe, and it covered cyclones, the jet stream, measurements of humidity, lightning, the structure of Earth's atmosphere. When I became established in the department, although still a lecturer, they gave me electricity

and magnetism. That was a mainstream physics course with over a hundred students and much more difficult to teach. I enjoyed it a lot. One of the nicest things was teaching undergraduates the origin of magnetism. In high school in those days, and probably even more so now, magnetism was taught as a force equivalent to the electric force. The idea that the two were connected by motion didn't emerge until you knew Maxwell's equations. So the idea of explaining magnetism as a form of the electric force was sort of a highlight of the course, leading to Maxwell's equations. Finally, when I came back as a professor from the RGO, I taught the final-year astrophysics course for physicists. That was a big class, and I had to write the notes myself. I didn't think the notes from the previous guy were very clear, so I rewrote that course. We were always doing practical classes [i.e., labs] as well. We did tutorials, which were usually made up of six to eight students, and we'd go through the set homework. There was also a final-year project where students were given a research topic and had to make a presentation. It's not as intensive as a senior thesis at Caltech, but it was something similar. So I managed that as well. At Durham I taught more than I taught anywhere else, including Caltech. It was quite an ordeal. I remember during the start of the summer marking over one hundred exam papers. You'd go away for three days and just bury yourself in correcting all of these exams. It was a lot of work.

ASPATURIAN: And this was going on along with your traveling, your research, and your graduate students?

ELLIS: Yes. The atmosphere at Durham was one in which teaching was regarded as very important. In those days, in a provincial university like Durham, there were many people who weren't doing research, and so they were somewhat critical of the astronomers whizzing around to telescopes. I had to explain to people that if I got observing time I had to leave even if it was in the middle of term, and that was quite an uphill battle. Once I became a professor, I had enough authority that people didn't question it, but to begin with, there was this suspicion that we were off on the beach somewhere. Especially when it was Hawaii.

ASPATURIAN: Of course, of course.

ELLIS: "Richard's got to go away next week." "Oh, why?" "He's going to Hawaii." "Give me a break." I had to explain, "Well, look, sonny, you've got your laboratory down the hall. My laboratory happens to be a telescope in Hawaii, and that's just the way it is."

ASPATURIAN: Just because I had the foresight to go into astrophysics and you didn't?

ELLIS: Exactly. [Laughter] Yes.

ASPATURIAN: So now we're in the early '90s. You've been involved in two important studies on the Hubble. What else were you doing at this time?

ELLIS: Well, because I had the Senior Fellowship, I was free to travel, and I very much liked coming to California, and by then I knew people at Caltech. In fact, instead of going to Australia via the direct route—a 24-hour flight—I started stopping overnight at Caltech on my way there. And Wal Sargent looked after me, often contributed to my airfare, and put me up at the Athenaeum [the Caltech faculty club]**,** and the next day I would fly on to Australia. I started feeling that I really enjoyed Caltech, and I began to get invited as a regular visitor. I gave colloquia at Caltech and explained some of the instruments. I remember when Caltech was interested in building a fiber spectrometer, they asked me to come out and give a talk.

ASPATURIAN: Is this the Norris Spectrograph?

ELLIS: The Norris Spectrograph, yes.

ASPATURIAN: I wrote the press release about it, which is why I remember.

ELLIS: After my student Ian Smail got a fellowship here, I started becoming much more familiar with Caltech. In 1992 I came on a visit, and I remember distinctly Wal Sargent coming to coffee one morning and saying to me that Martin Rees had moved out of his

Plumian Chair at Cambridge. Now at the time my former student, George Efstathiou, who I think we talked about, had got the Savilian Chair at Oxford.

ASPATURIAN: Ah, he'd come back from Berkeley.

ELLIS: Yes. He was very young when he got that chair—like thirty-two or something. So I remember thinking, "Wow, an opportunity to go to Cambridge." I was looking for a signal from Wal Sargent as to whether he thought I should apply. I didn't ask him, because I thought, "If I asked him and he says 'no,' I'm going to be hurt." And then of course, I thought that Wal, who was still British and always missed Britain—we often talked about football [i.e. soccer] and stuff like that—might have applied. So, what happened was that George urged the director of the astronomy institute at Cambridge, Donald Lynden-Bell, who was involved in the search for Martin's successor, to call me. Donald phoned me up and said, "You should apply because we need an observer." Cambridge had two chairs [i.e., professorships]: Lynden-Bell, who is ostensibly a theorist, although he was clearly interested in observation, had one, and Martin Rees, an out-and-out theorist who didn't ever go to telescopes, had the other, which was the Plumian Chair. And it turned out that Donald Lynden-Bell was somewhat embarrassed in that it had been expected he would be an observer when really he wasn't. He didn't build instruments, he didn't do observations. I remember his confiding in me later that he always felt that he'd let Cambridge down because he'd never done much observational astronomy. And—I'm pretty sure about this—Wal had applied for that chair [i.e., professorship] when the previous holder, [Roderick Oliver] Redman, who was also director of the Cambridge Observatory, died very young. Wal was the obvious person to come back from California to this observational chair in Cambridge, but he didn't get it.

ASPATURIAN: How come? Politics?

ELLIS: No idea. So Lynden-Bell had this burden on his shoulders for years of thinking that he had denied Wal this chair when he himself wasn't truly a respectable observational astronomer. He confided in me that when Martin moved out of his chair, here was an opportunity to mend this mistake and get a real bona fide observational

astronomer into this chair, and would I please apply? So I applied, and I didn't hear anything for about a year. And everybody knew why; it was because Roger Blandford had been headhunted for this chair. At the time he was at Caltech and although he wasn't an observational astronomer either, he had been invited to apply for pretty well every chair that ever came up in Britain in the '80s and '90s. When I got to Cambridge, I tried to get him back as well. So basically we all knew that Roger had been offered this chair, and I tried to put the whole thing out of my mind. So then I distinctly remember being at home at eight o'clock one morning when the phone rang. I picked up the phone, and it was the vice chancellor of Cambridge saying that I'd been offered the Plumian Chair. Barbara was adamant that we should go, but I was a bit torn. I had all this empire at Durham. It's not as if I was unhappy at Durham.

ASPATURIAN: And they'd been very good to you there.

ELLIS: Yes, they'd been very good to me. And of course Wolfendale was devastated. But we'd been in Durham nineteen years. The children were now in their teens—Tom would have been fifteen, and Hilary would have been seventeen, in what we call the sixth form, which is sort of pre-university—and they were old enough to have an opinion about it too. So we went. It all worked out very well.

ASPATURIAN: It would have been very hard to pass up, I think, a named chair with very ancient antecedents at Cambridge. I wanted to ask, you talked about coming to Caltech rather frequently. What were your impressions of the Caltech astronomers?

ELLIS: Great. Obviously, they were all different personalities. Jeremy Mould was here at the time, and I got on very well with him. A pleasant person, who was always interested in what I was doing. Wal had a great sense of humor and was a true Brit through and through. Anneila Sargent [Bowen Professor of Astronomy] was very nice to me. Judy [Judith] Cohen [Page Professor of Astronomy] was going through some medical crisis. I remember she had some horrible problem with her hands. [Stanislaw "George"] Djorgovski [professor of astronomy] was here. Peculiarly, I never really met Shri [Shrinivas] Kulkarni [MacArthur Professor of Astronomy and Planetary Science] at

the time. He was here, but either we never crossed paths or I don't remember Shri from those days. Nick [Nicholas Z.] Scoville [Moseley Professor of Astronomy] was here. He was great. So I found it friendly. The Robinson [Laboratory of Astrophysics] building was great.

ASPATURIAN: Did it ever cross your mind that you might enjoy joining the faculty?

ELLIS: Yes. I remember thinking, "Wouldn't it be great to be here?" The climate was great.

ASPATURIAN: Not to mention those telescopes.

ELLIS: Well, the Keck hadn't started yet, so it was the 200-inch. I remember that once a postdoc who was a big fan of the history of the 200-inch met me at LAX and drove me to Palomar and gave me a tour. That was so impressive. What a place. There was also Carnegie. Sometimes when I came through, I was invited to give a talk there. I was big friends with Alan Dressler. When I moved to Cambridge, he and I started collaborating quite a lot—oh, there's an interesting twist. Later, when I was at Cambridge, maybe in 1997, I applied for the Carnegie directorship. Leonard Searle had retired, and there was an advert for his successor. Obviously since I was director at Cambridge, I had to keep it under wraps, so I contacted Wendy Freedman [now director of the Carnegie Astronomical Observatories] and asked if she thought I was the kind of person who should apply. And she said, yes, and so I applied, and I didn't hear anything at all. Then one day I got a letter from the president of the Carnegie Foundation, who must have been—it was this woman.

ASPATURIAN: Was it Maxine Singer?

ELLIS: Maxine Singer.

ASPATURIAN: The biologist.

ELLIS: It just said, "Thank you for your application, an appointment has been made."

ASPATURIAN: Well, that's warm and fuzzy, isn't it?

ELLIS: Yes, so I didn't get very far with that. But back to Pasadena in the early '90s. Yes, I often thought this would be a fabulous move, but I was just at Durham. It seemed too far away at the time, I think.

ASPATURIAN: What was the reaction at Durham to your imminent departure?

ELLIS: They were very pleasant. The young people all sent messages that they were going to miss me. Carlos Frenk wrote me an e-mail, or maybe he said it at afternoon tea to everybody. At any rate, he said, "Richard's move is bad for Durham, bad for Richard, and bad for Cambridge." [Laughter] Looking back, I think it was a great move. I probably wouldn't have got to Caltech directly from Durham, because it was at Cambridge that I gained the experience, which really molded me, of managing all of these difficult people, which was an excellent training for coming to Caltech. [Laughter]

ASPATURIAN: You've mentioned a number of graduate students. Before we leave Durham, would you like to mention where some of them went, and how their careers developed?

ELLIS: I probably had about eight to ten students at Durham, and they've all done very well. Only one of them went into a nonacademic profession. My second or third student, Iain Inglis went into computing—a company called Logica [now owned by CGI], which is quite a famous company in Europe that does commercial software, often for the aerospace industry. I lost contact with him. My first graduate student, David Axon—the one who rescued me from this polarimeter guy, Scarrott—has just passed away, sadly. He became a professor at the University of Rochester and then he became the dean of maths and physics at Sussex University. I would say one of the stars of the later Durham period was Alfonso Aragón-Salamanca, the guy I did the evolution of galaxies with at UKIRT. He's a professor at Nottingham University. And then Ian Smail, who

introduced me to gravitational lensing, came to Caltech, where he did pretty well, and then he was offered a staff position at Carnegie. This was at the time I was joining Caltech, and I dearly hoped that he would be at Carnegie while I was at Caltech, but his wife didn't want to live in the U.S., so he went back to Durham, of all places. I think you should never go back to where you were a student, although I did it once, when I went back to Oxford a few years ago. But he's now head of the astronomy group at Durham, so he's risen to the top there. He's pretty prolific. Richard Bower was a student at Durham who went to Edinburgh and Germany, but then he also came back to Durham. Durham, in my opinion, is too inward looking. It tends to hire its old boys and to promote its own people. I've told them this—that they ought to really stir the pot a little and get external people in. I was always welcoming of foreign students. I had another Spanish student, Francisco Castander, who went back to Spain as a professor. And then there's Bahram Mobasher, who's a very interesting guy. He came from Iran, and was one of my early students. Durham had this tradition of taking people from the Middle East, and it became a staple academic route for students from Iraq and Iran. One day I was given the paperwork about Bahram, and he had very high test scores from Tehran. I said, "Well, what do these scores mean?" I had no idea, so I said, "Let's accept him for a master's degree." He came and he did a master's degree on the Hubble constant, like a literature review. For his PhD we decided to put him on an infrared survey of nearby galaxies with UKIRT, which involved measuring the infrared brightness of lots of nearby galaxies and constructing the first catalog of them. Well, blow me down, this became a classic paper. It was the first genuine catalog of infrared luminosity distribution. He's now a professor at UC Riverside.

So I think that's pretty well it from Durham. Most of the students came from Oxford or Cambridge, and they've all done well. Many of them are full professors.

ASPATURIAN: Has Durham maintained its reputation in astronomy?

ELLIS: Yes. Increased it, in fact. We hired Carlos Frenk when I became a professor at Durham. He's become a media star and a big figure in theoretical cosmology. He's raised a lot of money for Durham, hired a lot of people, and created an institute of

computational cosmology. He's very famous in Britain, and he's kept the flag flying at Durham.

ASPATURIAN: It sounds like they've broken new ground.

ELLIS: Yeah, they have.

ASPATURIAN: This may sound a little out of left field, but I think it's interesting. When I did an oral history with Seymour Benzer [Boswell Professor of Neuroscience, Emeritus, d. 2007], he talked about the year he'd spent at Cambridge as a young molecular biologist [*Interview with Seymour Benzer*, Caltech Oral Histories, 2002; http://oralhistories.library.caltech.edu/27/1/OH_Benzer_S.pdf]. He was there right around the time Watson and Crick were discovering the structure of the double helix. He said it was the most amazing thing: the Brits never seemed to do any work. They were always having tea, always playing tennis. They were in the pubs, and yet—

ELLIS: They got it done.

ASPATURIAN: They got it done. He said he never quite figured out how that worked.

ELLIS: Well, the interaction at Durham was fabulous. Everybody knew what everybody was doing because people talked to one another. Whereas in the United States, it's been very hard, even at Caltech, to get people to mingle and talk because they think it's not a valuable way to spend your time. Better to be in your room flat-out working, writing papers and everything. So Britain had this relaxed attitude. Both Cambridge and Durham were fabulous in this regard. I think that's the secret of the success.

Some of the material in this session was originally recorded during interview session 5.

RICHARD ELLIS

SESSION 5

February 10, 2014

ASPATURIAN: I wanted to ask you, based on your years and years in the field. What do you think makes an outstanding as opposed to a merely competent observational astronomer?

ELLIS: Ultimately, a choice of good projects. Some technical ability with the instruments, because they don't always perform, so knowing when data is not up to scratch in real time at the telescope. Sometimes I've seen people blindly carrying on gathering data and then they go home and look at it, and it's not up to snuff. But I think, ultimately, the key is creating new ideas. It's disappointing how many people are following others' ideas—what we call "me-too astronomy." The thinking goes, "I can do that, and I have more time, so therefore I can do it better." The people I admire are people who have a completely new idea and are able to match that idea to the new capability of an instrument. It's a bit depressing how many people are chasing the same science projects at the moment—even on Keck, because the time is split between many communities. You get to the telescope, and you find that lots of people are chasing the same objects and just trying to get there first.

ASPATURIAN: Do you think it's partly because it's harder these days to get funding for truly original projects if it's not clear what the payoff will be?

ELLIS: Well, there's some truth to the idea that people are trying to get instant results so they can advance their careers. The competition, especially for the postdocs, to do something that has high impact is great. But I don't think you need funding to come up with new ideas. It's the spark of originality that does that.

ASPATURIAN: Were there any women that you worked with at Durham?

ELLIS: Not at Durham. I had two female PhD students when I got to Cambridge. Cambridge typically was much more concerned about the genders. The first time I became aware of the gender balance thing was when I moved to Cambridge. It didn't occur to me in the 1980s in Durham, sorry to say. I think this whole issue of women in science really unfolded in the '90s, and then became a big thing. Of course, at Caltech I'm acutely aware of it. But in Durham, I don't remember even interviewing female applicants, to be frank. There were female undergraduates that I taught, and they were doing projects under my supervision. There were a few female graduate students in Durham, but they were really few and far between. Maybe ten percent or less.

ASPATURIAN: So you moved to Cambridge in 1993. Can you talk for a moment about the significance of the Plumian Chair?

ELLIS: So there are two famous chairs in Cambridge that date from around 1700. One is the Lucasian Chair, which is the chair of cosmology and mathematics that Stephen Hawking held until recently. Everybody's heard of that; that was Newton's chair. At the time Newton was trying to test his theory of gravitation using the positions of objects in the solar system, and he wanted accurate measurements of the positions of the planets. He tried to get these from [John] Flamsteed, who was the first Astronomer Royal, but Flamsteed was either being difficult or he genuinely didn't want to release observations that weren't yet completely verified and accurate. Of course I'm no historian, but those are basically the two versions of this story. But Newton was quite a nasty piece of work. He went to [Edmund] Halley [the discoverer of Halley's Comet] and asked Halley's assistance in getting the measurements, and it almost got to the point where Newton had more authority than the Astronomer Royal and could command the release of these observations. Newton was so frustrated with the Royal Greenwich Observatory that he decided that the only solution was to create a Cambridge University Observatory, for which there had to be a professor. So the Plumian Chair was funded by Thomas Plume, who was a landowner in East Anglia; there's a Plume Farm, which I've visited. He gave money to create the Plumian Professorship of Astronomy and Experimental Philosophy. When I got this chair, I was given the piece of paper that was produced by the university

when they originally announced it. I can't remember what the endowment figure is, but it was only a small amount of pounds to endow the chair and "to provide funds for a boy or two to assist the professor"!

ASPATURIAN: That's interesting language, isn't it?

ELLIS: Yes. "A boy or two to assist the professor in his observations." I hate to say, I've forgotten the first holder of this chair [Roger Cotes, 1707–1716], but he was Newton's appointment—somebody who'd worked with him—and he was a disaster. He created an observatory on top of the entrance to Trinity College. If you go there, it's a grand entrance with a tower with two little turrets on the top and an archway into the college. Newton's rooms were to the right as you face the entrance of Trinity College, and you can see there's an apple tree and tourists taking photos of it today. After ten years, the equipment that he purchased was derelict, and no serious observations had been done, so the Plumian Chair got off to a terrible start. But arguably in observational astronomy, it is the most important chair. The only comparable one is the Savilian Professor of Astronomy at Oxford, which George Efstathiou was sitting in. So there we go.

ASPATURIAN: Martin Rees had occupied that chair for a very long time.

ELLIS: Since 1971.

ASPATURIAN: Why did he give it up?

ELLIS: There's a story here. There was a very distinguished astronomer at Cambridge, Simon White, who was a graduate student of Lynden-Bell's, and then became a postdoc at Berkeley. He's a terrific scientist, and everybody believed he would get a faculty position at Berkeley. But to everyone's amazement—and this is probably in the 1980s—he was beaten to it by a woman, and so he ended up as a faculty member at Steward Observatory in Arizona.

ASPATURIAN: Who got the job?

ELLIS: Imke de Pater. She's chair of the department now. So when Simon White eventually decided to come back to Cambridge, he came back as what we call a reader, which is—there's no U.S. equivalent—higher in rank than an associate professor. It's basically a position that is regarded as being in orbit, waiting for a chair. So he was a huge addition to Cambridge, and my hypothesis is that Martin Rees wanted Simon White to become the Plumian Professor, and so he moved sideways into a Royal Society Professorship, which become available very infrequently. They are the ultimate position, funded by the government through the Royal Society. You don't have to do any administration; in fact, you're forbidden to do any administration. You're given resources for five to ten years to do research. So Martin, who must have been close to fifty, must have thought, "I've had enough of all this administration, directing the institute." I don't know this for sure, and I've never asked him, but my feeling is that he moved sideways to create a vacancy for Simon. But the way Cambridge works is they have a permanent committee to select this chair-holder. Its membership may rotate, but the previous holder is not on the committee, and so, although Martin could write letters for Simon, he was not on the committee itself. Lynden-Bell, who was the director of the institute at the time, was on this committee, and he subsequently told me that Simon White did apply, and that Roger Blandford was ranked No. 1, I was ranked No. 2, and Simon White was ranked No. 3, but that if Blandford and I declined, the committee would meet again. So Roger declined, and I was offered the position and accepted. In my negotiations with the university—and I want this to be on record because not many people know it—I wrote to the university and asked for Simon White to be given a chair [i.e., a professorship]. I knew that if I took the Plumian Chair, and he didn't get promoted, he would leave. We were good friends, but I didn't discuss this with him, and I don't even know if he realizes I did this. Anyway Lynden-Bell wrote to me and said, "I will help you in your negotiations with the university." We're not talking big startups—Cambridge didn't do big startups in those days. So I think I got 30,000 pounds total, moving expenses, and permission to proceed with a vacancy in instrumentation; and I think that was about it. But when I made this case for Simon White, Lynden-Bell phoned me up and said, "It's very noble of you to try to get Simon promoted, but the university will not accept external influences like this. That's not the way Cambridge works.

There's an enormous long list of readers waiting for chairs, and if one could be promoted by somebody from Durham, there'd be a riot." So, sadly, I didn't get Simon promoted and when I turned up at Cambridge, there was an evident tension, which made for a quite difficult start.

When I was hired Lynden-Bell had said, "It's inevitable that you will eventually be director of the Institute of Astronomy. Are you comfortable with that?" And I said yes. But when I arrived, I detected a mixed atmosphere. Martin was not visibly defensive or difficult or anything, but you don't have to penetrate very far in these personalities to realize that there's something going on. Several people said, "Well, you know, Richard got the chair instead of Simon; therefore this means that Simon is going to leave." And pretty soon, I could sense that people were even blaming me for Simon's departure, even though I tried to keep him. So the first thing that happened after I arrived was that Lynden-Bell said that he was not going to be the astronomy institute director next year, and that therefore the institute needed to appoint its new director and that it was obviously going to be me. The faculty met, and I was appointed director by a majority vote. And I could sense that Martin clearly wanted Simon to be director, but Simon wasn't a professor. There were e-mails going 'round, which I saw, that indicated that although it was unprecedented for a reader to become director of the institute, the statutes permitted it, or if not, all the statutes could be changed. I remember thinking that this was really a bit of an insult to me. But in the end, it all panned out. I became director, and Simon White left to become a Max Planck director. He's done supremely well there. Some years later when he was encouraged to apply for the Plumian Chair which became vacant, he turned it down.

ASPATURIAN: I wanted to ask something about how academia works in the UK. Is every professorship there an endowed chair?

ELLIS: It was. In the good old days, in the '80s and early '90s, to become a full professor, was exceptional, unlike in the United States. And many of the professorships were named and endowed chairs. Sometimes as you traveled around the UK, there was only one professor in a university's entire astronomy department. That's all changed

because the salaries are so pitifully low. The only way that you can give people a decent salary is to promote them to professor. So now it's expected that as long as you're productive you will eventually become a professor, much like in the U.S.

ASPATURIAN: So what was the move from Durham, which is a northeastern city—much closer to Edinburgh, I think, than to London—like? You moved way down south.

ELLIS: Oh, it was great. Barbara loved it. She and the children were happy. We got a fabulous family house. We were very lucky: The housing market collapsed in 1993, so we were able to move from a house that was very large in Durham to a large house in Cambridge, despite the fact that the housing prices are traditionally much higher in the south of England than in the north. But at that particular time, they were nearly equal. It's so much warmer in the south. The summers are hot. You could cycle around—it's a cycling city, of course. The children went to high school and slotted in very well. You're close to London, fifty minutes away. So suddenly we were in a much more civilized part of Britain. Durham's a beautiful town, but it's very small, and you struggle to find things in the stores that you need. You had to drive to Newcastle to get clothes and things like that. Cambridge was a well-stocked city and a beautiful part of the countryside. The university has a very vibrant atmosphere academically. And Donald was very good at helping me find a college. Do you want to go through the college story? It's hilarious.

ASPATURIAN: Absolutely.

ELLIS: Most Cambridge academics elect to be members of a college, which in Cambridge are essentially undergraduate teaching centers. But professors in those days were so rare that they didn't teach. So for a professor, the college is really a social center—beautiful old buildings and a dining club where you can entertain visitors and people in the department. It's a way of meeting people from all over. It's a bit like the Athenaeum, but it's much better organized because you're forced to sit next to people on high table who are in different disciplines, and it encourages an atmosphere of sharing knowledge across different areas. So very quickly you meet people. So how does a professor get

assigned to a college? Well, the colleges all have different wealths—Trinity College is the wealthiest. And to avoid it accreting all the professors, every college has a quota of professorships. When I arrived, I was given a list of the colleges that had vacancies, and one by one I was invited to dine at these colleges. They were testing you as well as you testing them, just as when you go for any position. And I'll never forget—I went to dine at Gonville and Caius, which was Stephen Hawking's college. I behaved quite naturally; I had a lovely evening; I talked about my work, but clearly I didn't pass muster, because at the end of the evening, the master of the college leaned over to me and said, "Very nice to meet you, Professor Ellis. I do hope you find a college." [Laugher] It was clear that I didn't get through that one. Eventually I ended up at Magdalene College, which is one of the oldest colleges—it's 1480, I think. It's where Roger Blandford was a graduate student. They were very nice to Barbara and myself. It was rather stuffy, but in many ways, it was what people wanted to see— very old fashioned and traditional. It has formal dining every night in a beautiful hall without electricity, all candles.

So I started going there regularly. Of course I met everybody at the college. I was the only astronomer there. What you realized about Cambridge on arrival from another university was how insular it was. Most of the faculty had been undergraduates and graduates there. They never left. They stayed there all their academic lives. They had no idea what it was like in any other university.

ASPATURIAN: They were in a bubble.

ELLIS: They were in an amazing bubble. But it's a charming city. It's a fabulous university, and there are lots of very intelligent people and a great atmosphere of discovery. Of course, it's bigger than Caltech, and in many ways it's broader. When I arrived, I realized, "Wow, I've really made it. I've come to Cambridge as the Plumian Professor." Then I became director of the institute, and it's a huge responsibility. I'd sort of run things in Durham but I'd never had to deal with such high-powered people. I was responsible for dealing with them, and getting on with them, and seeing their point of view. I was learning a huge amount about a complex university with all its amazing committee structure. If I wanted a new position in the astronomy institute—say

somebody retired or needed to be promoted—I had to navigate all of these committees. I had to marshal the arguments and then go before these committees and be eloquent. I had to be as powerful as people who'd been there for decades.

ASPATURIAN: It sounds like Durham had a much flatter organizational structure.

ELLIS: I think Durham was not so formidable, and once I became a professor at Durham, whatever I said, people gave me room. But in Cambridge, everybody was important. I remember going to one university committee meeting and saying that my plans for a particular vacancy in astronomy were very important because, frankly, we had the best astronomy institute in Europe. I remember this guy leaning over to me and saying, "So what; we've got the best chemistry institute in Europe." I thought, "Oh, yeah, right, you probably have." [Laughter]

ASPATURIAN: Did Cambridge have an independent, autonomous astronomy department? It was not embedded in physics?

ELLIS: It was not in physics, and that was another big change for me. The Institute of Astronomy was independent of the physics department—in fact, across the road from it.

ASPATURIAN: What was Cambridge's reputation in astronomy vis-a-vis Oxford?

ELLIS: Oh, much stronger. I have to be careful here. Until George arrived at Oxford, it was very traditional.

ASPATURIAN: Is this George Efstathiou?

ELLIS: Yes. The previous Savilian Professor had been my supervisor, Donald Blackwell, and he held the chair until retirement. He never really had vision, so all was lost in this world of stellar astronomy and measuring these transition probabilities in the laboratory. So Oxford had a hard time rising into this explosion of the Anglo-Australian telescope, UKIRT, and the birth of the Canary Islands observatories. There were a few good people

there, but they didn't keep them. And then George arrived, and finally Oxford moved into the modern era. Whereas Cambridge—c'mon, they had Martin Rees, the most famous astronomer in the UK, and Donald Lynden-Bell, and they had hired a lot of good people in the '80s. So when I arrived there, the place was very strong.

ASPATURIAN: I wanted to ask you about Martin Rees. He came to Caltech many, many years ago and gave a lecture, which I went to, and I remember saying to someone as I walked out, "Well, that was a great lecture, but I couldn't tell if it was about astronomy or about Martin Rees." What was he like as a personality?

ELLIS: Well, he's very personable and can be quite charming. But if things don't go his way, he can be very difficult. We had many, many battles. He was very upset when I moved to Caltech: In his words, I "defected." At various times, I tried to go back to England, and he's been extremely welcoming and then he's washed his hands when I changed my mind and came back to Caltech. So we have a love-hate relationship. But when he's talking about his science, he's a fabulous expositor. He's still without question the most visible academic in astronomy in the UK, even though he's now seventy-one.

ASPATURIAN: What were you doing your first couple of years in Cambridge?

ELLIS: So I had a vision. Hubble had been launched and was now repaired. I started trying to build an instrumentation group at Cambridge, which was much harder than at Durham, where there were lots of people waiting to work in instrumentation. When I left Durham, we had quite a vibrant instrument group. We had good contacts with the RGO. Let's separate my research from my administrative duties. In my research I was very keen on gravitational lensing. I had started working on the supernova cosmology project with Saul Perlmutter, which we can talk about. And I was doing galaxy surveys with the Hubble Space Telescope. Administratively, I was trying to set up an instrument group. I had a vacancy, which I eventually filled with Ian Parry whom we discussed earlier, and we started raising money privately for instrument initiatives.

ASPATURIAN: Was this a new feature of your academic life?

ELLIS: It was. Through this fundraising I met the Sacklers—Raymond and Beverly. I got on very well with them, and we raised significant funds from them, first for instrumentation and then eventually for a lecture theater.

ASPATURIAN: The Sacklers, for the record, are a New York family, who I believe made a fortune in medical diagnostics?

ELLIS: Pharmaceuticals, actually. What else? We were being forced as an institute to do undergraduate teaching—originally it was set up by Fred Hoyle as a theoretical institute that had no teaching obligations. But the university was adamant that there'd be no free lunch, so we started doing undergraduate teaching. Then there were positions that were becoming vacant, and we made several female appointments to those positions. Those women are still there. So slowly the institute had to raise money. We had to get grants from the Science and Engineering Research Council, SERC, and I was in charge of raising those funds. Midway through my directorship, there was a bombshell—often these things come. The RGO, which I had been a member of in the '80s [Session 3], had meanwhile moved from its castle in Sussex to Cambridge, right next to the institute of astronomy. That had been Martin Rees' achievement; and I think it's one reason why Martin, when I arrived, thought that I should not be director of the astronomy institute. He wanted me to somehow forge a strong connection with the RGO.

ASPATURIAN: He wanted you to be the liaison?

ELLIS: Yes. I remember him coming to me and saying, "Why are you building instruments?" and I said, "Well, this is what I did at Durham, and I think it's good to have an instrument group in an academic department." He said, "Well, we've got the Royal Greenwich Observatory right next door. They do nothing but build instruments. Why not use those efforts?" I said, "I will, but basically, the institute should be in charge of it, whereas the Royal Greenwich Observatory is a national entity." He had a very strange view. I think he wanted me to sort of move sideways to be the director of the

RGO or something. Anyway in 1996, the SERC had finally decided that this antiquated idea of two royal observatories, one in Scotland and one in Cambridge, was no longer financially tenable, so they decided they were going to close the Royal Greenwich Observatory, which Martin had now brought to Cambridge.

Well, frankly, the RGO had some very good scientists who didn't have any teaching duties, but it was also true that it was a somewhat rundown place. And, partly through my efforts, instrument groups had been demonstrated to work more cheaply in universities than in this national center. The head of the SERC at the time in astronomy was a man called Ken [Kenneth] Pounds, and he was absolutely adamant that they were going to close the RGO and combine it with the one in Edinburgh. So there were various committees set up to determine what should happen. They recommended—and I wasn't on those committees—consolidating these two royal observatories into one, probably at Edinburgh. Martin Rees was livid, and of course he came to me as the director of the institute, and said, "You've got to do something about this." I went to see the vice chancellor of the university, and said, "What's the university's position?" The vice chancellor at the time was Sir Alec Broers, and we got on very well. And he said, "Well, we've got to be very careful here because if the government isn't willing to support this royal observatory, then the university certainly isn't." So I decided to have a faculty meeting, and I said, "Let's hear views. What do we want to do about the demise of the RGO?" Most people were not interested in saving it. One very famous astronomy professor, Andy [Andrew C.] Fabian, said, "It's not an observatory, it's not at Greenwich, and it's not royal." [Laughter] Martin basically got very upset. He was in full flight, writing to MPs; he was on the BBC, saying this is a travesty, three hundred years of tradition. He was doing his job as Astronomer Royal, whereas I was taking the party line from the Cambridge faculty. And in the end, it closed down. What we managed to do—and I'm quite proud of this—is we saved the jobs of probably five or six people. I managed to raise funds to bring all of the distinguished astronomers who were there onto the faculty of the institute.

ASPATURIAN: So they were spared the horror of moving to Edinburgh?

ELLIS: Or of actually being made redundant in some cases. Martin even fought me on that one, which I never understood. So there are people now in the institute, including one Fellow of the Royal Society, who were at the RGO, but many of the engineers moved away and did other stuff.

ASPATURIAN: Was that the largest administrative crisis of your career there?

ELLIS: Yes. That was the biggest headache because the relationship with Martin got very tense. But I was supported by everybody else. At that time, I think I began to think, "Well, when I'm no longer director, which would be 1999, what should I do? Am I going to go back to being a professor in the institute?" By now, finances in Britain were really beginning to go downhill. The golden period in astronomy was over. The U.S. looked very attractive, and I was forty-eight, and Hilary was already at university; my son Tom was in his final year at high school, and we'd always loved America. So it became quite exciting to think about moving on.

ASPATURIAN: Did you teach much at Cambridge?

ELLIS: Yes. We set up this undergraduate course, and I taught cosmology to undergraduates, which was quite fun. Classes were quite small, maybe twenty students. And I taught graduate classes as well. The one novelty was you had to do tutorials. So what is a Cambridge tutorial? In a Cambridge tutorial, the class is divided into groups of two students each, and those two students meet with a tutor or a professor who goes through the homework in an hour. Of course it's outrageously expensive in time and effort, because imagine how many times you have to do this. But the rules were very clear: You couldn't tutor more than two students because otherwise they wouldn't get the personal attention of a professor. It's the same at Oxford. This was a novelty for me to have to a lot of tutorial work, several times a week, in addition to running the place. But my time at Cambridge was very happy apart from these administrative storms every now and again. Scientifically, it was great. I had great graduate students, and the instrumentation effort was good. I think people respected me when I raised private money. This was something that the institute hadn't done before. I had a very good

departmental administrator, and this is something I think we miss at Caltech a lot. A guy called Paul Aslin. He was completely unflappable, and he and I effectively ran the institute. In terms of administrative duties, I could focus on the vision and the effort, writing the proposals, and he would do everything that ran on a day-to-day basis. He organized the secretaries, handled allocation of rooms, organized events, and so on. We had lots of events. We had a big party for Fred Hoyle; we entertained the Sacklers. It was just a great time, working with this administrator. We don't have that in astronomy at Caltech, although I know some departments have it. In Cahill [Cahill Center for Astronomy and Astrophysics] it's a disaster; we just don't have a single point person. It's so obviously cost effective to appoint one senior administrator that takes the burden. I see it in my daily life at Caltech: professors running around like headless chickens, writing letters and organizing meetings, instead of giving the authority to one person to do this, somebody who has authority over the secretaries, who can commandeer them to fulfill some organizational task.

ASPATURIAN: What were the programmatic emphases of the Cambridge institute?

ELLIS: It was primarily strong in theory: compact objects, accretion theory—binary systems and black holes—X-ray emission. There was a strong galactic group, and one of the big discoveries while I was there—by a guy called Gerry [Gerard] Gilmore—was of the Sagittarius dwarf galaxy, which is colliding with the Milky Way. It was a department of about a hundred-and-fifty people, including maybe twenty faculty as well as some college lecturers. Some of them were on the university payroll, some on college payrolls. There were loads of students, all of high power, all very qualified. We had fifty postdocs—that's an enormous number—from all over the world. So it was a very vibrant place. What other areas? I brought extragalactic astronomy there. There was one guy working on quasars. There was a strong x-ray group—Andy Fabian was an internationally known x-ray astronomer. But as I say, theory was particularly strong, and this was something I had to get accustomed to. I had never really worked with theorists in Durham—it was a very empirical place. But suddenly I was in mathematics land, and I had to respect these people, understand what they were trying to achieve, help them.

ASPATURIAN: Who were your leading theorists?

ELLIS: Lynden-Bell was probably the most distinguished. There was Jim [James] Pringle, whom I eventually got promoted to a professor. There was Peter Eggleton, a stellar theorist who eventually moved to the U.S. Douglas Gough was a solar astronomer, but a very powerful theorist. Martin Rees, of course, was still active, now that he was a Royal Society Professor. He started writing his books, and that's when he started getting very famous, with his books. We also raised money for new wings for the institute building. It was originally designed by Fred Hoyle, but it got expanded. We got this lecture theater from Raymond Sackler. It's an interesting story, actually, the funding with Raymond. It started with Martin Rees. He was keen on a project called the Cambridge-Cambridge telescope—a 4-meter telescope, just for Harvard and Cambridge, in Chile. Originally we asked Raymond and Beverly for the funds for the telescope. We needed eight or ten million pounds. Sir David Williams, the vice chancellor at the time, got the Sacklers into his office and said, "Richard wants to realize this telescope," and they said, no. Ten million pounds was too much.

ASPATURIAN: How much would that have been in American dollars?

ELLIS: About $16 million at that time. I had tried to raise the money. First I went to Harvard in 1995, and Bob Kirshner and I agreed to court the Sacklers. We invited them to witness a night of supernovae observing at the Multiple Mirror Telescope at Mt. Hopkins, Arizona. Later, the Sacklers visited Cambridge and Sir David Williams made the pitch. Anyway, after he asked for this sum and failed to get it, there was a lunch in Pembroke College, and Beverly came up to me and said, "Sorry, Richard, it's just too much, it can't be us." I got on with Raymond very well, and when he came to me said, "I'm terribly sorry, it's just too much," I could see he was a bit embarrassed. And so I said—and I had no authority to say this—"Well, what about something smaller, like instruments?" He leaned over to me and he said, "Yeah, get me a paper on instruments, whatever they are. Don't make it so expensive." So I wrote up a case called the Deep Sky Initiative to build a new infrared camera. I had no authority from the university to do this. But he invited me to New York. In those days Cambridge had a fundraising office

in New York City—the Cambridge University Development Office in the United States, CUDOUS. [Laughter] The woman who ran it—a Cambridge alum named Victoria Hall—and I became incredibly good friends. We were an amazing combination because, living in Manhattan, she knew Raymond very well. All I had to do was fly to New York, have a fancy lunch with Raymond, and she would then follow up in classic fundraising style, saying how Richard was so excited about the meeting and he had grand plans and everything. So, over the years, we got a million pounds or more from Raymond for this infrared camera.

ASPATURIAN: Which telescope did you put it on?

ELLIS: It went first to the Canary Islands, then eventually it went to Carnegie's du Pont Telescope at Las Campanas in Chile, and we did a big survey with it in collaboration with Carnegie. That continued for the first few years after I moved to Caltech.

ASPATURIAN: So actually, why don't we talk about how you came to Caltech. I'll preface it by saying when I interviewed Tom Tombrello [Thomas A. Tombrello, Goddard Professor of Physics, PMA division chair, 1999–2009, d. 2014] for his oral history [*Interview with Thomas A. Tombrello*, Caltech Oral Histories, 2012; http://oralhistories.library.caltech.edu/206/1/Tombrello 2012_abridged version.pdf], he talked at some length about this; and from his end of things, he'd apparently been looking at some individual who went to Germany.

ELLIS: [Reinhard] Genzel.

ASPATURIAN: That sounds right. And he spoke to Roger Blandford about prospective candidates, who suggested that he talk to you.

ELLIS: In 1998 I was seeing the end of my directorship coming up. I was a bit frustrated by the tension with Martin, but other than that everything was going well. One thing that I achieved and that we've stepped over is that I had managed to move George Efstathiou from Oxford to Cambridge. He was a Savilian Professor at Oxford, and he had had a

divorce. It was a terrible time for George. He had to sell the house in Oxford—their family had two children. Oh, it was awful. But he and I were great friends, so we invited him to Cambridge. Well, Donald Lynden-Bell came to retirement so we needed a theoretical astronomer. Guess who we ranked No. 1? Roger Blandford! So I was in charge of recruiting Roger Blandford.

ASPATURIAN: Was he at Stanford at the time?

ELLIS: He was still at Caltech. I tried my darnedest to get Roger to move to Cambridge. I negotiated all kinds of new precedents with the university. For instance in those days Cambridge didn't give tuition money for a professor's family, but Roger was about to put his kids through U.S. university and on his Cambridge salary he would never have been able to afford it. So I went to the vice chancellor, and I said, "This guy is an absolute star," and for the first time I think in the university's history, they created a personal fund for Roger that would have covered this. In the end, Roger turned us down. His wife didn't want to leave California. Next on the list was George Efstathiou. So George moved to Cambridge just when the RGO thing was flaring up, and it was great to have him there. Still, I was thinking because the children were leaving home that if we were ever going to have an adventure in America, this was the time. I was observing in Hawaii when I met Len [Lennox L.] Cowie, who's a professor at the University of Hawaii, and he told me that the director of the institute for astronomy in Hawaii, Don Hall, had just been told to resign. It's a long story, but he'd been found drunk at lunchtime in charge of a university automobile. So suddenly the directorship of the institute in Hawaii was vacant. Initially they offered it to—oh gosh—the stellar astronomer from Berkeley whose name escapes me. Chinese guy, Taiwanese guy.

ASPATURIAN: Frank—

ELLIS: Frank Shu. Very good, Heidi. Very impressive.

ASPATURIAN: I knew one of his postdocs, so the name is familiar to me.

ELLIS: Frank requested an institute for his wife, and the university couldn't afford it. So he withdrew, and they flew me out. Of course, word got on the rumor page that Richard Ellis was moveable.

ASPATURIAN: You were on the market, so to speak.

ELLIS: I was on the market. I remember—I'll never forget this—I was sitting in my office one November day, looking at the dark. It gets dark in England at four o'clock in November. I was looking at this darkness, and I got a phone call from Chuck [Charles C.] Steidel, out of the blue. And Chuck said, "Have you thought about moving to Caltech?" and I said, "Frequently." And he said, "When are you next in America?" I said, "As it happens, I'm going for an interview in Hawaii next week." He said, "Can you stop in Pasadena?" and I said, "Sure." So that was the beginning of that.

There were two incidents connected with this that are worth recording, one involving Tombrello. The first is that I came not knowing what to expect for the Caltech visit. Chuck had said, "Let's chat about your application," because by now I'd sent them my CV. I came to Pasadena, and I went into Robinson [Henry M. Robinson Laboratory of Astrophysics] and I couldn't find Chuck in his office. So I went to his secretary, and I said, "Do you know where Chuck is? I'm Richard Ellis; I'm here for a meeting with him." She said, "Oh, I know where he is; he's downstairs. I'll take you there." So she took me downstairs. She opened the door, and in the room, all assembled, were the faculty of astronomy at Caltech, and they said, "Come in, Richard, and have a seat." They never told me they were going to interview me! And that was it. They interviewed me in real time as it were. It was quite friendly. That was one humorous aspect of coming to Caltech.

The other was when I finally got an offer from both places, and I said to Hawaii, "I can't just accept your position. I've got to bring my wife out to have a look at Hawaii." They said, "Of course, we know that your wife will have to come and visit Hawaii." Then I got an e-mail from the acting director of the institute saying, "Dear Richard, just to say that, of course, we are very happy to have your wife come out, but the state university will not be able to cover her airfare." The bottom line is, Tombrello said,

"We'll pay for Barbara to go to Hawaii so that she can see that she doesn't want to be there." I thought, "I think I see the writing on the wall here. You can achieve things at Caltech that you can't do in Hawaii." And once I visited both places, it was pretty clear. There was nothing in Hawaii for Barbara. And there was the additional distance. The kids were in school in England, and the additional burden of flying all the way from Hawaii to England would have been just too much. So I went to Lynden-Bell and said, "I've got these two offers. What do you think I should do?" And Lynden-Bell said, "Well, I was at Caltech as a young man. Caltech's a great place. You need to go to Caltech. But we'll keep your position here for three years—I'll go to see the vice chancellor. You go and have fun at Caltech and come back in three years' time." I said, "I don't think that's going to work. People at Caltech won't give me any telescope time if I've only got one foot out the door here." And then I said, "What will you do if I go to Hawaii?" And he said, "We'll never speak to you again." [Laughter] And Tombrello was fabulous. He introduced me to the Keck science committee, and put me on the committee immediately. He made me deputy director of Palomar with Wal, who was the director. And Wal was very good.

ASPATURIAN: How much experience had you had with the Kecks before you came?

ELLIS: Okay, so, looking back, why did I come to Caltech, scientifically? And I think the answer was really the Keck telescopes. Britain was of course planning the Gemini telescopes, but they were still under construction. I was on the science committee and the board of directors for the Gemini telescopes in Britain, so I could see the progress that was being made. But there were now two Keck telescopes, and sitting in Cambridge at the end of my directorship in '99, I could see the lure. I'll never forget my first observing night at the Keck. We went to the first target and took an acquisition image of thirty seconds or so, and it was deeper than anything I'd seen on any other telescope. Just thirty seconds, and I could see objects that I had struggled to see on the Herschel Telescope in much longer integrations. I remember thinking, "This is just going to be so exciting."

When I came to Caltech, Shri Kulkarni was executive officer. I must say he was very supportive. He gave me the teaching assignments that I wanted and encouraged me.

He said, "Oh, Richard's bound to bring a big group." And so he knocked a wall down in the basement of Robinson to create a big room, which he said was going to be Richard's group. One of my students put on the door a sign saying "Ellis Island,"—you know, because I had students from Cambridge who came with me to finish their theses. Of course, I had some start-up money, so I hired postdocs. Those years, 2000, 2001, were very exciting for me. I had access to Keck; I had my own group. It was a great start.

ASPATURIAN: You alluded to "huddled masses" a minute ago, which almost sounds like a play on galactic clusters. So it's very appropriate.

ELLIS: [Laughter] That's right. I got some Hubble money, I remember, and now that I had enough money to hire a postdoc, I hired Tommaso Treu, who was a student in Italy. He's now a professor at UCLA. He was just fabulous, very enthusiastic. We got on very well. He did his own programs as well because Caltech postdocs can apply for Keck time in their own right. The first graduate students I had were very good, too. Mike Santos was a student. He went to work for Boston Consulting. [Since 2013, he has been a deputy director with the Bill and Melinda Gates Foundation. *–Ed.*] Kevin Bundy and Dave Sand were great students from those early days, too. And Tombrello was interested in what I was doing. He was supportive of the science**.** Caltech's great. You don't get burdened with too much. The teaching load is modest. The support is good. People grumble that Caltech isn't what it was, and I can see that there's been some decline. But overall, I would say, people generally are supportive. Later, we tried to leave a number of times, largely for family reasons, but time and again when I thought about it, I thought how great it is to work with the students at Caltech and how enthusiastic they are. They have a camaraderie amongst themselves that is I think somewhat special.

Anyway, are we going to pause at this point? Oh, supernovae.

ASPATURIAN: If you're tired and we would rather do this in another—

ELLIS: Let's do the supernovae. It's an interesting story.

ASPATURIAN: All right. The supernova chapter. We return to about 1988, I think.

ELLIS: Yes. So in the mid-'80s I had taken these photographic plates with the Schmidt telescope in Australia, and we were scanning them for galaxies. We were counting the galaxies and measuring their clustering. There were two machines: COSMOS in Edinburgh, and the one in Cambridge, called the Kibblewhite machine because it was run by a man called Ed [Edward] Kibblewhite, who's now a professor in Chicago. I always enjoyed going to Cambridge because Ed was a real bright guy, intellectually smart. One day we were chatting over lunch, and he said, "You know, I've had this project that I wanted to do with these plates to detect supernovae." I said, "Well, that's very difficult, Ed; they're very rare." And he said, "But you know these plates are big; they cover many thousands of galaxies. The chances are that in any exposure, if you took ten plates one month and ten plates the next month, there's going to be a supernova in one of those galaxies." These galaxies are quite distant. The idea was that we'd be able to measure the deceleration of the universe because the Type Ia supernovae—the white dwarfs that explode—are standard candles, so you can estimate their distance from their brightness. So you would measure their velocity from the spectra and measure the distance from the brightness. Ed said, "But the plates would have to be Federal Expressed from Australia. It's just logistically practical to do the project." But that was when CCD detectors were hitting the telescopes for the first time. So one day, when I was still in Durham, I got a phone call from the director of the Copenhagen Observatory in Denmark saying that they were trying to find distant supernovae with a CCD camera they had just put on the 2-meter Danish telescope in Chile. But the field of view of their camera was small, so they were going to look at clusters of galaxies where there are many galaxies in a small field of view, maximizing the chances of seeing the supernovae. I said, "I've always wanted to do that project." He said, "Well, you've got a catalog of clusters and you know their redshifts, so if we find a supernova in the cluster, we don't have to get a spectrum because we know the distance of the cluster."

ASPATURIAN: That's an ingenious idea.

ELLIS: Yes. So I said, "You're on, this is a great project." At the time we had a postdoc, Warrick Couch, a New Zealander who was in Durham with me, and the two of us worked

with three Danish astronomers on this project. It was exhausting because you'd have to go to Chile, observe all night, and then compare the images that night with the images that had been taken the previous month. You couldn't go to sleep because time was of the essence. As soon as you found a supernova, then you had to trigger measurements elsewhere that would get the light curve and so forth. So I was in charge of the follow-up observations. I wrote a proposal to the British time-allocation committee to argue that if we found a supernova, then we had the right to kick people off telescopes, so that we could get a light curve and spectrum to prove that the supernova was in the cluster and determine its type. This was novel; British astronomy hadn't done anything like this before. This is what we call target-of-opportunity observing. It's now everywhere, but in those days this was a new concept. We had, I think, fifty clusters, and we would observe eight or so a night. We were only observing in certain months, February through to May, I think. We had no idea how many supernovae we would find because the rate of supernovae was highly uncertain: There were estimates in the literature that varied by factors of five. I think we only found four, and not all of those were of the kind that are useful for measuring the distance. There are two types of supernova: Type Is are the so-called standard candles, and the type IIs are much more heterogeneous and not useful for cosmology, at least not at that time. So of the four, I think, one was a Type II, and a Type I that we found was the most distant supernova by a long way that had been seen so far. In fact, it was the first cosmologically distant supernova—it had a "majestic" redshift of 0.31. We got a spectrum of it and a light curve from the Anglo-Australian telescope.

ASPATURIAN: What year are we in now?

ELLIS: This was 1987. So we wrote a paper that was published in *Nature*, and we had a press release. And it got a lot of attention, because it was an indication of the feasibility of this project, which was to measure the rate at which the universe is slowing down. We calculated in our paper how many objects we would need to make a measurement of omega, the mass density of the universe—there was no awareness of dark energy at that time. I was keen to carry on. After all, we had demonstrated that it worked. To my great sorrow, the Danes threw in the towel and said, "We've shown it's feasible. We're

exhausted; we're not going to put any more time into this." It was about that time that Carl Pennypacker, who is an astronomer at the Lawrence Berkeley Lab, came to Durham and said, "You guys have done a great job. We'd like to carry on, and we'd like you to be involved in the cosmology project." They were getting money from NSF. There was a physicist at Berkeley called Bernard Sadoulet, who had gotten one of these new NSF centers in cosmology, which had various research strands to it. One was direct detection of dark matter. Another was the cosmological measurement of the slowing down of the universe.

ASPATURIAN: Which was thought at that time to be happening.

ELLIS: The novelty was that because Pennypacker and Sadoulet were in the U.S. where there were now much bigger CCD cameras on the telescopes—that meant a bigger field of view on the sky—making it more likely to catch a supernova. It's a long story but the first two or three years of what we called the supernova cosmology project were not successful.

ASPATURIAN: This was the one based in Berkeley?

ELLIS: Yeah, and they were floundering. The software wasn't working. They were finding supernovae after they'd faded, and it was too late to follow them up. Because it was an NSF-funded activity, there was a review committee and on this review committee was Bob Kirshner from Harvard, and he was very critical. I remember visiting Berkeley and having lunch with Bernard Sadoulet, and he said, "If things don't progress soon, we're going to cut this project dead." And I said, "Please don't. It's just a matter of time before these guys get their act together."

ASPATURIAN: Why did you think it was so important when others were willing to discard it?

ELLIS: I thought that this *was* a feasible project. We'd demonstrated that you could find these supernovae. I thought we only needed to find ten or twenty, and we'd have the

answer. I knew that there might be subtleties; I knew that it wasn't straightforward; but it's the kind of pioneering thing I love to do, and it was just great fun. So one day, Perlmutter was moved in as the manager.

ASPATURIAN: Saul Perlmutter?

ELLIS: Saul Perlmutter. Without any announcement. Suddenly Pennypacker was out. Still on the team, but Saul was in charge.

ASPATURIAN: He was a cosmologist?

ELLIS: He was a particle physicist. Obviously he'd been recommended to take over this project. There's been a lot written about this, but Saul deserves a lot of credit. What he did do that many of us didn't like was to bring the project firmly into Lawrence Berkeley Lab, so that all of the data flowed in there and all of the analysis was done there.

ASPATURIAN: He centralized it.

ELLIS: Centralized it. But I was still playing a key role in the follow-up; so I measured the redshift of the next most distant supernova that they found. That was a redshift of 0.46, I think.

ASPATURIAN: What year was that?

ELLIS: That was 1992, '93. And then they shifted the observations from the AAT and Herschel to Keck because it was so much more powerful, so I was no longer in charge. So Saul got objects with a redshift of 0.7, 0.8. And then eventually, in 1999, we had fifty-odd of them, and that led to the famous paper.

ASPATURIAN: A couple of questions here: There was a second group at Harvard, headed by Kirshner.

ELLIS: Yes. As long as Saul and Carl Pennypacker were floundering, Bob Kirshner was happy to criticize them. But then—my reading of this is that the Harvard astronomers finally realized that Saul had got his act together and that he had some very clever ideas of how to organize this campaign and to make sure that the follow-up was coherent. I think then it slowly dawned on Kirshner and the people that he was working with—Adam Riess, Brian Schmidt—that by God, these guys might do it. They might actually get the result. So they then said, "We can do this, too." I have to say, they copied many of Saul's ideas. But they were a much more horizontally organized group, so lots of people could contribute ideas. I enjoyed meeting them; I often had dinner with them. In many ways I felt I was on the wrong team because Saul's team was too much Saul and then the rest of us, whereas the other team was horizontal. I kind of got on better with the other team.

ASPATURIAN: Were you tempted to defect?

ELLIS: One guy did switch, and that was [Alexei V.] Filippenko. On the final paper, he's on both teams, so his citations are gigantic. It's because he couldn't make his mind up which one was going to win, I think. Eventually he jumped ship. He fell out with Saul and moved over to Brian Schmidt's team. Well, then to end the story, we all went to Stockholm, and we all got on very well. [The 2011 Nobel Prize in physics was half awarded to Saul Perlmutter, and half jointly to Brian P. Schmidt and Adam G. Riess "for the discovery of the accelerating expansion of the Universe through observations of distant supernovae." The laureates at both Berkeley and Harvard invited all members of their respective teams to join them in Stockholm for the ceremony. —*Ed.*]

ASPATURIAN: I want to step back for a moment. I believe there was a paper published in 1997, suggesting that on the basis of the initial—

ELLIS: Oh, God. All right. You're very astute to spot that. So there was a mistaken paper.

ASPATURIAN: Those are often very interesting.

ELLIS: In that paper the Berkeley team reported that the universe was slowing down, a conclusion that was based on eight supernova events. Of course when the fifty-five events came in, we realized that the results were not consistent. We had to go back and look carefully at the eight events. It's certainly true that several of those eight events were poorly calibrated. It was the light curves that were flawed, and a very small change in a few objects out of eight led to this spurious result, which the other team, I think quite rightly, chastised us for.

ASPATURIAN: At the time?

ELLIS: No, later. Much later. So when we first put out this paper, it seemed to be consistent with what many theorists wanted—what we call an Einstein-de Sitter universe, which is one with a high density, characterized by a quantity called Omega The problem was that astronomers had not found enough dark matter to account for that high density and so there was this tension between different sets of observations, and I remember feeling uncomfortable about this 1997 paper. But that was the motivation for the supernova cosmology project. Once we had demonstrated in 1987 that it was feasible with CCDs to find these events at high redshift, it was just a question of whether you could find enough of them to get a statistical result. So we all imagined that we would measure Omega, and we initially wrote this paper that found a high value of Omega, which was incorrect, and then ultimately we found that the universe was not slowing down but accelerating, which was a big surprise.

ASPATURIAN: This was totally unexpected?

ELLIS: This was completely unexpected.

ASPATURIAN: No one had predicted this?

ELLIS: Well, the concept of what we call dark energy, which is the missing ingredient that explains the acceleration, goes all the way back to Einstein, who wanted a static universe.

ASPATURIAN: That's right.

ELLIS: From time to time, this so-called cosmological constant, which acts as a repulsion term, had resurrected its ugly head. But there was no observational evidence for it until early in the 1990s when there were a few articles that suggested that it could be one way to explain the age of the universe, in particular because the age of certain stars at one point seemed to be greater than the age of the universe.

ASPATURIAN: Yes, I remember that. The globular clusters.

ELLIS: Yes, the globular clusters. And so from time to time there were these types of tensions that led people to suggest that we lived in a universe that was accelerating and hence was older than it appeared to be. There are some critics of the supernova result. Some people believe that the work based on it shouldn't have gotten the Nobel Prize, but it's certainly true that in the general astronomical community, the two supernova papers were the first credible evidence of the dark energy.

But then after both teams published the papers that led to the Nobel Prize, Kirshner in particular chastised us for being inconsistent and changing our minds. It is an embarrassing part of the story, without a doubt.

ASPATURIAN: What was your role in all of this?

ELLIS: By now when we were in full flight I was in Cambridge, measuring fifty-five of these supernovae. So I was effectively the UK lead. I would come out to Berkeley, and I was involved in verifying the light curves and the photometry. I obviously played a part in shaping the final paper.

ASPATURIAN: So you went to Stockholm as part of—how many people went? It must have been a mob.

ELLIS: [Laughter] There were two teams, and it was slightly unfair that one team had two Nobel Prizes and we only had one. I don't think any of us knew what was

happening. So the announcement is in early October. The event itself is on Nobel's birthday in December. For the whole of October it was absolute chaos trying to figure out whether we were going or not. And if we were going, crucially, were we going to be able to attend the event itself and the banquet? In the end, after a lot of hard work by Saul Perlmutter and his secretary, every coauthor on the Berkeley paper and their spouses got an invite to Stockholm, but spouses did not get invited to the prize ceremony. At the banquet, there was a basement banquet room where the spouses were placed with some of the Nobel family. They had exactly the same meal and exactly the same cutlery but couldn't see the royalty and everything. We were asked if our institutions would pay our way, rather than Perlmutter coughing up for the whole bill, and I think in the end I combined it with a trip. I think I paid myself to go to Stockholm, and I paid for Barbara completely. It was an amazing event—a whole week of celebrations, with something on every day. There was a reception at the Royal Swedish Academy on arrival. There were lectures by the Nobel laureates. There was a team lunch at a beautiful Swedish restaurant on an archipelago. Then of course there was the ceremony. We all had to rent clothes—you know, you couldn't just bring a tux; you had to get a Nobel-approved tux. So we had to get fitted for that. Then there was the banquet, which was amazing and afterward there was a party held by students at Stockholm University, which went on until about four in the morning. You had to have a good constitution to go through this.

ASPATURIAN: It was cold, too?

ELLIS: It got dark at two-thirty in the afternoon, and it was bitterly cold. But it's a majestic city, and it's all waterways. And finally, perhaps the most enjoyable thing for me, where I actually contributed orally, was a session at the university on the final day when all of us, from both teams, spoke about our reminiscences. That was videotaped, and the place was completely full. There were four- or five-hundred students there. All team members stood up and made little speeches about their involvement. I talked about the Danish work and my involvement with Saul. The whole thing was humorous; it was peppered with jokes.

ASPATURIAN: This is one of the few times the physics Nobel has been awarded for astronomy, isn't it?

ELLIS: Well, you can go back to Taylor and Hulse [Nobel Prize in physics, 1993] for the discovery of the pulsar that was slowing down, which gave us evidence of gravity waves and then, of course, the original discovery of pulsars [Nobel Prize in physics, 1974].

ASPATURIAN: There was also the cosmic microwave background [Nobel Prize in physics, 1978].

ELLIS: The cosmic microwave background. The Nobel for the supernova work was one of the clearest optical astronomy prizes ever. One has to go back—gosh—to Chandrasekhar [Nobel Prize in Physics, 1983], but even that work was theory, really, as was Hewish's.

ASPATURIAN: The Hewish pulsar work was in radio, wasn't it?

ELLIS: That's right. So I can't think of an optical result before this.

ASPATURIAN: Do you think Kirshner felt slighted in being passed over for the Nobel Prize?

ELLIS: Yes. Definitely. If you read the quotes, it's clear that he was frustrated, and Brian Schmidt and Adam Riess have said so to me personally. He was a bit difficult in Stockholm.

ASPATURIAN: I can imagine.

ELLIS: I sympathize with him, but that's the way these prizes go. To be honest, so many people worked on these projects, it's invidious that only one person from our team got the Nobel Prize. Why were there two from that team and only one from our team? On our team, Pennypacker was instrumental in the beginning.

ASPATURIAN: Yes, he started it. There's one thing we've left out of this period, which is that you were elected to the Royal Society in 1995.

ELLIS: Oh, right, yeah, that was great.

ASPATURIAN: Was it expected?

ELLIS: The way it works is you're nominated by two people. I was nominated by ArnoldWolfendale and possibly Martin Rees, and probably the first time I was put forward was 1992. George Efstathiou got it in '94. I remember hearing about that. He was younger than me.

ASPATURIAN: And your former student.

ELLIS: Yeah, right. That was great for George. The next year I got it. The way it works is that somebody tells you privately that you've been successful, and then you get a letter from the Royal Society. The person who told me first was Martin Rees. I was visiting Baltimore, and I got an e-mail from him, and here's exactly what he said: "Congratulations, you've won the Royal Society lottery this year." Donald Lynden-Bell then came to my house, and I told him, "Well, Martin's already told me." I think he was a bit miffed because he wanted to tell me personally. And it was really very lucky that I got it so early. I realized once I became a fellow that there's such a long list of people waiting to get in. There's a quota, and typically we get one or two astronomers each year. But, yes, it was great. It was my first big honor, I think. My mother was still alive.

ASPATURIAN: She must have been so thrilled.

ELLIS: Yes, there's a special ceremony, and she came down to London, and that was nice.

Some material in this session was originally recorded during interview sessions 6 and 8.

RICHARD ELLIS

SESSION 6

February 14, 2014

ASPATURIAN: You said earlier that one of the inducements for you to come to Caltech was the TMT. What was it that you and Tombrello discussed back then, at the start of this project?

ELLIS: Well, Tombrello was crucial in getting me to come to Caltech. As I've said, I knew all the players in Robinson very well. I was particularly close to Sargent and Steidel, whom I'd known for many years because I work in similar areas. I'd been director of the biggest astronomy research institute in the UK, and I'd seen a big telescope project through from beginning to end with the Gemini telescopes, which were now being built—in fact, I went to the opening of Gemini North more or less as I arrived at Caltech. I think I felt that I had reached a limit at what I could do in Britain administratively. I was ready for an adventure. Caltech beckoned. Tombrello could see that I was able to orchestrate big projects, so he was very keen to get me to come. Everything was amazingly attractive. There were two Keck telescopes fully functioning. I'd reached the limit on what I could do on the William Herschel Telescope, and I knew that I would get a lot of observing time on Keck. And more or less immediately I accepted. While I was still in the process of leaving Cambridge, he invited me to Hawaii for a meeting of senior astronomers from Caltech and University of California. At that meeting in the summer of '99 in Kona, we met in a little room—there was Wal Sargent, Tombrello, myself, Jerry Nelson, and Joe [Joseph S.] Miller.

ASPATURIAN: Who is Joe Miller?

ELLIS: Joe Miller was the director of Lick Observatory [University of California]. We agreed at that meeting to set up a steering committee for what was then called the California Extremely Large Telescope [CELT]. Tombrello announced at that meeting

that Ellis would be on the steering committee, and that was great. Wal was going to be co-chair of it with Joe Miller. I remember Wal coming up to me after the meeting and almost apologizing that I wasn't the chair, and I said, "C'mon, I haven't even arrived yet." I thought this was just fabulous. I could see the project at its beginning. And of course [Stephen E.] Koonin was provost [1995–2004], and he was very aggressive about getting going on this. I believe—this is before my time—that it was in a conversation at Owens Valley [Radio Observatory] between Koonin and Tombrello where Koonin turned to Tombrello and said, "When are we going to build the next big thing?" That was the beginning. Of course, Tombrello got money from Gordon Moore personally to do a conceptual study between 2000 and 2003, which is a little book that we all produced. So this committee made a lot of progress.

Now there's an incident that I should probably mention. I also was very close to people at Carnegie. In 2000 the director of Carnegie was Gus Oemler, a very distinguished astronomer, whom I'd worked with since the late '80s and knew very well. I knew his family, Barbara knew his wife. When he was still at Yale, I invited him to Durham. He had now moved to Pasadena. So Oemler and I and Alan Dressler had worked together on a big Hubble program called the Morphs, with my former student Ian Smail, who was at Carnegie at the time. We got a lot of Hubble time to image a lot of clusters, and we wrote a series of very nice papers on the evolution of spiral galaxies in clusters. So I knew Alan and Gus very, very well. So Richard Ellis arrives in town; and Carnegie people felt that this was a breath of fresh air. Somebody from outside has come in, and Richard's now at Caltech, beginning a project with Wal Sargent and Tombrello. So one day I got a call from Oemler saying that he thinks Carnegie should join CELT. I said, "That's a great idea; we're just beginning." He said, "Well, what does it take?" I said, "Well, why don't you come and see Tombrello." So a meeting happened where Oemler and Dressler came to see Tombrello and Wal Sargent. Wal was always full of admiration for Jerry Nelson as a technical wizard—he had seen Keck from the beginning to the end and was full of admiration for Jerry and his engineering skills. So I think Wal approached this meeting with some nervousness that our UC colleagues would think that we were ditching UC for Carnegie. I wasn't present at that meeting, but I wish I'd been a fly on the wall because it led to a breakdown in relations again. Tombrello is a very hard

negotiator, and the version I've heard is that Oemler said, "We want to help you raise money." And Tombrello said, more or less, "Go away and raise some money and come back when you've raised it." Dressler got upset, Wal was not exactly cooperative, and of course they went away offended. You know, sadly, it led to a view—particularly with Dressler—that I had let them down. But I wasn't director of Palomar. I had no authority at that time. I wasn't in the room, and I only heard about this afterwards, and I was very upset because there was a lost opportunity. And look at the situation now. If that meeting had been positive, Wendy Freedman and we would be involved in one telescope not two. We probably could have funded the entire telescope within the United States.

ASPATURIAN: Do you want to talk about what that led to?

ELLIS: Gus eventually quit as director, prematurely in my view, but his wife put her foot down and said she couldn't live in LA any longer. She was close to Barbara so I know that for a fact. So they left. And Wendy Freedman was appointed his successor. An inspirational choice. Dressler was upset he didn't get it himself, of course, but there we go.

ASPATURIAN: He wanted the job?

ELLIS: Oh, very much so. When Wendy got the directorship, he tried to jump ship and go to UC Santa Cruz as director of Lick [Observatory]. He nearly got it, actually. So, of course, Wendy then said, "Well, to hell with Caltech, we'll build our own telescope, the Giant Magellan Telescope, GMT. We have the land in Chile, and we'll work with our colleagues in Arizona." So now we have this ridiculous thing where we have two institutions, two miles apart, not cooperating. There were various attempts in the history of TMT and GMT to try to mend fences. [David] Baltimore and Maxine Singer arranged joint dinners for us. They were embarrassing. We all had to be nice to each other while we were dining, and of course it was expected that this would lead to synergy and it didn't.

Then, when Richard A. Meserve replaced Maxine Singer as head of Carnegie, Henry Yang [chancellor, UC Santa Barbara] and [Jean-Lou] Chameau [Caltech president,

2006–2013] met with him, and they said, "Why don't we get some astronomers around the table and see if we can find common ground?" I was appointed to that committee, and we met at Carnegie and at Caltech, and we wrote a report—we were going to share instruments, we were going to try to synergize as best we could—and of course it led nowhere. My feeling on this is that, in the end, the history of Carnegie and Caltech goes back too deeply in many of my colleagues. Many have been at Caltech for so many years, and some of them are somewhat critical of the Carnegie astronomers' ability.

ASPATURIAN: You mean they're contemptuous?

ELLIS: Yes, saying that they're not up to the high standard of Caltech. So when you start from that perspective, it's very hard. It's unfair because there are some really great people at Carnegie. And, of course, from Carnegie's point of view, they obviously think we're offensive. I think in some sense we've tried a little harder than they have. We've given prize lectureships to several Carnegie people, but to my knowledge nothing equivalent has happened in reverse. We've given Wendy Freedman and [Stephen A.] Shectman the Greenstein lecture with a fabulous dinner afterward, and we've lauded them. And so we've tried a little bit to mend fences, but I think there's blame on both sides. And the price for U.S. astronomy has been very sad.

ASPATURIAN: Very high.

ELLIS: Very high indeed.

ASPATURIAN: I think now you have rival mirror designs as well, if that's correct?

ELLIS: Yes. SO TMT has been a lot harder than any of us imagined, and it still sadly could fail. That would be a big blot on Caltech's copybook, and let's hope it doesn't happen. So, in a nutshell, we worked well with the University of California on this design study, but Tombrello was frustrated with Wal. He felt that that Wal was not working hard enough as cochair of this committee to get a science case written. Wal was never a great committee guy. He's a great raconteur and a wonderful scientist, but I think

he found it uncomfortable having the responsibility of chairing this committee. I think the rest of us worked hard to make up for that, but Tombrello saw it. And so one of the first things that happened was that one day I got a phone call saying that Wal had resigned as director of Palomar.

ASPATURIAN: Was that a voluntary resignation?

ELLIS: I think he was more or less bullied into it by Tombrello. So I got a phone call saying that I was director of Palomar. And I was a little annoyed because I had barely arrived.

ASPATURIAN: Had you had any intimation of this beforehand?

ELLIS: No. Wal didn't mention it to me. It was summer of 2000, and I was back in Cambridge when I got a phone call from Tombrello saying, "Congratulations, as of today, you're the director of Palomar." I was deputy director when I arrived, and so it was natural that I would be the next director. I think there were two things happening here. One, the faculty were upset with Wal. Shri [Kulkarni] particularly was antagonistic, and Tombrello was bullying Wal with, "Why haven't you done this, why haven't you done that?" I was annoyed because I hadn't had time to establish my research group. I'd only been here a few months, and I had hardly met anybody on campus. I had to dive into administration, and I had to work with all the engineers. We were building the blue arm of the low-res spectrograph, which was a financial obligation that we had with the Keck Observatory. Suddenly I had to figure out how Palomar worked.

So I soon got into the swing of it. There are many aspects of my being director of Palomar, including the reorganization of it into what is now Caltech Optical Observatory.

ASPATURIAN: I'd like to go into that too [Session 7], but for now let's stick with TMT.

ELLIS: So anyway, we soldiered on with this conceptual study, and we eventually wrote a green book. The green book was a science case, a design, an operational model, a cost,

and then we had an external review. The guy who was chosen to chair this external review was Ed [Edward] Moses, who worked for the National Ignition Facility. He didn't do well there eventually, but he was a pretty high-powered guy, who I think Tombrello wanted to recruit as project manager for TMT. Also around the table was Gary Sanders, who eventually did become the project manager. And there were other external members—I can't remember them all. Anyway, we all whizzed up to Oakland [California], and we had a day doing presentations to this committee. The cost of the project at that time was $650 million. So I think that was a great time. I felt the project was going well. We received approval of this conceptual design. So now we had to raise money for what we call the full design, which would take us up to construction.

ASPATURIAN: Had the split with Carnegie over this already taken place?

ELLIS: It had already happened. So we were now trying to raise money from the Moore Foundation. There was a tension that developed—it's a regrettable period, 2003–2004. The tension was between Joe Miller, who was the director of Lick Observatory, and Tombrello, and to some extent myself. It was fueled by Koonin as well. We now had a CELT board: We were the Caltech members and on the other side, there were three UC members—Joe Miller and two others. So there was now a board of directors and a science committee. The heart of this dispute was that Tombrello had raised the $2 million that had paid for the CELT study, but Joe Miller regarded the intellectual property to be firmly in Jerry Nelson's brain. Tombrello wanted Caltech to have an equal technical role in TMT, so we were recruiting people. We recruited Steve Padin, who'd worked with Tony Readhead. He's a brilliant guy, a fabulous engineer, who's with us now working on CCAT [Cerro Chajnantor Atacama Telescope]. Steve Padin arguably is a young version of Jerry Nelson, and Santa Cruz saw this as a threat to the continuing supremacy of Jerry Nelson and the role of Santa Cruz in this project.

ASPATURIAN: Oh, I can understand how they would have felt that way.

ELLIS: It's just astonishing how these things escalate. I realize that not only does Tombrello have a temper, but Koonin could get worked up about things, too. So one

thing led to another, and because Gordon Moore was a Caltech alum, we found ourselves going up to see Gordon and the foundation without our UC colleagues.

ASPATURIAN: How did you feel about that?

ELLIS: Well, I was annoyed at Joe Miller, too. I remember even getting upset with Jerry Nelson. I had an argument with him about why was he so difficult to Steve Padin, why couldn't we work as a team, an equal team? We were not threatening him, and he, Jerry Nelson, was getting on in age. If this was a project that was going to go on until 2020, we needed young people. Did he have any problem with Steve Padin? No. It was all institutional rivalry—very similar to what had happened with Carnegie. I'd never seen anything like this in Britain. I was amazed at how everywhere we looked, there were territorial disputes.

ASPATURIAN: A frontier mentality.

ELLIS: Frontier mentality in California. So I was placed in the awkward position of phoning Joe Miller and telling him that we were going to raise money on our own. He was simply devastated. It was like breaking up with your wife. He was also very annoyed. So anyway in 2005, Tombrello, Koonin, and I flew up to San Francisco, and I made a presentation to the Moore Foundation, for $17.5 million. Gordon was in the front row. It was a fabulous occasion. Nobody dared ask a question until Gordon had asked one. The question Gordon asked was, "Why are the segments smaller than they are for Keck?" I said, "Well, the size of the Keck segments was determined by the computational power that we needed to orchestrate them in real time. But as you know, Gordon, computing power—and he just said, "Okay, Okay," and everybody laughed.

ASPATURIAN: [Laughter] Moore's Law. That's too funny.

ELLIS: And we got the money. The people at UC were devastated, and, not surprisingly, they then went up a few weeks later and got a matching $17.5 million dollars. So together we had raised $35 million. Now, besides, this we had a meeting with Baltimore.

This occurred while I was on an observing run in Hawaii. I had taken two days off to go to Molokai, where I got a frantic phone call from Tombrello saying, "We're on, we're going to have a telecom with Baltimore because he wants to know what we are expecting from Caltech for TMT." It was a very high-powered meeting.

ASPATURIAN: There you are, with a surfboard on Molokai.

ELLIS: Yeah, right. I was in this little hotel in Molokai, and I was on the phone to Baltimore and Tombrello, and Baltimore said to me, "Richard, this telescope is costing $650 million at the moment. It could go up in price. What is the minimum share of this telescope that is scientifically interesting for Caltech?" I said, "Thirty-three percent." He said, "No, twenty-five percent is all we can afford. You've got to go and find two more partners." Tombrello, when I got back, then said, "Let's go and find some partners."

ASPATURIAN: Had Tombrello had an inkling that Baltimore was going to say this before this meeting took place?

ELLIS: I think Tombrello knew that this was the answer. He was asked, and he said twenty-five percent. So he got the right answer and I didn't. But I thought I'd stick out for one-third. I knew fifty-fifty was hopeless.

ASPATURIAN: Actually, if you'd both said twenty-five percent, Baltimore might have tried to take you down to twenty percent.

ELLIS: Well, at the moment, we're at thirteen percent. We'll get to that. So I flew up to Victoria [Canada], and I got a fabulous reception. I explained where we were, and the astronomers there were absolutely in raptures.

ASPATURIAN: Which institution was this?

ELLIS: This was the Herzberg Institute of Astrophysics. It's their government lab in Canada—the national institute for astronomy. The Canadians already had done a similar

study for what they were calling the Very Large Optical Telescope, VLOT, and we had ours. There were some synergies there—the science case was similar, and they knew they couldn't build it themselves. So that was great. Simultaneously the group at the National Optical Astronomical Observatory, in Tucson, together with the Gemini director, who was at the time [Charles Mattias] Matt Mountain, had done a similar study called the Giant Segmented Mirror Telescope. So everybody was designing these things. And sure enough, the director of NOAO at the time was Jeremy Mould.

ASPATURIAN: Ah, whom you knew.

ELLIS: Yes. A former Caltech professor who'd gone to Australia and come back to Tucson. So Jeremy Mould and the director of AURA, William Smith, emailed me and said, "We'd like come visit Tombrello and you and talk about joining TMT." So we had a fabulous meeting in Tombrello's office. Jeremy Mould came with some PowerPoints, and the focus was, the U.S. wants to join the Gordon Moore telescope.

ASPATURIAN: Did the UC people know all this was going on?

ELLIS: Well, they did. It was reassuring that Caltech was the center of building this partnership. Joe Miller is not the kind of guy who goes around being humble and saying, "we need your money," but this was very much something I had learned to do in Britain, flying around the world, saying, "How about a partnership?" We'd mended fences by now because both sides had $17.5 million. We hired Gary Sanders—I was not involved in that decision—and then, of course, we had to set up a project office. So more skullduggery. Gary lives beyond Irvine, near Laguna Beach I think, and that's a hell of a commute from Caltech. So Joe Miller latched onto this right away, and said UC Irvine is willing to host the project. Well, Ed Stone had now been appointed to the board, and he and Tombrello fought very hard against Joe Miller again. Caltech must have the project office in Pasadena.

ASPATURIAN: Irvine sounds like an odd place.

ELLIS: Well, it would move project headquarters to UC.

ASPATURIAN: I understand.

ELLIS: So we're now up and running. We have four partners, including the U.S. astronomical community through AURA and Canada. Things seem to be going swimmingly well. And lo and behold, Canada raised $17.5 million too. We needed $70 million for the design study, according to Gary Sanders. Ed Stone is now fully in command of the board. All we needed was the federal money. So the federal money would be $17.5 million from the NSF through AURA. Then suddenly Wendy appears on the scene.

ASPATURIAN: Wendy Freedman?

ELLIS: Yes. Oh, before that happened, the AURA board, which is essentially the governing body of AURA, voted to endorse Jeremy Mould's proposal to join the TMT project. Wendy was on the AURA board at the time, and I think she voted for that motion. But then, as soon as the motion was passed, she gathered an assembly of professors in what was to become the GMT partnership and they wrote to either the National Science Foundation or to the AURA directors—I can't remember which—and they objected. How could AURA make a decision between TMT and GMT without a review? So there was a review of the whole process—which Roger Blandford was involved in—and we were all very annoyed.

ASPATURIAN: Did Wendy's maneuver take you all by surprise?

ELLIS: Oh, yes. This is the first chapter in what would become the telescope wars. Everything up until that point was going really well. So AURA was ordered out of TMT, and we were back to three partners. This is 2006–7, and now we didn't have enough money. Fortunately the Moore Foundation continued to pour money into this project, although they were upset that it now didn't have U.S. federal government endorsement. Their hope was that their money would lead to a public-private partnership within the

U.S., and if Canada was included as well, that's fine.

So suddenly now we needed another partner, and I had long had connections with Japan. I had lots of happy trips to Japan over the years. I'd gone there in the early days leading to Gemini, to set up the British partnership, which as I said earlier didn't work out. But now I knew all the key players in Japan, so off I went to Japan. In November 2001, I was invited to Tokyo for six weeks as a visiting professor. Part of this visiting professorship gave you money to travel to all of the Japanese institutions and give lectures. Then I went again—I can't remember when—but over that period, from 2001 to 2005-6, it was clear that Japan was interested in TMT. So we got the Japanese delegation over here and had a meeting with them. Tombrello was very enthusiastic. And then we had this process whereby first they would become observers on the board, and then they'd become full-fledged members once they'd agreed to raise money for TMT.

Meanwhile the cost of the project started rising: It went up from $650 million to $800 million. Shri became director of Caltech Optical Observatories, so he came on to the board. There was a little bit of a tension at one point, because I'd been with the TMT project from the beginning, and then one day when Shri took over from me as COO director, he quite rightly said, "Well, this is the biggest thing on the director's books; I should be on the board." To be frank, Ed [Stone] has never been very good at this kind of thing, keeping people happy. There's too much automation in his mind. So it looked like I was going to be kicked off the board and replaced by Shri. I remember talking to Tombrello, and Tom said, "This is ridiculous! You've been involved from the beginning; you brought Japan in; you brought Canada in. We need continuity here." So they exceptionally got me an additional seat on the board, but in a nonvoting category. So since about 2007 when Shri took over as director, I've still been on the board. But I would say I'm not as important a member as Tom Soifer, who took over from Andrew Lange and Tombrello.

ASPATURIAN: Do you want to talk briefly about the kind of science the TMT is envisioned as doing?

ELLIS: Sure. The science case has always been very strong, and it's evolved over time. We hired a guy called Dave [David] Silva as a kind of project scientist, and we worked very hard with him on the detailed science case after we got the Moore money. This led to the design of the instruments, the choice of first-light instruments. There's a very energetic science committee called the science advisory committee, which I was involved in from the beginning—I just finally came off it last year. The most impressive thing about TMT is that it is designed to have adaptive optics from the very outset. Adaptive optics—the correcting for blurring in the earth's atmosphere—is a revolution that I witnessed at Keck when I arrived. This was being discussed in Britain, but I was a skeptic. I mean, we were pouring huge amounts of money into adaptive optics, but I never really saw much promise in it because you always needed bright stars in the field of view. So what really transformed it at Keck, and at Palomar too, was what we call laser-guide star adaptive optics, where you shine a laser into the sky, and it scatters off a layer of sodium atoms high up into the atmosphere to create an artificial star. So wherever your celestial object is, you can create an artificial star next to it on the sky, and the wave front that comes from this artificial star is analyzed in real time, and the signal is then sent to a deformable mirror, which corrects this wave front so that you get pristine images.

ASPATURIAN: So you always have a target to correct off of.

ELLIS: That's right.

ASPATURIAN: What a clever idea. I remember when this work started at Caltech.

ELLIS: Yes, and so I saw this revolution at Keck, and I started becoming an adaptive optics user myself because you could study distant galaxies with Hubble-like resolution.

ASPATURIAN: It's really that good?

ELLIS: Yes. And of course you have the big advantage of a 10-meter aperture compared to Hubble, which is only 2.5 meters. The bigger the telescope's diameter, the better its

image quality inherently is, because of diffraction patterns. So scale this up to TMT—thirty meters. I could see that adaptive optics plus thirty meters gave us an enormous gain in power. It goes as the fourth power of the diameter for a point source in terms of spectroscopy in imaging. So two science cases stand out. One is direct planet searches—the imaging of objects near stars. The other is studies of very high redshift galaxies and other distant objects.

ASPATURIAN: So, observational cosmology.

ELLIS: Yes, particularly the assembly of galaxies. When you look at a distant galaxy without adaptive optics, all you see is the integrated light, which allows you to measure the redshift, the colors, the star-formation rate. With adaptive optics, you can resolve the galaxy to determine its morphology. You can see the gradient of composition across it, you can see its kinematics, how it's rotating. So this is a new revolution.

But as the price of TMT went up painfully, we needed more partners. Shri Kulkarni now became very influential in TMT, and he brought India in. I think Shri and I both worked hard to bring China in—I went to China and gave presentations in Beijing. The board itself changed. The chair is now Henry Yang, who's the chancellor at UC Santa Barbara, educated in Taiwan. He speaks Mandarin so he's been very influential in helping along the political aspects of TMT in China and even Japan, actually. Chameau and Henry Yang played a key role in getting the permit for construction on Mauna Kea, which was controversial.

ASPATURIAN: So I understand.

ELLIS: That's a whole story in itself. Some aspects of TMT have been disappointing. Caltech's share has gone down. We tried very hard, unsuccessfully, to argue that since we initiated the project and raised the Moore money, that early money should count extra. This is what became known as the premium factor. Japan fought tooth and nail to oppose this, and in the end we only got a very modest bonus for our early money. So I think we're at something like thirteen to fourteen percent with the Moore money that was pledged.

ASPATURIAN: And the other countries have like, what, twelve percent?

ELLIS: China and India are at sort of the ten percent level; Canada and Japan are at the twenty percent level.

ASPATURIAN: Do you feel that with the departure of Koonin and the arrival of Chameau, and eventually Ed [Edward M.] Stolper [Leonhard Professor of Geology, Caltech provost 2007–present; interim president, 2013–14], that support for the TMT at Caltech was affected?

ELLIS: I think Chameau saw it as his mission to work with the Moore Foundation to promote TMT. I never sensed that he was anything other than as supportive and as aggressive as Koonin. Stolper very cleverly kept out of it. I don't think Stolper is a big fan of TMT—he's not a big fan of big projects per se on campus. But rather than obstruct it, his view was always that it was Chameau's baby. One aspect that Chameau was not very helpful with was this premium factor. That was a big disappointment to the astronomers in Cahill—that when we had this stalemate on the board about the value of early money, we appealed to Chameau to help us; and either he decided it wasn't worth going to bat for, or if he tried, sadly he was ineffective. And Ed Stone to a large extent has not batted for Caltech. He's seen his role as coordinator, facilitating the board, and not as a Caltech representative. So for a brief period, the Caltech flag had really been up to Andrew Lange; and he was batting for this premium factor very effectively when he died. That was a tragedy. Now we have Tom Soifer, Shri, and myself. So it's left up to us to hold up the Caltech flag.

ASPATURIAN: Do you miss having Tombrello in the thick of things?

ELLIS: Oh, very much so. Tombrello could silence any discussion on a controversial topic. He was liked. The Canadians loved him. They admired his style. He would have been very effective with China.

ASPATURIAN: While we're on this topic, do you want to talk about your relationships with the division chairs you've dealt with?

ELLIS: Sure. Tombrello and I worked together very well. There were a few tense moments. I tried when I was director of Palomar to raise money from Francis Clauser [Clark Blanchard Millikan Professor of Engineering, Emeritus, d. 2013]. We were trying to raise money for—I can't remember—instruments, probably. I got a hint from Marshall Cohen [professor of astronomy emeritus] that Francis Clauser was keen to contribute to Palomar. So I met him and took him up to Palomar in my car; we gave him lunch and we toured him around, and then I wrote him a letter, which I very foolishly copied to Tombrello. And I got slapped on the wrist by Tom, who said, "You have no authority to raise money; that's my job." We had a difficult issue with an employee named Keith Taylor, whom I hired, and Tombrello loved him. He was building an instrument that was the forerunner of MOSFIRE [Multi-Object Spectrometer For Infra-Red Exploration]—this was under my watch as director. And it went horribly wrong. The project went over budget and failed a review, and so we cancelled it. Keith Taylor was obviously very upset. When Shri took over as director, he let Keith Taylor go. Tombrello, I think, was in a difficult position. He tried to support me in this difficult transition period, but in the end, he could see that this was the right thing to do. But apart from those two wrinkles in my time as director, Tombrello was amazingly supportive, especially on TMT. He was good to me about my science. Then Andrew Lange came in as PMA [Division of Physics, Mathematics and Astronomy] chair, and by this time I had an offer from Oxford. I'd been at Caltech for seven years, and I'd more or less told Barbara that we'd stay in America for eight years, and she wanted to go home. By now our kids were out of university and had jobs in London. I got an e-mail from Martin Rees saying that there were six Royal Society professorships coming up, which is quite a large number. These are fabulous positions. If you're appointed to one, you can choose which university in Britain to take it to, and you can negotiate your salary on top of what the Royal Society gives. So I applied, and I got an offer from the Royal Society. Then the question was, where would I go? There were three choices. I could go back to

Cambridge, but I just think you should never go back to where you were before. Here he is; he's come back with a suntan, and he expects to take charge.

ASPATURIAN: It's better to be the new kid in town.

ELLIS: Yes. There was Edinburgh, which was very keen to have me, and John Peacock, a professor at the Royal Observatory there, worked very hard to invite me, but Barbara didn't want to go to Scotland. We thought that the weather in Scotland after California would be too much to bear. At Oxford, where I'd been a PhD student, we worked with Roger Davies, who was the head of the astronomy department there, and we were given royal treatment. Wal Sargent noticed the Royal Society announcement that I'd been appointed to the professorship, and he broadcast to all the faculty that Ellis is leaving before I or Tombrello had time to react. That was unfortunate. In fact, Tombrello was fabulous. He said, "We knew this moment would come. Richard is going to try it out. We'll give you a joint position with Oxford for two years, and we're sure you'll come back." Sure enough, we did. To cut a long story short, Britain was depressing. It was not a good financial time for Britain. We went for the summer to Oxford. We found Oxford unchanged, really, since 1974.

ASPATURIAN: Really? That says a lot.

ELLIS: Unbelievable. The department was poorly organized, and the weather was terrible that summer. Some things were nice. But it was expensive. I missed TMT. I remember talking to Ed Stone, who said, "Well, it will be a shame if you go." I said to Ed, "I like working with you." When I came back to Caltech for the winter to teach, Shri was very good. He said, "We'll keep you in your observing time; just try and be reasonable in your request." By now Andrew Lange had become PMA division chair, and he invited me back. He called me and said, "What will it take to get you back?" and I told him I wanted to be more involved in TMT. I wanted an official role.

ASPATURIAN: You didn't want to be a nonvoting board member anymore.

ELLIS: That's right. And Andrew went to see Ed Stone and said, "Richard wants to come back; we need to give Richard a leadership role such as a spokesperson position." Shri was very keen on this and said, "Richard would be an effective spokesperson for TMT." Sadly, it never really happened. I think Ed keeps things very much to himself—I think he wanted to be the key person, even though he is not an astronomer. They gave me some leeway but only once. When we made the decision that the telescope site should go to Hawaii, I became the lead spokesperson. But I never got anything official, and Andrew of course passed away. When Tom Soifer came in, I tried to resurrect some of these arguments with him, but I didn't get very far. He wasn't able to convince Ed to give me a role, I suspect. In fact, quite recently, Ed decided to curtail my membership of the TMT board of directors. He didn't even bother telling me. I only found this out from a secretary preparing the Board papers. I told Soifer this was very ungrateful given the fifteen years of effort I had invested for Caltech getting TMT to where it now is, including five years before Ed even arrived on the scene.

ASPATURIAN: Andrew Lange will not be here to do his own oral history. I wondered if you'd like to put into the record a few recollections that you have of him.

ELLIS: Well, I didn't know him very well. He interviewed me when I came to Caltech in 1999. He said, "This is a fabulous place. I moved here from Berkeley; I've done magnificent things. You'd be nuts to not come here." Then, when I first arrived, I worked on a Spitzer proposal with him and got to know him then. I found him a tough cookie. He was building Bolocam at the time. But he was a nice guy. He was good to me. We wrote a Spitzer proposal together that we didn't get supported at the time. I remember the BOOMEraNG results—that was a huge step forward. During the time that I was working with Tombrello, he clearly thought Andrew Lange was Nobel Prize-winning material, that it was just a matter of time. We all knew that this was the golden boy at Caltech. But I didn't interact with Andrew very much until he became division chair for this brief period.

ASPATURIAN: And then he was succeeded by Tom Soifer, who has been running the division ever since.

ELLIS: So Tom is an astronomer, and he's more personable in many ways than Tombrello. One would have a hard time criticizing Tombrello to his face. Tom Soifer can take criticism, and he has been the brunt of quite a few difficult e-mails, I know. The relationship between Tombrello and Koonin was very constructive. I think Tom Soifer has had a much harder time with Ed Stolper and Chameau, and we've lost ground because of that, to some extent. Tom Soifer is not as effective as Tombrello in difficult situations, but he's certainly put a huge amount of effort into TMT and CCAT. I like Tom overall. He's been reasonable to me. It's been a harder era for him. Tombrello, you see, really rose on this fabulous stock market boom and the Gordon Moore era. Those were golden years. Tom Soifer hit the recession and stringency, with various people losing their jobs.

ASPATURIAN: That's right. And I think the administration became a different organism to deal with. Regarding the TMT, I believe Jerry Nelson was very sick at some point?

ELLIS: Jerry had a stroke. It happened very suddenly. Jerry was in full flight, an excellent engineer, a very creative part of the project. He attended the board meetings, was aggressive on any difficult topic, and would always raise embarrassing questions that Ed Stone was not willing to raise. And then very sadly, probably about eighteen months to two years ago, we got a message that he'd had a massive stroke. Now, he's in a wheelchair. He still goes to work, but he doesn't attend any of the board meetings. I haven't seen him since the stroke.

ASPATURIAN: What is the current status of the TMT with regard to the [Astronomy and Astrophysics] *Decadal Survey*?

ELLIS: That was a huge disappointment. There were various subcommittees of the decadal survey. The one on ground-based optical astronomy was led by Professor [Patrick] Osmer, who's at Ohio State. That committee ranked a generic large telescope—because there were two, GMT and TMT—as the top priority item. And then, when all of these subcommittees reported to the main committee, the order was reversed, and the Large Synoptic Survey Telescope, LSST, was ranked first and we were moved to No. 3

in the priority list of ground-based initiatives. I remember I was on holiday with Barbara in Madagascar when this news came out, and I thought, "This is really bad news; we're never going to get federal support for this telescope."

ASPATURIAN: I think they not only said that, but they recommended that the NSF do something to bring TMT and GMT together? That they do a kind of LIGO [Laser Interferometer Gravitational Wave Observatory] maneuver.

ELLIS: Yes, and that hasn't happened, really. NSF now has an observer status on the TMT Board. I was pretty negative about involving NSF—and Tom Soifer was pretty negative, too—but Ed Stone and Chameau were pretty keen on it, as was the Moore Foundation. So now we have NSF sitting around the table at our board meetings, but they're really a negative influence because they have no money, so they just pour cold water on the project, given the low likelihood that they will ever contribute significant amounts of money.

ASPATURIAN: With all this, what do you think the prognosis is for the TMT?

ELLIS: Well I'm still hopeful. We are right at the limit of the patience of the Moore Foundation. They've been so patient. They've put in a $140 million to get to this point, but their foundation has venture capitalists on it. They know that sometimes these things don't work out. So my biggest fear is that the Moore Foundation will wake up one day and say, "It just doesn't look as if this is going to fly." On the other hand, Japan is fully committed and has started putting real cash on the table. India and China—I'm confident they eventually will come through. Canada has this austere government at the moment, and so that's probably the biggest risk. Other than that we're ready to go. We've been reviewed countless times. We have the site permit. We know exactly what everybody will do. There's no disagreement now on observing time. We're ready to go.

ASPATURIAN: Now maybe in the end it wouldn't be too bad if there were two such telescopes. One looking at the southern sky, and the other studying the northern hemisphere.

ELLIS: I'm confident that Wendy's telescope will one day happen, too. And of course there's a European version as well.

RICHARD ELLIS

SESSION 7

February 19, 2014

ASPATURIAN: I thought we could talk today about your experience leading Palomar, a historic and significant observatory.

ELLIS: Right. Well, I was honored to be appointed director, even if I had expected it to happen a year after I'd arrived. Basically, Tombrello said, "You will be the next director of Palomar." But it was agreed that I would have a year of grace, as it were, with Wal Sargent continuing as director. And as we discussed [Session 6], suddenly in the summer of 2000, I got a call saying I was director of Palomar.

ASPATURIAN: How long had you been here at that point?

ELLIS: Just six months, so it was a bit of a shock. But I rose to the challenge, I think, and I learned a lot about Caltech. Suddenly I had to meet all the engineers; I had to deal with the people on the mountain. They're all delightful people. Palomar just has this history of great discoveries, which is the heritage of the observatory. It's one of the icons of Caltech. Right away I was looking at a number of issues. One was, what's the future of the observatory? TMT was on the books as a project, even though we hadn't really begun the design. So I decided to convene a committee of professors, which enabled me to get to know them. And within six months, we came up with a plan for the observatory out to 2018.

ASPATURIAN: Who was on this committee?

ELLIS: Steidel, Djorgovski; I'd have to go back and look up the others.

ASPATURIAN: The usual suspects.

ELLIS: Yes, about four or five of us. In those days, everything was so formal compared to what it is today. I reported to a committee called the Observatory Council, which has long since disappeared. It was chaired by Tom Soifer. Once a month I had to appear before it, which was quite a formidable experience, especially since I was new. I had to come into a room and give an update on what was happening at Palomar. I'd be interrogated, people would complain, and this Palomar plan, point by point had to go through this council.

ASPATURIAN: Had this council been in existence for a long time?

ELLIS: Yes. It was the tradition at the time. So that Palomar plan laid out a vision for the 200-inch telescope that had us finally winding it down in 2018, which we thought was comfortably within the TMT era. Ha, ha, ha—TMT is now not scheduled to start until 2022. But at the time, 2018 seemed a good horizon. We had two partners—Caltech had fifty percent of the observing time, and Cornell and JPL had a quarter each. They reimbursed us in two ways for that obligation: They paid cash for the operations, and they had to provide instruments. One of the big headaches was that Cornell, which as a university is of course resource-limited by the grants it can raise, was not maintaining its instrument contributions. So one of the first headaches I encountered was the astronomy faculty baying like hounds for blood from Cornell: "Richard, you've got to go to Cornell and tell those guys they've got to get their act together. Otherwise, we're going to reduce their observing time," that kind of thing. I did go to Cornell, and it was tough. I told them that they were not honoring the agreement. However, they were building an infrared camera, and fortunately we found a way out of this. I found a private Caltech fund—it was really amazing that it had been hidden—which was for the use of instruments at the observatory. So with a little bit of a helping hand, we bought a big detector for Cornell. At the time it was the biggest infrared array on any ground-based telescope in the world. So in partnership with Cornell we put their camera, called the Wide-Field Infrared Camera [WIRC], on the 200-inch. That was a big hit because suddenly the 200-inch had something no other observatory had. Steidel got a lot of observing time with it. I had postdocs who were using it. It was quite successful. So

overall I thought I got off to a good start with Palomar.

I also hired several people. We had a few engineers that were not productive, and I had to fire one guy, which was not very nice. I loved going up to the observatory. I went up there and gave talks to the staff. I enjoyed working with [Palomar site manager] Bob [Robert] Thicksten, a real ace of a guy, dedicated to the observatory. He worked so hard I was worried he'd have a heart attack or something on my watch. Eventually, after my directorship ended, he took retirement. I inherited my assistant director, Bob [Robert] Brucato, who'd been there since Neugebauer's time and was very supportive and loyal to me. He always thought the director was in charge and never did anything behind my back. I spoke with the earlier directors. Neugebauer was not well, but he was also very supportive, and thought my Palomar plan was a good idea. I had a very nice chat with Jim [James A.] Westphal [professor of planetary science, emeritus, d. 2004], what a nice guy. He told me not to get too worked up, that the faculty would always be complaining. Wal Sargent, because of the circumstances of his departure, kept a low profile. There was a crisis about halfway through my tenure—well, not a crisis, but an issue that arose when I decided to be much more rigorous about time-allocation. I chaired the time-allocation committee for both Keck and Palomar, which traditionally was the role of the director. And to be honest, I was shocked at how poor some of the proposals were.

ASPATURIAN: If you were doing time-allocation for Keck, your title must not have just been director of Palomar?

ELLIS: At that time, it was. However the director of Palomar also had jurisdiction over Caltech's time allocation for Keck.

ASPATURIAN: I did not know that.

ELLIS: This was one of the reasons why we eventually reorganized as the Caltech Optical Observatories—I'll come to that. Anyway, some of the proposals were truly dreadful. People had got used to just throwing together some ramshackle text—sometimes the same text from one observing season to the next..

ASPATURIAN: These were proposals from inside Caltech?

ELLIS: Yes, just internal, and the standard was low. Tombrello was right behind me in sharpening this up. He said, "It's a disgrace. Keck is a $200 million asset. An observing night on Keck is tens of thousands dollars a night. We ought to be scrutinizing the science very, very carefully." Now there was an impression on campus, even among distinguished professors, that Keck was a laboratory, and you didn't really have to justify your time on it. If you're a Caltech professor, it was your right to go there. I said that there was no excuse for shoddy proposals. Thinking about what you wanted to do and writing it down was an essential ingredient in discipline to get to the telescope. I made myself quite unpopular. Some people didn't get observing time, so they came to see me and they were very unhappy. Some bypassed me and went to Tombrello. Tombrello was fabulous. He defended me to the hilt. But ultimately it led to one professor going round and fomenting a kind of revolution. Part of this was a perception, which I took as an insult, actually, that I was getting more observing time than I ought to get because I was somehow manipulating the system. Anyway, one day I got a phone call from Tombrello—I can't remember where I was; I think England—and he said, "There's a revolution happening. You need to come back." So I came back, and I immediately called a faculty meeting and said that I was halfway through my directorship and wanted a review of everything that was happening.

ASPATURIAN: Faculty meeting of whom, the division?

ELLIS: Well, no, the astronomers. But in those days everybody turned up. Mike [Michael E.] Brown [Rosenberg Professor of Planetary Astronomy], Geoff [Geoffrey A.] Blake [professor of cosmochemistry and planetary science and professor of chemistry]. We just don't have that kind of turnout these days—if you called a faculty meeting now to discuss Palomar, maybe two or three people would turn up.

ASPATURIAN: But at that time, anyone with any interest showed up?

ELLIS: Exactly. It was clear that there was a cabal organizing this, with Shri and Djorgovski in the forefront. Wal was silent. It was very depressing—Wal could have defended me but chose to keep quiet. Soifer was on my side. He was impressed with how I was trying to improve the rigor of the observatory, but he wasn't that influential. The bottom line was they changed the rules. I agreed that the director would be an *ex officio* member of the time-allocation committee, but that the committee would now be chaired independently by somebody else. Brucato, I remember, was very upset and thought this was the beginning of the end. From Jesse Greenstein's time, the Palomar director had always led this committee. But I had no alternative, really. The hilarious thing is that of course this meant that I had more time to write proposals. I didn't have to do as much work. All I had to do was choose the chair of the committee; the chair would choose the committee itself with my advice; and we would jointly do the paperwork. As a result I actually got more observing time than I had before. And that's still the current model: The director now advises the committee but doesn't vote.

ASPATURIAN: Did the committee go back to this slightly slipshod assessment process?

ELLIS: No. I served on it throughout that period of my directorship, of course, and I've served on it since, and I think the standard I wanted to establish has been maintained. One additional thing I did was to bring postdocs onto the committee for the first time, and that's continued and has been a big success. The postdocs actually take the duty more seriously than the faculty.

ASPATURIAN: Yes, that would make sense, actually.

ELLIS: So then, along with the Palomar director allocating time for both Keck and Palomar, under my watch at Palomar we had instrument contracts for Keck, and we had engineering work under way for TMT instruments. We finished the near-infrared camera 2 [NIRC-2], we had the blue arm of low-res [LRIS-B], and we were beginning this ill-fated instrument, KIRMOS, which eventually became MOSFIRE. So it really seemed ridiculous that the people working on these projects were called Palomar engineers. So in 2002 I proposed to Tom Soifer as observatory council chair that we change the name

of the observatory to Caltech Optical Observatories, so that we could have a unified model. Then in 2003, Brucato retired, which was a sad moment. He was a great guy. To replace him, I appointed two deputy directors. Rich [Richard] Dekany came from JPL, as the associate director of development, and, after a search, we hired Andrew Pickles, who'd run the University of Hawaii Observatory, as associate director for observatory operations. I did one extra year as director, and by then I was glad to get out of it. I remember having a discussion with Koonin when he came through campus in 2005 and telling him that when people came into my office to discuss something about Palomar or Keck, as soon as I saw their faces, I knew what the topic would be, and he said, "Yeah, I know that feeling. When you get to that point, it's time to move on and do something else."

ASPATURIAN: You could tell by looking at them?

ELLIS: Yes. If it was Djorgovski, he was going to complain about observing time. If it was Shri, it was something to do with the way things were run. But overall, I enjoyed it. The best bit about the whole directorship was going up to Palomar. We had an open house day in June 2005, which was an enormous success. We had hundreds of people coming to see the 200-inch and riding in the elevator up to the prime focus. The other thing I'm proud of from my Palomar time is that I hired Scott Kardel as the observatory's first public affairs coordinator. That was a new departure.

ASPATURIAN: What prompted that?

ELLIS: The plan for the future of Palomar envisioned that around 2018 Palomar would become something like a super Griffith Observatory, and one question was, What are we going to do with that land? Caltech owns the property, and it's worth a fortune. There was so much demand from people to see Palomar, and I had thought there was a market for a bigger visitors' center there—the existing visitor center was closed at the time I arrived as director, because, according to Brucato, it was an embarrassment. We talked to people from the U.S. Forest Service about it, and there was a woman in the agency who was interested in a joint initiative with us. So we took it quite seriously. We had a

search for a public relations person, and we found Scott Kardel. He had a degree in astronomy and was tremendously energetic. I really enjoyed working with him. He set up a system of docents and trained them to help on viewing nights. We gave a bit of 60-inch time away, so that the public could look through that telescope. We completely refurbished the visitors' center and when we reopened it, we created a Friends of Palomar with donations for the visitor's center. Scott actually lived on the mountain with his family. There's a local school, but his kids got older, and what are teenagers going to do on that mountain? It's a remote place to live at that age, so eventually he and his family moved on. He's now managing director of the International Campaign for Dark Skies In Tucson. After he left, I think Shri, who had become the director by that time, felt the budget just couldn't sustain a replacement. So he gave Scott's duties to Dan McKenzie, the replacement for Thicksten, and I don't know how well that's actually worked.

ASPATURIAN: What science was going on at Palomar when you were there?

ELLIS: Well. The biggest news was that Mike Brown was finding these things in the outer solar system with the 48-inch Schmidt Telescope. There was a bit of tension over this because the 48-inch was now known as the Samuel Oschin–Schmidt, and this was because of a big donation from Samuel Oschin, who also funded the Oschin Air and Space Center, which the California Science Center is building to permanently house *Space Shuttle Endeavor.* He passed away in 2003, but his wife, Linda Oschin, was quite a difficult woman. When she saw all these discoveries coming from *her husband's* telescope, she wanted these edge-of-solar-system objects named after her husband.

ASPATURIAN: All of them?

ELLIS: [Laughter] Some of them, like Quaoar—I can't remember all their names. Mike Brown had such peculiar names for his objects.

ASPATURIAN: Sedna was another.

ELLIS: Sedna, thank you. She obviously fell out with Mike over this, and I became the middle man. I had to help Mike, so I used to phone her and try to placate her. I was trying to be nice, saying, "I'm sorry, Linda, there's a tradition in how we name these solar system objects." But she couldn't get it. What else? Well, WIRC was a huge hit. For the first time Palomar was doing surveys of distant galaxies in the infrared.

ASPATURIAN: There was enough visibility?

ELLIS: There was, yes. The weather at Palomar can be pretty good. The trouble is, in the summer it gets warm. In the infrared, the longer the wavelength, the more you're affected by temperature. But for certain observations in the shorter bands, Palomar was competitive, especially with this big camera. So there were a number of postdocs who were looking at clusters of galaxies with the camera. After he came, Rich Dekany negotiated access to a laser, so we started doing adaptive optics experiments at Palomar. That was going really well, but then the economic downturn hit the observatory, and its budget was cut. Sometime after Shri came in as director in 2006, he had a meeting where he said, "Richard's put a lot of effort with Dekany into developing adaptive optics. We have to make a decision how many nights per year we can afford to host it." If you project a laser into the sky, you have to have plane spotters, and this whole thing was getting so expensive, so that era came to an end, unfortunately, in about 2007. You can still do adaptive optics without a laser, but that restricts the areas of the sky that you can look at. So there was a resurgence of interest in Palomar, but now Keck was in full flight, with two telescopes, so it was inevitable that more people were interested in Keck than in Palomar. To his credit, I think Shri has continued to improve Palomar, and it's attracted a different clientele. His emphasis has been what we call transient science—supernovae, things that move and change and go bang, and you don't need Keck for many of those science programs. That area has blossomed.

ASPATURIAN: It's actually a very clever use of the observatory.

ELLIS: It's good use of all the telescopes because each one has its own little niche. I was never that much excited with that area of astronomy myself. But that subject really has unfolded in the last five years or so.

ASPATURIAN: I wanted to ask you, coming from Durham and Cambridge to an American university, was there an adjustment for you?

ELLIS: Oh, goodness, yes. In Britain, things were more orderly. For instance if we did a faculty hire, candidates would all come on the same day, would meet one another, and all be interviewed that day by a committee. Then there'd be a decision that day or the next. So the first thing that puzzled me at Caltech was this laborious process of hiring people. Andrew Blain and Lynne Hillenbrand [professor of astronomy] were the first people who were hired in Caltech astronomy when I was here. And it was just so tedious. Candidates would come through, but interviewing them was regarded as an insult. You couldn't possibly interview somebody—how demeaning! And then, of course, some members of the faculty were here for one candidate but not for another, so how could they make a comparison? In the final analysis, it didn't seem rigorous to me. This rather disorderly aspect of the day-to-day running of the department was one difference. Another was an astonishing lack of understanding of the importance of secretaries or assistants. I remember going to Tombrello when I was Palomar director and saying, "I'm won't be able to manage with just the secretaries in Robinson." And he looked at me and said, "Thank God, somebody has made a sensible request. I hereby give you fifty percent of a secretary; find the rest from the department." And I remember going around and saying, "We need to find money to match this half." This was how we got Judy McClain. I remember talking about this with Chuck Steidel, and he said, "Well, I don't really need a secretary." And I said, "How do you type your letters of reference?" He said, "I do it myself." "Well, how do you book your air tickets? And what about setting up meetings and everything?" It occurred to me that most of these people had never really learned how to deal with an assistant. In Britain, this was just standard. It puzzled me, because this was Caltech where the mentality was supposed to be that the professor's role was to focus on what they did best, which is thinking about science and teaching it. And here

were these professors doing all their own administration because it never occurred to them that they could off-load this. When Judy came, I remember, it was like a light suddenly went on for many people. I remember Chuck saying, "Judy's great. I don't know how we managed without her." So there were some aspects of American academic life that I just couldn't understand. I'm trying to think of further examples.

ASPATURIAN: Earlier, when we were talking about the almost innately collaborative nature of work in the British universities, you mentioned that you didn't see that here.

ELLIS: Every astronomy professor at Caltech has his or her own program. It's extraordinarily rare to see two professors work together. To his credit, Shri has managed it with the Palomar activity, and people rallied behind him when he started doing this—Tom [Thomas A.] Prince [professor of physics] and Sterl Phinney [professor of theoretical astrophysics]. And when I arrived here, it did occur to me that Chuck Steidel and I could work together. Although, when I was still at Cambridge, he and I were once asked to write a joint article on distant galaxies, and in the end, it never worked out; Chuck never put much effort into it. So I got the message that Chuck was not that kind of guy. Djorgovski and Kulkarni worked together, and that eventually fell apart. Yes, in England there was much more camaraderie. Caltech has not really lived up to that, unfortunately. Within the U.S., it's a completely different game. Joining forces with Harvard or Princeton doesn't seem to be an agenda item anywhere.

Now, with the Subaru spectrograph I'm building, we are finally collaborating with Princeton. That's because the scale of the projects that we now have to do post-Keck are so great that we just couldn't possibly go it alone. TMT is another example. Clearly we're working very hard with the University of California.

ASPATURIAN: You can't maintain insularity.

ELLIS: Yeah, insularity is not possible.

ASPATURIAN: How about your relationships with other wavelength astronomers? The radio, the infrared?

ELLIS: So Nick Scoville and Tony [Anthony C. S.] Readhead [Robinson Professor of Astronomy], and Anneila Sargent of course, I got to know well. But my first two to three years here I was so immersed in sorting out Palomar that I had very little time to get to know other areas of astronomy. I didn't even visit Owens Valley [Owens Valley Radio Observatory] probably until three or four years into my time at Caltech. I eventually went up there and asked for a tour, but I never have observed there. I think that is one of the biggest challenges we have at Caltech is that people are so busy that they don't often have the quality time to get to know other people's work, visit the other sites. I've never been inside the [Caltech] Submillimeter Observatory dome on Mauna Kea, for instance. Tom [Thomas G.] Phillips [MacArthur Professor of Physics, Emeritus, and CSO director] I never really got to know before he retired. We had PMA visiting committees under Tombrello's watch, and we've had them recently under Soifer's watch. One of the observations of the visiting committee was that the Caltech faculty were overworked and didn't have enough time to think. I remember Tombrello said, "What a ridiculous comment. It's always been like this." [Laughter]

ASPATURIAN: In his oral history Tombrello compared the PMA division to Canada and the astronomers to Quebec and its sometime separatists—"bombs in the mailbox, that kind of thing." Would you like to comment on that?

ELLIS: For me to survive as director and for me to survive as a professor even now, you have to have a very thick skin and a sense of humor. Never a week goes by without some temper tantrum or some ridiculous statement or crisis. There are some difficult professors with strong personalities, but even the most well-balanced people can get upset. Actually after all these years, in some sense, it keeps you young and fit. You think, "What's going to happen this week? I would go home and talk to Barbara, and she'd say, "Well, how did it go today?" I would say, "Absolutely hilarious! Guess what's happened?" And we would laugh over it. Somehow, everybody is working so hard, and the progress is so amazing that it makes up for all these difficulties.

ASPATURIAN: What about your relations with other areas of the Institute. Have you served on committees?

ELLIS: No. When Andrew Lange was division chair, and I came back from Oxford with his help in 2008, I told him that I didn't feel I'd integrated into Caltech outside Cahill. I'd never been on any Institute committee. I'd never been to a trustees' meeting. The first thing he said was, "Be careful of what you wish for." I'm not pushing it anymore because I enjoy doing my science. But, yes, in Cambridge, I could walk around the city, and everybody knew me because I was head of the astronomy institute**.** I was on all the central committees; the vice chancellor knew me. Here, because I was a key figure on TMT, I knew the provost and the president, and I'd always had good relations with them. But teaching committees, faculty committees, central committees—no; I was never on any of them, aside from search committees for astronomy. So my interface with physics has been pretty weak.

ASPATURIAN: Speaking of search committees, has the caliber of candidates for jobs and graduate students and so forth remained consistent in your years here?

ELLIS: Let's start with graduate students. I've served on graduate student recruitment committees, and the standard is very high. The graduate students are truly excellent. I would say they're slightly above Cambridge caliber, but not in a completely different league. They were definitely better than the students that I saw at Oxford. The challenge is getting enough per year. There's a bit of tension because students are expensive, but of course they're the lifeblood of any academic department. Sometimes we do very well; sometimes there aren't enough to go around. Judging how many offers to make is the big issue. But overall I've been impressed, although one of the difficulties I had serving on the committee was that I found it very hard to understand all these grades and test scores that undergraduates get in this country. This was gobbledygook to me; I didn't know what these meant. In Britain, the country is so small that we would interview students.

ASPATURIAN: And everybody knew everyone else, I suppose.

ELLIS: Everyone knew everyone else's references and so forth. Something else I learned in America for both graduate students and faculty recruitment is that it's very hard to judge recommendation letters. Some of them are off-scale—"The best person since

Einstein." Give me a break. Letters from some institutions are traditionally gold-plated. What do you do? You find that these letters are not realistic, but people are swayed by them. Often you see a ricochet effect where, say, the candidate for a junior faculty position has obviously got very good letters and one institution will move and then suddenly loads of other institutions will move as well, making offers simultaneously. It's as if they have no judgment of their own; they're just all following the crowd. I have found that a little unnerving. I think at Caltech we've suffered from this reputation of driving our students very hard, particularly in their first year with course work, and during my first several years at Caltech people were nervous about applying here because of that. I think the students themselves have helped us get out of this pit. Under Lynne Hillenbrand's watch as executive officer for astronomy—and she gets credit for this—the students produced a brochure about what it's like to be a graduate student in astronomy, which was a very nice piece of work. With faculty recruitment, there's just this slight worry I have that we set the bar so high, especially in theory, that nobody's good enough for us. There's this attitude of "if Martin Rees will apply, we'll consider him; otherwise, we'll just pass, thank you." That seems counter-productive. I haven't been directly involved in theory searches, but generally that's the impression. We've lost a lot of theorists in the last few years.

ASPATURIAN: Who were never hired or who just left?

ELLIS: We were very unfortunate. We've had two two-body problems recently. One was Chris Hirata and his wife.

ASPATURIAN: Oh, I remember Chris when he was an undergraduate.

ELLIS: He was a big star here, and he went to Ohio State. And before that, Marc Kamionkowski, who arrived when I arrived, so I got to know him very well and really admired him. He left with his wife to go to Johns Hopkins. And of course Blandford left.

ASPATURIAN: Astronomy is not the huge potential moneymaker that other scientific disciplines, such as biology and, in certain respects, physics, have become. Do you think this has had an impact on the caliber of people who want to go into the field these days? Do students think, well, that's great, but there's no future in it compared to what I could accomplish financially somewhere else?

ELLIS: Yes, I think that's happening. And it's happening more as people see the trajectory to get a tenure-track position. Because of the downturn, in the last five years, there have been fewer faculty positions than there were, although it's improving now that a lot of people from the good old days are hitting retirement age. But I think over the time I've been at Caltech and at Cambridge, the leakage of graduate students into finance and industry and other things has actually been pretty modest. How much attrition from undergraduate to post-graduate, it's difficult for me to judge. But you're right, it's definitely an issue. What I'm struck by is how many of the students we accept now are from overseas, especially Asia. That's become a big thing.

ASPATURIAN: What courses have you taught here?

ELLIS: Over the years, I've taught stellar structure and stellar evolution. I inherited that course from Judy Cohen, and I like it very much. It's something that relates back to my PhD thesis on stars, and I've put a lot of effort into it. I've taught the galaxy course, obviously. I prefer to teach on my own. Sometimes at Caltech, you share a course, which I've found disappointing because you don't control the whole syllabus. I like to be organized; I like to have written notes; and I like those notes to become the students' notes, so that they see the structure of the course, with the equations all numbered. If I teach with somebody, I find this just messes everything up, so I'm not very comfortable. I've taught instrument lab. That was hard work, actually, and took lot of time. I've taught Topics in Cosmology. Classes here are good, and we have teaching assistants who are very dedicated. I'm impressed with how seriously the students take it. With a similar arrangement in Cambridge, I found the graduate students didn't take things so seriously. They thought, "I don't have to do well in this course. I'll skip a few lectures."

ASPATURIAN: You don't find that here? They're more conscientious?

ELLIS: Students are dedicated. We get undergraduates coming to the graduate lectures. I'm amazed—they do well. Some of the undergraduates are truly spectacular.

ASPATURIAN: Have you worked with any SURF [Summer Undergraduate Research Fellowship] students?

ELLIS: Yes, I have. I like the SURF program, something that we didn't have in England. It's well run. It's a great opportunity. I think it's wonderful that SURF is not just a Caltech thing and that students come from everywhere. One of my SURF students, an Italian, eventually became my graduate student.

ASPATURIAN: Who was this?

ELLIS: His name is Sirio Belli. He came to us from Bologna, and he's working with me right now. Most of the SURF students I've had have stayed in the field. One went to Heidelberg as a graduate student. Another went to Melbourne. And I've kept in touch with them. What strikes me about these undergraduates is that they're so polite. They come to talk with you, and they knock on your door and call you professor. There isn't this hierarchy in England, and I imagined America was much more horizontal and that people would call me Richard or Rich or Rick. So it surprised me when these undergraduates would address me as professor. I found this amusing.

ASPATURIAN: Sometimes in the various branches of astronomy, there's tension between the observers and the cosmologists on one hand and the instrumentalists on the other. Did you personally ever experience any of that in your career?

ELLIS: Not really. I think occasionally at conferences the theorists are regarded as the more important people because they have the vision of what it all means, but over the development of my career, the observations have become so important. When I began, we were just counting galaxies, and we had to interpret what we saw just from that. So

clearly in those cases, somebody who had a theoretical vision, modeled from computer simulations, of how a structure forms in the universe was probably given more attention at meetings than some Mickey Mouse observer who got a photograph and just counted a few galaxies. But now we've seen the microwave background, we've measured the Hubble constant, we've found the universe is accelerating, and we've found the most distant galaxies.

ASPATURIAN: The universe has become such a magnificent laboratory.

ELLIS: It's such a huge achievement over the last twenty years in observations that now the observers can't be ignored. There have been surprises. The accelerating universe wasn't predicted. Gamma ray bursts weren't foreseen. It's interesting—if you go back to the science case for the Keck Telescope in 1985 and then you look at what Keck has accomplished, we actually did much better than we predicted we would. I do think instrumentalists get a hard time. Papers on instruments themselves, some journals won't publish. I think that's a great shame because instrumentation is crucial. It drives the field.

ASPATURIAN: I'd like to jump forward for just a minute and ask about a couple of other signal honors you achieved. The Gold Medal of the Royal Astronomical Society, and of course the CBE [Commander of the Most Excellent Order of the British Empire]. Why don't we take the gold medal first? This was in 2011, I believe.

ELLIS: Yes, I was sixty. Well, of all the prizes I've had—not that I've had that many—the gold medal is the one that has given me the most personal satisfaction, partly because it showed that people in Britain still respect me, even though I, as Martin Rees put it, defected. When I got the phone call from Roger Davies, the RAS president, I was surprised and very touched. It's a delightful prize, and you don't have to do anything—you don't even have to give a lecture.

ASPATURIAN: You just have to show up.

ELLIS: The only sad aspect was where they held the meeting. Every year, like the American Astronomical Society, the RAS chooses a city for its national astronomy meeting. And that year it was in Llandudno, in North Wales, five miles from my hometown, where I grew up. And my mother had died a few years before. If I had gone there to collect the medal, and my mother had been in the front row, that would have been so nice, but it wasn't to be. But another thing about the medal that touched me very much is that afterward Tom Soifer organized a dinner on campus, and both Stolper and Chameau came with their wives. That was nice.

ASPATURIAN: What was the medal awarded for specifically?

ELLIS: It's a lifetime medal. The citation mentioned my work in distant galaxies, but also the 2-degree field survey in Australia, and my students, I think. One never knows how these medals are decided. Subsequently I got put on the awards committee, and we gave Roger Blandford the gold medal last year. So I saw how it works. The committee itself is proactive. I often feel it's important that the awarding committees for these prizes are proactive and don't just respond to people's friends saying, "Hey, nominate this or that person for this medal." I'm not saying this happens all the time, but I think it's very important that every possible candidate is examined. I was pleased to see that the RAS awards committee takes its role very seriously.

ASPATURIAN: Are there any particular challenges that arise from being essentially a trans-Atlantic astronomer?

ELLIS: I still go to Britain every year. Barbara thinks that's one reason why I haven't integrated into Caltech as much as I should. I enjoy the international aspect of my career and being an international traveler very much, but I do think that maybe the downside is that my roots in Caltech are not as deep as they would otherwise be. Another reason is that I arrived here when I was fifty. A lot of these people who know each other very well go back decades.

ASPATURIAN: Yes, they do, and their mentors go back decades also.

ELLIS: So, you know, here comes this fifty-year-old from England. I had no history of being a student here; I didn't come through first as a postdoc. So that's not the heritage. I suppose with a lot of effort, I could have invented it, put effort into it, but I decided that my horizons were always on the international scene.

ASPATURIAN: I do think you are the only member of the faculty ever to be named a Commander of the British Empire.

ELLIS: Probably. [Laughter].

ASPATURIAN: This was in 2008. What was that like?

ELLIS: Well, it was great. And it was a bit of an eye opener that the reaction at Caltech was warmer and more amusing and livelier than the reaction in Britain. I was observing in Hawaii, and of course I was on a night schedule, and when I woke up there was a voice mail from the Los Angeles consul general of the British embassy. I struggled out of bed and phoned him up, thinking he wanted me to serve on a committee or something, and he said, "Well, I have many duties, but this is the most pleasant one." I was gob-smacked! I couldn't believe it.

ASPATURIAN: What did he say?

ELLIS: He said, "I have to ask you whether you would accept this title because not everybody does." I said, "Well, this is just an amazing honor because I'm not in Britain anymore." He said, "That's why I'm phoning you. If you accept, you have a choice: You can go and collect it at the British Embassy in Washington, but most people want to go to Buckingham Palace." So I said, "Yeah, Okay." He said, "It is very important that you tell nobody until it appears in the newspaper on December 31." This is the queen's new year's honors list. There are two of these lists per year. The other is the prime minister's honors list, which is on the queen's birthday and then there's this. I said, "What about my wife?" He said, "Okay, you can tell your wife." So of course I wanted to tell other people, but I couldn't. This happened in November, and Barbara and I had to

keep it quiet. The children came to California to spend Christmas with us that year, and we did tell them when they arrived. Then we got online on New Year's Eve, and there was the list. Now, as it happens, it's very rare to get this honor if you live outside Britain or a Commonwealth country. There were only two of us that year, and we were in what is called an appendage of the list. So when it appeared on the webpage, you had to scroll to the very bottom to find the additional countries' list, and most people in Britain didn't spot it. Then Caltech did a press release, as did the Keck Observatory, which was quite amusing. People here thought it was so hilarious. I remember Djorgovski coming into my office saying, "Commander of the British Empire! What British empire?" Then you get all these formal letters from Buckingham Palace telling you that there are these various dates. You never know which member of the royalty is going to confer the award. It could be the Queen. It could be Prince Charles. It could be Princess Margaret.

ASPATURIAN: She was dead, I think.

ELLIS: Princess Anne, I'm sorry.

ASPATURIAN: Oh my goodness.

ELLIS: Yeah, Princess Anne wouldn't have been great. So anyway, we chose a date in the summer. There are rules about how many people you can bring: I could bring Barbara, Hilary, and Tom. We all had to get the right clothes. It turned out it was Prince Charles, which was nice. The way it works, there are various levels. The knighthood of course is the most distinguished. Then the commander is the next one. Then there's officer, and then there's member.

ASPATURIAN: Which the Beatles got.

ELLIS: So the knights and the commanders are taken away into a room for a rehearsal. You're told what to say and what not to say. You're to address Prince Charles as Your Royal Highness, but after that you can say, Sir. Don't bring up Lady Diana. All this kind of stuff.

ASPATURIAN: They really give you a catechism like that?

ELLIS: They go through it. It's all light-hearted, not pompous. I met fellow commanders and knights. We all line up in order, with the knights first, then the commanders, and so forth. It's a chain of people going to meet the prince. And of course there's an audience, which Barbara and the kids were in. They announce the honor and the person. I was quite nervous. When I finally got to meet Prince Charles, I don't know what came into my head. I just said, "How's it going?" or something like that. He was a bit struck by the informality, and he said, "Well, I'm still here." And then I mentioned to him that he'd opened the Anglo-Australian Telescope in 1974. I wasn't there because I was still a student. And he said, "Oh, yes, I remember that. Did that telescope do anything useful?" I said, "Oh, absolutely!" Then he said to me, "Are you an intergalactic astronomer?"—the meaning would be extragalactic astronomer. I said, "Yes." And he said, "I'm really pleased that you got this award." And that was it. You get a nice medal and a little script that you can frame. My father was a Member of the British Empire. He went through this in the '50s.

ASPATURIAN: Because he saved his ship, I believe, during a typhoon.

ELLIS: That's right. Off Madagascar. So when he died, I inherited his MBE certificate. It's very similar. And it's amazing—the same queen was still the monarch fifty years later when I got my CBE.

ASPATURIAN: Your mother was alive for this particular honor?

ELLIS: No. Sadly. She would have been so proud. It's very unfortunate. My mother died just shy of her ninety-fourth birthday in 2007. So she missed all of that.

ASPATURIAN: You say the reaction in the U.S. was more good-natured than the reaction in Britain?

ELLIS: Well, in Britain people eventually spotted the announcement. The Royal Society sent me a nice letter. George Efstathiou, who was then the director of the Institute of Astronomy in Cambridge, wrote a nice letter on behalf of everybody at Cambridge. At the time I actually had one foot out of the door at Caltech. This is my love affair with Oxford.

ASPATURIAN: That's right, you were considering a move to Oxford.

ELLIS: So people at Oxford were very proud and made a big thing about it. Because I was arriving there, the news got into the Oxford alumnus magazine. And that's how most people in Britain finally spotted it.

ASPATURIAN: Did you have colleagues here calling you, "Your Commandership"?

ELLIS: Yeah, yeah. Tom Soifer is always bringing this up. He made a point of mentioning it when I gave a Watson lecture. Then there's this, "Do we have to salute you?" kind of stuff. You know, it's fine as long as people take it light-heartedly.

RICHARD ELLIS

SESSION 8

February 26, 2014

ASPATURIAN: It seems to me, looking at the last dozen years or so of your career, that the strands of your work in gravitational lensing, observational cosmology, galactic evolution, all start to converge in the early 2000s. Then you get also into the dark-ages epoch and more dark matter research. So let's talk about that.

ELLIS: At the time that I came from England, I think the redshift frontier in looking back was really set by Steidel. I was a bit in awe of Chuck. He had been Wal Sargent's graduate student here, and he'd been here several years on the faculty. He was very popular within the department, and admired. I remember people outside of Caltech saying, "Richard's too similar to Chuck. This is not going to work."

ASPATURIAN: Why would they have said that?

ELLIS: I had spent most of my time in the '90s studying galaxies out to redshifts of 1 and charting their history. And Chuck had developed a very clever technique—the Lyman-break technique—for finding objects out to redshift 3. So everybody said, "Well, what's Richard going to do? The territories are rather overlapping." Looking back now, I think that Chuck has stayed in the area that he championed and broadened his interests there in a number of ways. I like to think that I've covered more diverse territories. For example, I talked about this Wide Field Infrared Camera at Palomar [Session 7], which was the biggest we had. When you put a camera like that on the 200-inch, you can use it to chart the masses of large numbers of galaxies. So shortly after I arrived, a graduate student named Kevin Bundy, a very bright, very interesting guy—he's now in Japan— walked into my office. He'd been an undergraduate at Berkeley and was working with Peter Goldreich [DuBridge Professor of Astrophysics and Planetary Physics, Emeritus] on the solar system, but he said he wanted to work with me. Very flattering, but I said, "Well, I'm not sure I'm taking students." I don't know why, but I made it very difficult for him.

I kept sending him away. I'd had one unfortunate experience with a Caltech graduate student who left the field. So I was putting the defenses up. Anyway, he kept coming back—I couldn't send him away. In the end, I took him on, and it was just a fabulous partnership. He had something like sixty nights on the 200-inch with this new camera, and he just cleaned up. He basically measured the masses of about 8,000 galaxies.

ASPATURIAN: So this was a shift from his work with Goldreich.

ELLIS: Oh, yes, he completely changed territory. He was an all-rounder in every way. He went to the observatory himself. He designed the program. He wrote the pipeline for reducing the data. And he produced the first convincing history of the distribution of masses of galaxies with time out to redshifts of about 1.1—seven billion years back in cosmic history. It's one of the most cited papers that I've been involved in, with about five hundred citations now. He got the Trumpler Prize—this is a national prize—for the best PhD in North America.

ASPATURIAN: Cosmic archeology.

ELLIS: Yes. And it was for work at Palomar. I remember Chuck Steidel came into my office and said, "This camera is amazing. It's back to the good old days at Palomar, what Palomar is doing now." That project was very satisfying, and Kevin did very, very well. Now, staying in galaxy evolution, the next thing I did was with this Italian postdoc Tommaso Treu. He and I decided to use DEIMOS [DEep Imaging Multi-Object Spectrograph], a spectrograph that had been produced at Santa Cruz by Sandy [Sandra M.] Faber. It has a big field of view. So there had been an instrument at Keck called low-res [Low Resolution Imaging Spectrograph], which Judy Cohen and Bev [John Beverley] Oke [professor of astronomy, d. 2004] had produced. But there was so much demand for multi-object spectroscopy at Keck that they decided to have two instruments, one on each telescope. DEIMOS was put on Keck II, and we got in there right at the beginning. I said to Tommaso, "Let's do a big study of the dynamics of galaxies." Kevin Bundy, whose work we just talked about, was measuring what we call the stellar mass of a galaxy, which is how much light is coming out in stars. The dynamical mass is a more

profound quantity because it tells you about the dark matter as well. But it's much more demanding because you have to get the spectrum of the galaxy and measure the kinematic motions of the stars within the galaxy. Up until that time, there'd been maybe twenty or thirty distant galaxies for which we had these kind of data. I think Tommaso and I got a five-night allocation, which is not something you get very often, and they were all clear nights, every one. In that single run, and this is where Keck can be so powerful, we did like two hundred galaxies.

ASPATURIAN: What year are we talking about?

ELLIS: It was 2003 or '04. For the first time, you could see the mass distribution and the relationship between the dark matter and the stellar mass. You could see how old each individual galaxy was and how it had assembled. This survey gave us the first really convincing view of a subset of the galaxy population of what we call elliptical galaxies.

ASPATURIAN: You looked only at ellipticals?

ELLIS: Yes.

ASPATURIAN: Were they easier to gauge than the spirals?

ELLIS: No, in some sense they're harder because they only have stars, not gas, and so you need high-quality spectra. But this instrument, DEIMOS, has been an enormous success. Fair play to Santa Cruz: It's still one of the most exciting instruments on Keck.

ASPATURIAN: Why did you choose to focus on ellipticals?

ELLIS: Well, Tommaso for his PhD thesis had studied nearby ellipticals, and I had worked in ellipticals. They're appealingly simple. They're balls of stars. They're uniform in color from one to the next. And yet paradoxically the question is, Are they genuinely old objects? What we found is that the big ones are very old, but the little ones have been assembling more recently. This is a phenomenon that is sometimes called

downsizing. The big things form first and then the little things form latter. This is a big topic. My Italian student, Sirio Belli, has now extended the observations out to a redshift of 2.5. He's got over a hundred objects.

ASPATURIAN: Similar morphology?

ELLIS: Yes, similar morphology. So that's been very exciting.

ASPATURIAN: Was anyone else doing this work, or did you pretty much have this to yourself?

ELLIS: The work on ellipticals was in competition with a group in Europe based in Leiden Observatory. We were well aware of this. Their sample wasn't quite as big, but they found the same result: The massive galaxies form first. It's an interesting story, because at the time there was a Hubble fellow at Caltech, Pieter van Dokkum, who had done his PhD at Leiden and with whom I got on very well. Tommaso was my postdoc and Pieter was independent, but we involved him in our work, and he was on the Leiden paper as well. So with this sort of political issue, well, Pieter was very generous about it. He said, "Well, Tommaso's done very well here," and was happy to give Tommaso credit for this work. Pieter is now a professor in both the physics and astronomy departments at Yale. It's amazing where these young people end up—they come to Caltech for their PhDs or straight out of their PhDs; they get on to Keck, and look where they are now.

ASPATURIAN: Keck has made all these careers.

ELLIS: Keck made all these people. Tommaso was effectively the group leader at Santa Barbara and is now something of a "star professor" at UCLA, and Pieter van Dokkum is running the Yale astronomy group. It's amazing. What a great finishing school Caltech is.

ASPATURIAN: I wanted to ask you about gravitational lensing. Recently you've used it to spectacular effect in imaging very, very deep galaxy fields. You have done scrupulous and detailed work with gravitational lenses. Were you alone in pioneering this?

ELLIS: No.

ASPATURIAN: We've talked about the French school [Session 4], but—

ELLIS: Okay, so, lensing can be used in two ways. Lensing is the distortion of a background image by a foreground lump of dark matter or a galaxy cluster. So, just like optical lenses, you could use lensing as a magnifying glass to look at the distant universe, enlarging and brightening objects that would otherwise be out of view. And we've done that. Or you could use the background galaxies as a sort of screen and use their distortions to look at the dark matter in the lens itself. And we've done both. The second goal is telling us something about how the dark matter is distributed. One of the highlights of our work in that area involved a PhD student of mine at Cambridge, Richard Massey, who came to Caltech as a postdoc afterwards. Unusually, I jointly supervised him. I don't normally do this, but he was so good, and he wanted to come to Caltech as a postdoc. We worked with Hubble and with Nick Scoville, who had a big survey with Hubble called the COSMOS [Cosmic Evolution] survey. Nick was an amazing leader of this team because he's pretty laid back. Most team leaders are into organization and telecons and rules of how to organize things. Nick was into, if I might say it, having a great time: "Let's all enjoy ourselves doing this science." We'd go to conferences in Kyoto, and there'd be dinner parties. I don't know how he managed it, but anyhow we had a great time. At that time COSMOS was the biggest survey ever done with Hubble. So I had two postdocs—Richard Massey, my student from Cambridge, and Jason Rhodes, now a senior scientist at JPL. They were experts on converting these gravitational-lens distortions into a map of the dark matter. Hubble has so much higher fidelity than a ground-based telescope, and this was the first panoramic Hubble image. We got this onto the front cover of *Nature*, and it was a huge hit. It was the first 3D map of dark matter. I think that was about 2005.

ASPATURIAN: I have the press release here pegged to 2007 [*Nature*, January 7, 2007].

ELLIS: 2007, yes. And Richard Massey was just an expert at the media. He knew how to make these images. He could make movies: he could do fly-through simulations. The PR from this got everywhere. It was on the front covers of most of the dailies in Britain. I got so much e-mail. I would travel to Britain, and people would say that they were tired of reading about this 3D map, and I'd say, "Well, what can I say? Everybody loves to see a map of the dark matter." Not only could we make our own map of the dark matter, but because the field in this COSMOS area had been so well-studied at other wavelengths, we had an X-ray map that let us compare point by point where the dark matter was in relation to the gas. We could compare where it was with the stars and galaxies. The *Nature* paper compared these in a very nice way, and it showed, I think convincingly, that the dark matter was what we call the scaffolding around which galaxies and clusters assemble.

ASPATURIAN: Which had previously been hypothesized.

ELLIS: It had been hypothesized but never really demonstrated. You asked whether this was a competitive field. Yes. There were others who claimed that they had demonstrated this, but it had never been done so convincingly as with Hubble. Now the other aspect of lensing is this magnifying of distant objects, which also turned out to be successful. We were not unique, no. There was the French group in Paris, led by Yannick Mellier, who had moved there from Toulouse, and his collaborators. There was also a very strong lensing group in Germany, led by Peter Schneider. But as soon as I got to Caltech, I realized that there was an angle that I'd always wanted to pursue but never had the audacity to do. When you look through a cluster of galaxies that's acting as a lens, the magnification is not uniform across the cluster. There's a region that circles the cluster—we call it the critical line—where the magnification formally is infinite. Of course you don't realize an infinite magnification because the region is infinitely small. But as you approach this critical line, which is also sometimes known as a caustic, the magnification rockets up. So we didn't know what would lie in this region, but it occurred to me and my student Mike Santos that we might find spectacularly magnified

objects if we just blindly searched around this region with Keck. We were on our own. Nobody else was doing this.

ASPATURIAN: Why do you suppose that was?

ELLIS: I think the field was only emerging. Lensing as a tool rather than a theoretical fascination was only beginning to emerge in the late '90s. So we had an observing run—in fact, the first proposal I wrote when I got to Caltech, was about scouting these critical lines. We used the low-res spectrograph, and we only went to the clusters that had Hubble images, so we knew exactly where this critical line was. I had a visitor here who had been my postdoc in Cambridge, Jean-Paul Kneib, a Frenchman from the Toulouse group. As part of my start-up, I invited him to come to Caltech for two years, and we set out to do this mapping with Mike Santos. We hit gold pretty quickly. We found a galaxy in 2001, I think, that had a redshift of 5.7. And then later we found a triply imaged galaxy at a redshift 7. These were all right next to these critical lines—in the same cluster, as it happened. So this was a demonstration that this technique really did work, and that you could use it to find spectacularly magnified objects. And subsequently we've been employing this in a different way. I know most people are enthused by looking at the most distant object and finding magnified things that you would otherwise not be able to see. But closer to home, say at a redshift of 3, a galaxy is not only magnified, it's also enlarged on the sky. It's like you have a magnifying glass so you can see its internal structure more easily than you could any other way. So, for example, normally the kinds of galaxies that Chuck Steidel studies are just little dots on the sky. They're very small in angle so you wouldn't be able to resolve them. Do they have a nucleus, do they have spiral arms? Even with Hubble, you couldn't tell; they're too small. But if you place one of these behind a cluster of galaxies, it's magnified, and its angular size is enlarged. Suddenly you have a zoom lens that allows you to see the internal structure. If you have a spectrograph, it enables you to determine whether the galaxy is rotating. You can measure the chemical composition across it. So Tucker Jones, a fabulous student, who came to us from MIT—a true outdoor guy, who came to work on a skateboard, with a ponytail, and a really great guy to have at the telescope—

and I have been using lensing to magnify and hence measure the internal properties of high redshift galaxies. That's been another angle. So I think there are three angles I've studied on lensing: Charting the dark matter with Richard Massey; finding the very early objects with Jean-Paul Kneib, Mike Santos, and Dan Stark; and then studying the internal properties of galaxies at intermediate redshift with Tucker. There's yet another aspect where you can use gravitational lensing to look at the dark matter on very small scales in galaxies and clusters. I worked on that with David Sand and more recently, another graduate student, Drew Newman. Rather than making a map of the dark matter on panoramic scales, we wanted to know how it behaves on very small scales in the centers of clusters and galaxies. That behavior tells you about the nature of dark matter. If dark matter is what we call cold—that is, if it's made up of massive particles that don't interact with the stuff that makes up you and me—then the simulations suggest that the dark matter should be very sharply cusped in the center. And we—Drew Newman and to some extent my earlier student David Sand— showed that this is not the case. We don't see this cusp. This is now a widely accepted result, based primarily on Keck and Hubble data. There are two possible explanations for this. One, which is truly amazing, is that the dark matter has some interactive properties, and so is not completely "cold." Some people buy that explanation, but many people think it's too revolutionary. The other theory is that in these galaxies, explosions of supernovae have pushed out the dark matter. For example, if a black hole or supernova explosion injects energy into the center of a galaxy, that would redistribute the dark matter. It's still controversial, exactly what's going on here. So that's pretty well it, other than this latest work on reionization.

ASPATURIAN: Yes, I would like to talk about that because it is very interesting. There's a theorist you've worked with at Harvard, I believe.

ELLIS: Avi Loeb.

ASPATURIAN: Avi Loeb. Your observational work was tending toward this area anyway, but how did you get into this?

ELLIS: Okay. I think it goes back to Dan Stark, a brilliant student, originally from Wisconsin.

ASPATURIAN: He's the one who was written up with you in *Time* magazine.

ELLIS: That's right. You know, this relates to Steidel again. So there was Chuck measuring literally thousands of galaxies at redshifts 2 to 3. There was a little bit of a dance around this with him, and I waited to see whether he was going to push farther out or not. Up to this point, I had got out to redshift 7 with my lensing work, but I had not done what I would call a systematic survey of very high redshift galaxies. I'd probed this cute technique—the caustic magnification—to show that they were there and demonstrated how unique this information was, but Chuck was still at redshift 3 and up until then in terms of survey work, I had really been working a lower redshift. Then Dan Stark turned up, and I thought, "Well, I'm going to go out there now and do a big survey beyond the redshift 3." So Dan and I began a survey out to redshift 7. It started as an earlier program with an Oxford collaborator, Andy Bunker, but basically Dan and I took that over and began working together in 2005, 2006. Dan was fabulous. We used DEIMOS, and we slowly built up a sample of about 600 galaxies between redshifts 3.5 and 7.

ASPATURIAN: What was the title of his thesis if you remember?

ELLIS: It was "Observing Galaxy Formation in the First Two Billion Years [2009]."

ASPATURIAN: Okay. So an early epoch.

ELLIS: Oh yes, and you know, Dan was just amazing. Basically, it was a period of great excitement. We were looking farther back than anybody else in a systematic way. No lensing—just using the colors to select the very earliest objects. The story with theory is interesting. Avi Loeb I've always admired. He's now head of the astronomy department at Harvard. But he's been interested in what we call reionization for many years and we used to meet at conferences. Well, at the end of Dan's thesis I said to him, "It would do

you good to go to Harvard for a couple of months." He gave a colloquium there, and we wrote papers with Avi Loeb. Later, a former Harvard student, a very talented theorist called Brant Robertson, came to Caltech as a Hubble Fellow. And Brant, Dan, and I decided that we wanted to probe beyond redshift 7 into what we call the reionization era. So we wrote a Hubble proposal.

ASPATURIAN: Will you explain briefly what reionization is.

ELLIS: What is reionization? When the universe was very young, there were no stars and galaxies. There was just hydrogen gas. This hydrogen gas is neutral—that is, it's in atomic and molecular form. There's also dark matter, which is already clumped. So the hydrogen clouds fall into this dark matter by gravity, and eventually these hydrogen clouds collapse under their own weight. They get hot in the center and they ignite and form stars. So that's what we call the cosmic dawn. But the first stars are very hot because they don't have any metals, just hydrogen and helium. So because they're hot, they emit copious amounts of ultraviolet light, which breaks hydrogen apart again into a proton and electron, by what we call photoionization. This hydrogen atom was there and then suddenly, as soon as the first stars and galaxies form, it's split up again. So it's what we call reionization—this transition from a neutral hydrogen in deep space to an ionized gas.

ASPATURIAN: Over how long a period of time was this thought to go on?

ELLIS: Well, nobody knew. We now know from our observations that it probably lasts about two or three hundred million years. I think Dan and his successor student, Matt Schenker, who's just finishing up, have demonstrated that we've detected the last phases of this reionization at a redshift 6 to 7, which is about a billion years after the Big Bang.

ASPATURIAN: So Dan came back from Harvard.

ELLIS: Yes. Brant Robertson was here, and we decided that we would put in a Hubble proposal to get a very deep image of the sky that would allow us to quantify for the first

time whether there were enough galaxies at this epoch to sustain this reionization. We had a suspicion that the redshift period from 7 to 12 was the critical period when galaxies were forming ferociously, and that energy from these young galaxies would burn off the hydrogen and make it ionized.

ASPATURIAN: So you'd be watching kind of the transition from the dark ages to the cosmic dawn.

ELLIS: That's exactly right. Hubble got a new camera in 2009, and its early data became public and we analyzed it. We used that data to justify an even deeper image in 2012, called the Hubble Ultra Deep Field, which provided the first meaningful census of galaxies beyond a redshift of 7. Dan by now had left.

ASPATURIAN: I want to step back for one moment to this *Time* magazine story ["How the Stars Were Born," August 7, 2006], which opened with you and Dan observing at Keck, and which I think was pegged to this research to some degree.

ELLIS: It was, yes. We were doing a lensing search. We were trying to determine whether there were any objects at these great distances, just to show that there were galaxies at this time. When the article came out, we were driving across the country to Toronto, Canada, where I was doing a sabbatical. My son was in Boston at the time and sent me a message that not only was I in *Time* magazine, but Dan and I were on the cover. We were in Wyoming, where you can't buy *Time* magazine. Well, maybe you can in the state capital, but we couldn't find it anywhere. I think we finally got to—what's the state west of Wisconsin?

ASPATURIAN: West of Wisconsin? Minnesota, I think.

ELLIS: Minnesota. We got to Minnesota.

ASPATURIAN: Minneapolis?

ELLIS: I don't think we went through Minneapolis. But we got to some part of civilization, and Barbara went into a bookstore and brought ten copies of this *Time* magazine article. But it was so funny because for this *Time* magazine article—the author was very nice guy, called Mike [Michael] Lemonick.

ASPATURIAN: He's a very good writer.

ELLIS: He came to Keck and watched us observe. You never know what's going to happen when a journalist comes and sits with you on an observing run. At the beginning of the night with Keck, you have to enter your name into the log, and Dan misspelled his name, and instead of hitting backspace, he hit delete. You know, there's always this confusion on the keyboard about how to delete a character—do you go back, or do you hit delete? Whichever it was, he hit the wrong one, and the software that writes the header for each image couldn't dissect this spurious character. So the telescope wouldn't store any images. So we couldn't focus the telescope.

ASPATURIAN: A multi-million dollar instrument, and this tiny glitch hangs you up.

ELLIS: Right. So the magazine article starts with Richard Ellis, Welshman with a temper, pacing up and down, cursing the telescope that won't focus.

ASPATURIAN: The language he has you using is, "We're stuffed."

ELLIS: "We're stuffed!"

ASPATURIAN: And I thought, could that really be what you said? [Laughter]

ELLIS: I don't know. I can get pretty worked up at the telescope when things go wrong. Anyway, we got a lot of publicity from that magazine article. I think Dan went into hiding. I don't know where he went. Dan is not a great fan of publicity. He's done very well though. He's now a professor at Arizona, so he can't complain.

ASPATURIAN: How did you happen to be chosen for this profile? Do you know?

ELLIS: Lemonick phoned me. He and I have had a number of conversations over the years. He called me and said he was very interested in the work that we were doing. He was going to do an article, and I said, "Why don't you come and observe?" And he said yes. You imagine these guys; they're freelance, so he pays his own way. He lives on the East Coast, I think, in New York or Boston. He flew all the way to Hawaii, and he stayed up all night with us. The survey that features in the *Time* magazine article is from redshift 7 to 10. We found a few examples to provide a proof of concept that these faint objects are out there. Then the Hubble image came in in 2012, and that's the most recent work we've done on charting this reionization history out to redshifts of 10 and beyond. The later student there is Matt Schenker.

ASPATURIAN: The *Time* article quoted some astronomer colleague of yours as saying, "It's always great fun to go to a meeting and see the latest Ellis most-distant-object sweepstakes entry." You're in work that now gets a lot of publicity. It's easy to understand. It's exciting. It relates to cosmic origins. Do you get any ribbing from your colleagues about this?

ELLIS: Well, okay. Let's confess a few things. One is that some of the objects that Dan and I published are very faint. So we try to recover them with other telescopes to see whether we can demonstrate that they really are robust objects, and some of them we haven't seen again. I remember George Djorgovski making a comment that the center of low signal-to-noise astronomy has now moved from Berkeley to Caltech. This was a sort of criticism that these objects are very, very faint. Unfortunately, this comment on the sweepstakes lottery is very accurate. Basically, there are many pitfalls out there in this high redshift game. It's so tempting to look at a little piece of noise or blip and say, "I think it's really there." So I think in the early euphoria from 2005 to 2009, there were many claims, and all of us made mistakes. The French had an object at redshift 10 that eventually went away. There was another paper describing an object at redshift 8.5 that went away. Some of Dan's objects that got into *Time* magazine can't be recovered.

Some of them are real, but some aren't. What's all changed, thankfully, is this new camera on Hubble, which finally demonstrated robust objects in this era.

ASPATURIAN: That didn't go away?

ELLIS: That didn't go away. And we all agree on that. There have been three groups championing these Hubble data sets—ourselves, a group in Santa Cruz headed by Garth Illingworth, and a group in Baltimore. We all agree. We compete, but we all agree.

ASPATURIAN: A couple of other press releases caught my eye, one dating back to 2008 that describes your discovery of a primordial spiral galaxy. That seemed like a really exciting result. And then the Himiko object, which is described as appearing to be composed almost exclusively of hydrogen and helium, so it's a very early object also.

ELLIS: Himiko is very interesting. In my many trips to Japan in the early 2000s, I was introduced to this student, Masami Ouchi, and he was pretty impressive. He was interested in very early galaxies. He's a nice guy, and we got on very well. He suddenly wrote to me one day and said that he wanted to apply to come to the United States as a Hubble Fellow. I said that's a very competitive process; there are usually three hundred applicants. He asked if I would write a letter of reference, and I did. He was the first Japanese applicant ever to get a Hubble Fellowship, and he went to Baltimore. Afterward I tried to hire him at Caltech. I was very disappointed—he went to Carnegie. I think he wanted to be a little bit distant from me. I think he thought if he came to Caltech, he would be under my thumb, which is not quite fair because I do give my postdocs complete freedom. Eventually he got a professorship in Tokyo. We've been colleagues for years now. And so, using the Subaru telescope, he found this amazing energetic galaxy by himself, and he called it Himiko after a historic and mysterious Japanese princess. It's a very distant object, redshift 6.6, but it's forming stars at a hundred times the rate of the Milky Way, which is amazing at that time in cosmic history. I remember telling him, "I think this object is so special; it couldn't sustain this activity very long, so we must be seeing it at a very special time in its own history." He wrote to me, and asked whether I would like to collaborate with him on following up on this object with Hubble

and with ALMA, a new telescope in Chile—the Atacama Large Millimeter Array—that allows you to look for dust—

ASPATURIAN: And gas, the star-forming regions.

ELLIS: Yes, that's right. My role here, to be completely frank, is that he wrote the proposal and his English isn't great, so I knocked this proposal grammatically into shape. We got the data, and it turns out Himiko is, as we imagined, not a regular object—it's three objects merging. It's a very rare triple merger of galaxies. But the most surprising thing was the data from ALMA, which shows that there are no heavy elements, at all.

ASPATURIAN: Is it the first object of its kind?

ELLIS: It's the first object that looks like what we call primordial in its composition. Carbon is the element that we looked for with ALMA, and we didn't see it. So it's an interesting object. It's got three little knots, and then there's this huge hydrogen nebula around it, which suggests that it's collapsing, but the source of star formation that triggers and powers this nebula is this very rare triple merger. As we imagined, we're seeing this early galaxy at a very spectacular moment in its history. It can't have been around for very long before that, either; otherwise it would have been enriched with supernovae. We had a press release through the ALMA office in Charlottesville. I think it's good news for ALMA, which has only finished construction in the last few months. We weren't using its full power at the time these observations were being made. So the prospects of finding more objects like Himiko with ALMA are pretty good.

ASPATURIAN: That's a very exciting result, I think.

ELLIS: It's a nice marriage between Ouchi's work and what we've been doing. It worked out very well.

ASPATURIAN: So what's next on the agenda for you?

ELLIS: In terms of this reionization era, we now have MOSFIRE, an infrared multi-object spectrograph on Keck. Up until now, most of the work that Schenker and Stark and I have done has been one object at a time. We used an infrared instrument called NIRSPEC, built by Ian McLean at UCLA. He and Chuck Steidel were the PIs of MOSFIRE; and it's forty times more powerful than NIRSPEC in efficiency and capability. So charting this epoch of redshift 7 to 9 with MOSFIRE is something that we're starting to do, but we've had very bad luck this season with the weather at Keck. The time-allocation committee has been very generous to us, but it's been a very bad season so far. We've had maybe twelve cloudy nights. But we will carry on.

ASPATURIAN: That must be frustrating when you have graduate students who are doing on-site work, because they're kind of at the mercy of the elements.

ELLIS: Well, as we've discussed, I'm a big fan of traveling to Keck rather than observing remotely, and I take the students there. Of course they get very worried about their theses when it's cloudy. My job is to cheer them up.

ASPATURIAN: What do you think are likely to be the big areas of astronomy in the next dozen to two dozen years?

ELLIS: Well, if one looks where the territory is going, if one compares where we are now with where we were ten years ago, a big area of excitement is transient science— things that go pop and move. Shri has been championing this at Palomar. Our new hire at Caltech, Mansi Kasliwal, is working in that. And there's real synergy there with LIGO. If LIGO eventually finds evidence of gravitational waves, then triggering a search for the electromagnetic signals that come from two merging black holes or a neutron star colliding with a black hole is something that's interesting. In the early 2020s there will be the supreme instrument for looking for transient objects, the Large Synoptic Survey Telescope. This was the telescope that got the top spot on the *Decadal Survey*. It displaced TMT, so very sad for TMT.

ASPATURIAN: Is that a ground-based telescope?

ELLIS: It's a ground-based telescope that's now more or less funded by the NSF. It's going to Chile.

ASPATURIAN: Is it optical?

ELLIS: It's an optical telescope with a big field of view. It will survey the sky very frequently and look for faint, moving, and variable objects. So that area, I think is a growth area. It began with supernova searches, but now it turns out that there's a much broader range of variable objects than we ever imagined. There are peculiar stars that we didn't realize existed. People didn't search in the time domain in a rigorous way. They knew about novae, they knew about variable stars, but nobody had really done a systematic search of the full parameter space. So that's an interesting area. I'm not involved in it, but I find it interesting that they're charting it. I don't yet see a synthesis with physics coming out from that area. It'll come in due course. Another area is exoplanets, a subject that has just completely exploded in the last ten years.

ASPATURIAN: Yes, because of the technology.

ELLIS: We have new technology to detect planets—the Kepler Satellite that looks for eclipsing objects, and now investment in TMT and the James Webb Space Telescope will help us image these planets, and maybe even measure the composition of their atmospheres. There again, we're in transition from a subject that has been, gee whiz, collecting as many objects as possible and charting the territory. I predict a consolidation of that area into physics pretty soon. We can't just keep finding exoplanets—we have to start understanding the whole history of planetary formation. And I see the beginnings of that in some of the talks I attend. So I'll say those two topics are major growth areas, along with reionization. What we've begun to explore tentatively there with Hubble and Keck will be so much more effectively done with James Webb.

ASPATURIAN: Which is due to be deployed—

ELLIS: 2018.

ASPATURIAN: Not long now.

ELLIS: I may, fingers crossed, see that era. TMT, well you know, 2021, 2022; it's a little bit farther down the road. Hopefully, TMT and James Webb will overlap, which will be useful. So those are the three areas I think are most exciting. Galaxy evolution in this whole sort of zero to 5 redshift range has now got very crowded, I find. Lots of students and postdocs, big teams. There was a conference recently in Aspen that I didn't go to, but I spoke to a senior guy, Ray [Raymond G.] Carlberg, who did, and I asked him what he thought. He said, "The subject is kind of dull. It's become like the weather—there's too much detail." So I think there is some scope for somebody, I don't know who, to stand back from all this now and wrap it up with some profound review that summarizes what we've learned. I see so many young people just chipping away at detail.

ASPATURIAN: So what areas would you steer graduate students into?

ELLIS: Well, we're recruiting grads right now. I interviewed a whole bunch yesterday and this morning. They're interested in reionization. Many of them like this idea of using lensing to enlarge a galaxy and study it in detail. They're saying that instead of just looking at counts of galaxies and their colors and star formation rates, let's look internally at a galaxy.

ASPATURIAN: Studying them as individuals?

ELLIS: Studying them as individuals in detail, which is now possible through lensing and adaptive optics. For instance, the development of spiral arms and discs and central regions of galaxies is something that we don't really have a good observational handle on at the moment, but that's possible with lensing and the capabilities that are coming up.

ASPATURIAN: At some point that will probably converge with exoplanet studies because you deal with stars, stellar systems, and so on.

ELLIS: Yes.

ASPATURIAN: One other thing I was wondering about is that you're a native son of Wales. How has your reputation and career in astronomy been treated there? Are they proud of you?

ELLIS: Yes, I think so. There are a few Welsh astronomers around the world, and we know each other. There's one very famous Welsh astronomer in London who appears on the BBC Wales television channel, and he gives talks in Welsh, which I could no longer do.

ASPATURIAN: What is his name?

ELLIS: Professor Iwan Williams at Queen Mary College. The only concrete evidence that I'm respected in Wales is that one day I got a phone call from the Welsh development office in New York.

ASPATURIAN: There is such a thing?

ELLIS: Yes. Wales is its own country and it's got its own promotional arm. Sometimes if you fly on British Airways, you see adverts for Invest in Wales—it's got great climate, nice people, good workforce, and all this kind of stuff. So anyway, this woman from the New York Welsh development office said that they were doing a brochure to promote famous Welsh people, and would I want to be in this brochure. I said, "Sure, yeah." They sent a photographer out, and they had a picture of me with a cluster of galaxies behind. Anyway this brochure eventually appeared, and I was in it, together with Catherine Zeta-Jones. [Laughter]

ASPATURIAN: Anthony Hopkins, I bet.

ELLIS: He's in it. So that was nice. That brochure is the only evidence I've got that I'm respected in Wales. Eventually it got made into a poster—famous Welsh people—and was sent to all the Welsh secondary schools, so presumably it's pinned up somewhere in some of the schools.

ASPATURIAN: But you don't have it?

ELLIS: I don't have the poster; I have the brochure. I've also been back to my high school. There was an article in the *Times* about my research—I think this was around 1997 when I was in Cambridge—and I got a phone call from the headmaster of the secondary school I went to, and he said that they read about me in the *Times* and would I like to come give a lecture at the school. So I went back to my old school, and that was nice. I gave a talk, and they had a local newspaper photographer come along and take photos of me with the school children. So that was cute.

ASPATURIAN: Anything else?

ELLIS: No. It's been most enjoyable. It's been therapeutic, I think.

ASPATURIAN: My goodness.

ELLIS: I feel like I've been to the psychiatrist. [Laughter]

ASPATURIAN: Not really.

ELLIS: And thank you very much, Heidi.

ASPATURIAN: It's been a pleasure.

APPENDIX

PERSONAL RECORDS OF FELLOWS AND FOREIGN MEMBER OF THE ROYAL SOCIETY

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PERSONAL RECORDS OF FELLOWS AND FOREIGN MEMBERS OF THE ROYAL SOCIETY

SECTION A. General Curriculum Vitae

1. Surname : followed by forenames.

ELLIS, RICHARD SALISBURY

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2. Birth : date and place.

25 MAY 1950 COLWYN BAY, WALES

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3. Parentage : number of brothers and sisters, and your place in the series; places of residence.

FATHER : CAPTAIN ARTHUR ELLIS, MBE — MERCHANT SEAMAN 1913 – 1990

MOTHER : MARION ELLIS (née DAVIES) 1913 –

#1 SISTER : CATRIN WILLIAMS (nee ELLIS) 1944 –

#2 SELF & NO FURTHER CHILDREN

RESIDENCE : OLD COLWYN, CLWYD, NORTH WALES

1950 – 52 MERVUE, COED COCH ROAD

1952 – 68 40 LLANELIAN ROAD

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4. Ancestry and relatives, with special reference to any who have been noteworthy in science, learning, public service, or other ways.

My father was a captain in the Merchant Navy and awarded an MBE for courage in saving his vessel during a hurricane in Madagascar in 1950.

My grandfather on my father's side was a famous Welsh Baptist minister, Humphrey Ellis.

The lineage on my father's side is not clear to me

My mother's family was poorer although some eminent people resulted. Her great uncle was Sir Henry Jones (1852 – 1922) who became Professor of Moral Philosophy at Glasgow in 1894.

My grandfather on my mother's side, William Davies, was the son of Ann Salisbury – hence my middle name – a direct descendent of William Salesbury who translated the New Testament into Welsh following an Act of Parliament in 1567.

5. Marriage: date and place, spouse's full name and parentage, with relatives of scientific or public interest, offspring, name and sex of each with brief details of careers.

28 JULY 1972 OXFORD REGISTER OFFICE, UK

BARBARA WILLIAMS - born out of wedlock to Ruth Kastheuser of Braunschweig, Germany and later adopted by her and her husband George Anthony Williams. True father David Catto of Edinburgh.

CHILDREN:

HILARY RHONA ELLIS b 9 AUG 1976 FEMALE
DURHAM JOHNSTON COMPRENENSIVE 1987-93 (DURHAM)
HILLS RD SIXTH FORM COLLEGE 1993-94 (CAMBRIDGE
UNIVERSITY COLLEGE LONDON 1994 - present

THOMAS MARC ELLIS b 5 JAN 1978 MALE
DURHAM JOHNSTON COMPREHENSIVE 1989-92 (DURHAM)
NETHERHALL COMPREHENSIVE 1993-94 (CAMBRIDGE
HILLS RD SIXTH FORM COLLEGE 1994 - present (")

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6. Circumstances, influences and relevant memories of childhood.

See Section B

PERSONAL RECORD

SECTION A (cont.)

7. Schools: primary and secondary, with dates. Entrance or leaving scholarships; scientific education and opportunities, if any.

DLD COLWYN COUNTY PRIMARY SCHOOL 1954 – 61
11 plus examination 1961

ABERGELE GRAMMAR SCHOOL (later YSGOL EMRYS ap IWAN COMPREHENSIVE) 1961 – 68

GCE 'O' Level subjects & grades (Welsh Joint Educ. Ctee)
MATHS (incl CALCULUS & COORDINATE GEOM) 1
FRENCH 1
PHYSICS 2 CHEMISTRY 2 HISTORY 2 GEOGRAPHY 2
ENGLISH LANG 2 WELSH (O2 – intermediate level) 2
ENGLISH LIT 4

GCE 'A' Level subjects & grades (WJEC)
PHYSICS A
APPLIED MATHS A
PURE MATHS C

8. University and other higher education : scholarships; subjects of study; prizes and distinctions; degrees, with dates; undergraduate researches, if any.

UNIVERSITY COLLEGE LONDON 1968 – 1971
DEPARTMENT OF ASTRONOMY (PROF. C.W. ALLEN)
B.Sc. (Hons) First Class
1970 Huggins Astronomy Prize
1971 Faculty of Science Silver Medal (for best first class degree
Undergraduate tutors: D. McNally, W.B. Somerville, C.W. Allen
Dissertation 1971: Absorption Lines in QSO Spectra

9. Postgraduate studies : at home or abroad; higher degrees; research fields to date; details of any major engineering works; travels, scientific expeditions, etc.

Oxford University D.Phil. Astrophysics 1971–1974
awarded November 1974

Corpus Christi 1971–72
Wolfson College 1972–74

Prizes: Johnson Memorial Essay 1972 'Nucleosynthesis and the Iron Peak'
Graduate Award (Wolfson College) 1972

Thesis : Supervisor Professor D E Blackwell
'Stellar Abundances and Nucleosynthesis'

Two observational campaigns to Wise Observatory, Israel following construction of a spectrum scanner at Oxford. Laboratory astrophysics using an absorption line furnace at Oxford's Department of Astrophysics. Modelling & analyses.

Attended IAU European Assembly 1972 Athens, Greece

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PERSONAL RECORD

SECTION A (cont.)

10. Appointments held : with dates; research, teaching, administrative; publications and activities, other than scientific.

1974 – 77 Senior Demonstrator in Physics, Durham University
- laboratory demonstration, postgraduate lectures, tutorials & junior administration

1977 – 81 Postdoctoral Assistant, Physics Dept, Durham University
- research on observational cosmology
- entered national SERC committees via Panel for Astronomical Data & Image Processing (which established STARLINK project) 1978

1981 – 83 Lecturer in Astronomy, Physics Dept, Durham U
- Undergraduate lectures in physics, laboratory work, tutorials, postgraduate supervision
- major SERC committee activities

1983 – 85 Principal Research Fellow, Royal Greenwich Observatory
- research, commissioning Isaac Newton Telescope on La Palma, support of visiting observers

1985 (April – July) : secondment to Space Telescope Science Institute, Baltimore MD - research only

1985 – 93 Professor of Astronomy, Durham University
newly-created chair, part-funded by SERC
- undergraduate teaching, research programme, administration & personnel procurement

1989 – 94 SERC Senior Research Fellowship
transferred to Cambridge in 1993

1993 – Plumian Professor of Astronomy & Experimental Philosophy, University of Cambridge

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1994 – Professorial Fellow, Magdalene College

1994 – Director, Institute of Astronomy
University of Cambridge
- postgraduate & undergraduate teaching
- research programme management
- staff welfare, funding, administration
- University & national committees

Committees : a selection of most significant in my career (SERC unless otherwise stated)

1978–79 Panel Astronomical Image & Data Processing (initiating STARLINK project)

1980–83 Panel for Allocation of Telescope Times

1981–84 STARLINK Special Interest Group 2-D Image Processing (Chairman)

1984–85 NASA Space Telescope Science Working Group on Deep Surveys

1984–87 La Palma Users Committee

1985–88 Studentships & Fellowships Panel

1985–89 Astronomy & Planetary Sciences Grants Committee

1988–90 Space Telescope European Coordinating Facility Users Committee (Chairman)

1986–1990 Large Telescope Panel (Chairman)
- initiating UK participation in Gemini Project

1990–1994 Gemini International Science Committee

1990–1995 Anglo-Australian Telescope Board

1994 Astron Foreign Evaluation Committee (decade review of Dutch Astronomy)

SECTION A (cont.)

11. Public honours; honorary degrees; fellowships; medals, lectures and awards; membership of, and office in, British and overseas scientific societies, academies etc; membership of official advisory bodies, scientific editorships.

Fellowships :

Royal Astronomical Society 1974 –
SERC Senior Research Fellowship 1989 – 94
American Astronomical Society 1985 –
Astronomical Soc. Pacific 1985 –
Senior Visiting Fellow, Space Telescope Science Institute 1985
Anglo-Australian Obs 1991
Princeton University 1992
Caltech 1991

Lectures : Bishop Lecture (Princeton) 1992
Halley (Oxford) 1993

For committees & official bodies see §10

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PERSONAL RECORD

SECTION B. Autobiographical notes. [CONFIDENTIAL]

(The following headings are suggested, but you may prefer to arrange your notes in some other form.)

1. Early years and interests; any special features of the locality or facilities available to you.

I was brought up in a Welsh-speaking household. My father was a sailor and rarely home until I was a teenager. My mother was strict and religious. I had less freedom than English children around me. I was evidently eager to break out of the household world because, exceptionally, I was allowed to attend school with my sister at age 2, although formally I was entered at age 4.

By the age of 6 I was certain I wanted to be an astronomer. Two specific books triggered my fascination. Colwyn Bay had an excellent public library & I recall a book by Patrick Moore 'Into Space' & a book on space travel. I could readily grasp the concept of the exosphere & soon devoured all astronomy books in the children's library, progressing to the adult section which had an excellent collection. My early interests were very general & I cannot remember specific books of importance. I seemed more concerned with gathering facts & establishing the geography of outer space than the physical laws. I suspect this is because, although fascinated, no father was present to guide me in things mechanical or physical. Only when I entered secondary school did I catch up on physics & realise the role astronomy had to play. The early years were thus characterised by fascination for outer space.

At school I was well-known as 'the astronomer'. I distinctly remember the launching of Yuri Gagarin into space but cannot recollect the launch of Sputnik at the time (age 7). Wales was a sleepy place to grow up in. There was no clear family recognition of my fascination with astronomy until I was 10 or so. Thus I largely developed my interest in the subject single-handed

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2. School life : teachers of influence; introduction to science; encouragement to a career in science; extra curricula activities.

No science was done in primary school although a final year teacher, Miss Cole, spotted my enthusiasm. I suspect more could have been done. After passing the 11+ I went to Abergele Grammar, following my sister. The school had a better academic reputation, was smaller & had a greater Welsh-speaking proportion, than Colwyn Bay Grammar. However the journey was tedious.

In the first year I became interested in physics & sciences generally but the teacher was poor & I was frustrated. He lacked rigour and showed no interest. In the second year I entered Mr EOP Williams' class in physics. He was extraordinarily strict and made a big impression. I was desperate to please him. Physics fascinated me because 'mathematics was needed to make progress. Both were my best subjects. Biology was too descriptive; chemistry too disorganised. I enjoyed practical work immensely.

My father returned home after a nervous breakdown at sea. His presence in the house changed the atmosphere. He took interest in my physics homework. My mother relaxed her strictness as I became a teenager. I felt free to explore.

I performed well in all the examinations but did not score as high in the GCE 'O' levels as I predicted. It did not disappoint me overly. I seemed to be very relaxed, and not too ambitious In the sixth form I was very happy indeed — studying Physics Pure & Applied Maths. I did the minimum load but scored well. After 1 year I already had a 'A' level in combined maths at grade A. I did very well in the final exams but was astonished to discover a grade 'C' in Pure Maths. The Maths teacher, D. Tristram, complained to the Welsh Joint Education Committee but no regrading was done. In Physics & Maths I was the highest scorer in my year & I had no doubts I had made the right choice of subjects.

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SECTION B. (cont.)

3. Development of interest in your subject and in your particular research field; senior scientists who may have influenced your ideas; key discoveries that impressed you.

My interest in astronomy had been pushed aside somewhat as I learnt physics, calculus & maths. It was dormant to some extent. I built a telescope (4") & continued to read but there were many teenage distractions & I was eager to develop intellectually & socially.

I chose astronomy at University without question although nobody at school guided me. The Headmaster, H. Thomas, actually tried to dissuade me. Curiously my two teachers, E Williams & D Tristram, played a passive role.

Only UCL & St Andrews were worthy of consideration & London clearly had greater prestige. I had read of C W Allen's work & attended for interview. To actually meet a Professor of Astronomy when I had barely travelled outside Wales was a formidable event. I was delighted to go to UCL but soon got disenchanted with undergraduate education. The teaching was poor. One lecturer, D McNally, deliberately kept his distance and made spherical astronomy difficult. I was surrounded by physicists of apparently greater intellect & lost confidence. Slowly I recovered & scored well. With confidence I embarked on practical projects of greater scope. In 1971 I studied a solar eclipse with the large telescope at Mill Hill, photographed M31. I realised I was hooked as an observational astronomer

I worked very hard to get a First Class honours degree & invested a lot of effort in considering how to do a Ph.D. McNally was the only academic frank enough to advise me.

I was interviewed at Sussex (Prof R. Taylor), Manchester (Prof Z Kopal) & Oxford (Blackwell). I was rejected by Cambridge & Edinburgh. Sussex might have been a good choice but a pre-M.Sc. course was obligatory. Manchester prevaricated

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and seemed too theoretical. Oxford had prestige & Blackwell seemed charming. It never occurred to me to go abroad.

Oxford was a dreadful mistake. The department was dull & weak. The University was tediously snooty. Peach was working in cosmology but unwilling to take me on. Blackwell took me in his stellar group. I had done an undergraduate dissertation on QSO absorption lines which had fired my extragalactic interest & desire to do research. Without supervision I had invested a lot of effort reading the literature. I was eager to work in extragalactic astronomy & McNally had claimed Oxford would be an exciting place as Sciama, the theoretical cosmologist, would be linking with Blackwell, an experienced observer. This merger never occurred and I was too meek to object when Blackwell insisted I worked on stellar atmospheres.

The only aspects of my D. Phil that I enjoyed were the portions concerned with nucleosynthesis, computing and observing. Blackwell was a very traditional supervisor who did not establish close relationships with his students. I rarely met anyone else & was fortunate to go to even one conference in my 3 years. I never presented my thesis work anywhere whilst at Oxford. The best thing I can say about my time at Oxford is that I learnt radiative transfer and saw a 1-metre telescope.

Blackwell was President of the RAS at the time & I accompanied him to RAS meetings. Both the Oxford experience and the RAS (a stuffy society not interested in young astronomers) convinced me to seek a career outside of astronomy. I also suffered a depressing incident at the Wise Observatory (Israel) in 1972 where I felt so disenchanted that I abandoned the observing run causing Blackwell much anguish. The relationship was not an easy one. When I completed my thesis, he refused to read it arguing he might exert undue influence on the outcome. When I compare my D. Phil experience with those of students today I realise how times have changed.

3. contd.

The confidence I gained in London was lost at Oxford and I went to the Careers Advisory Office for guidance. Surprisingly I found I was employable! I discovered I had good interview talents and could present myself in letters effectively. I secured several tempting jobs including an advertising executive (at Boase Massim Pollit), working on journals (at MacMillans) and at research (in IBM). I also considered teacher training & secured a position in London University.

Blackwell, to his credit, showed me a letter from Wolfendale (at Durham) requesting a young observer to work on a new imaging polarimeter developed by Scarrott. I was invited to visit & liked Durham. The salary was good & housing was cheap in Durham so I applied. I interviewed well & began in October 1974 as a Senior Demonstrator in Physics.

Scarrott had built an imaging polarimeter that fed an electronographic camera. His goal was to make maps of nebulae and infer various properties. Wolfendale was head of department and a strong figure. He intervened in the relationship between Scarrott & myself and asked me to consider the question of cosmic magnetic fields. With two masters, confusion reigned and the young postdoc could not always deliver. Scarrott ejected me from his research team in 3 months. Wolfendale said if I did not get a move on I would have to resign!

These was still unhappy times as I struggled to explain I was an astronomer in a physics department. Fortunately Scarrott had also ejected a student, David Axon. We compiled a catalogue of the polarisation of over 5000 stars of known distance. Using the Stokes parameters as vectors we published 2 articles on the magnetic field of the galaxy. I gave a departmental colloquium which was well received.

SECTION B. (cont.)

4. Research collaborators and interactions with other scientists; key scientific meetings; major career decisions; how your research field developed.

1975 marks the beginning of my extragalactic research career. Wolfendale was ingressed by the work with Axon (who left with his Ph.D) but pressed for understanding the question of an intergalactic field and its large scale form. This was rather an esoteric question with no observations of use in the literature. I stumbled on the question of the orientation of galaxies via papers of Piddington. Peebles & Hawley found no alignment of galaxies as might be expected. Brown in the 1950's & 60's had catalogued several thousand galaxies. I visited the old amateur before he died & photocopied his notebooks Computer analyses yielded a null result.

1975 was an interesting time in galaxy formation with a Workshop at Cambridge which, for the first time, brought me in contact with US leaders & Cambridge staff. Wolfendale pushed me into contact with Fong, an ex-particle physicist at Durham who had ingressed Dirac as a student in the US, but done little since. I visited the COSMOS machine under development at Edinburgh & considered using it to map the galaxy distribution. At Cambridge, Peebles' papers on the covariance function were prominent although Peebles did not attend. Jones, Fall, Rees, Gott were prominent players. I realised the prime aim of the COSMOS data might be to examine evolution in the correlations. This was not motivated by any theoretical discussion in Cambridge but simply by my realisation we had deeper Schmidt data than anyone else. A new student Steve Phillipps assisted Fong & I in this work. We began by establishing, rigorously, what the Schmidt plates were likely to be detecting. George Abell was visiting Edinburgh that year (as was de Vaucouleurs later). I remember interesting debates with both on the selection function.

Wolfendale, pushing as ever, organised a workshop on COSMOS in Edinburgh in 1977. I gave a talk which was well received and discussed in New Scientist. We seemed to detect evolution in both galaxy properties and in the degree of angular clustering

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at faint limits. Several articles were written with Phillipps and Shanks, a new student. Fall took the trouble to visit Durham & give us encouragement. More plates were scanned with COSMOS but the machine had idiosyncrasies and the plates were not always good. Cannon at Edinburgh was keen to encourage me to visit the Schmidt in Australia & see how plates were taken. This was a generous introduction to the AAT and the UK Schmidt. Tritton looked after me & I met Morton, Peterson & Penston for the first time. Morton impressed me.

Wolfendale paid for me to visit Fall at Harvard & ~~toot~~, giving talks about our correlation results. This was my first true international opportunity & I met Davis, Geller, Oemler, Tinsley Peebles & Gott. Tinsley was super & genuinely interested in the young Brit. I sincerely regret we never collaborated before she died of cancer. Peebles was welcoming. I certainly returned to the UK in high spirits.

My teaching position ended in 1977 & I applied for numerous positions in the US, securing none. It was a fact that Durham was little known, I had no sound referees except Fall & Wolfendale. Even Wolfendale, I later learnt, had no strong US reputation. Fong kept me on a postdoc & I decided to corner new areas. Until 1978 Fong, as the tenured member of the group, believed we had one 'mission' - namely to measure correlation evolution. He was an eccentric & ridiculed by many. A young Ph.D student, Efstathiou, broadened my horizons in discussions & convinced me to apply for AAT time to study the dynamics of ellipticals. I sought Carter as a possible collaborator In 1978 I first observed with Carter on the AAT; it was an inspiring event. Peterson visited Cambridge that year & drove up to Durham. He stayed with us & showed me a superb AAT plate which went much deeper than the Schmidt material. Fong & Shanks were too preoccupied to notice its significance so I collaborated with Peterson producing the first faint counts to $B=24$. My modelling with Phillipps held me in good stead as I was able to demonstrate the counts exceeded no evolution predictions. Rees asked me to speak at a Royal Soc. meeting attended by Gunn & Tinsley. I also spoke at the Texas meeting in Munich attended by Peebles. News of my results spread to Kron in Berkeley.

It was clear to understand this evolution a better local galaxy survey was required. Efstathiou realised such a survey could simultaneously determine the cosmic mass density. I led the proposal with Peterson, Shanks & Fong which led to the Durham Anglo-Australian Redshift Survey. This showed no dynamical evidence for $\Omega = 1$ and, with other surveys, led to biased galaxy concepts. The luminosity function was based on 350 galaxies.

In 1979 I attended an IAU Symposium at UCLA organised by Abell & Peebles where I met Kron, Tyson & Koo. It was also my first extended visit to California & made a big impression. Kron & I were keen to use Kibblewhite's APM machine in Cambridge & this led to a long association with the Institute culminating in an offer to go to IoA as a postdoc in 1980–81. By this time I felt I had outgrown Fong's group. Efstathiou was in Berkeley & the world was a much bigger place. Wolfendale secured a lectureship & tenure. By this time I had two small children so the timing was good.

My rejection of a position at Cambridge caused some amusement. Rees claimed I had made the right decision if I wanted to stay in Durham a long time. Starting in 1981 I set to building a strong extragalactic group in Durham. Fosbury at the RGO asked if we were interested in building a faint object spectrograph. Mike Disney was keen & so I began acting as a broker & consultant for a new instrumentation group. Disney recruited many students by his charms; one (Ian Parry) became exceptional. I also recruited two posthocs, Couch & Sharples, who kindled my interest in distant clusters. Mackay in Cambridge had a CCD camera we used for the first time on the AAT. It was an exciting development & Couch & I worked hard to publish an early series on the colour & redshifts of distant galaxies in clusters.

I was convinced spectroscopy was the key to understanding galaxy evolution. I attended an RAS meeting where Angel described his fibre-fed spectrograph MEDUSA. Disney had urged me to take it seriously & I pushed Morton to do this on the AAT. Morton was a great Director & put his best young engineers, Peter Gray, on the problem. Within 2 years I was measuring B=21 fibre spectra.

Teaching was now encroaching on my ever increasing research time & I sought leave of absence to go to RGO to commission the 2.5m INT on La Palma. I sensed a chair was coming up at Durham & believed it increased my chances to have one foot out of the door.

I misjudged the finances & domestic difficulties of working at RGO. My family moved to Lewes but we aborted & returned to Durham soon after & I commuted. The chair was advertised at Durham & I was shortlisted with Caswell. Pagel had been unfairly eliminated by Wolfendale on grounds of age. Rees & Boksenberg, the assessors, objected & reinstated him. Pagel was interviewed & offered the job.

As Pagel was at RGO I knew him well. A man of stature I was for his junior. I felt pleased I got on with him & did try to assist his plans to move. He became sick with worry about the move, however, & eventually rejected the offer. I never had a frank debate with him at the time about it.

My interests had moved also to the infrared. Longair & Lilly were using UKIRT to study radio galaxies at high z and I was studying normal galaxies at faint limits with David Allen & locally with Shaples. I became as regular a visitor to UKIRT as the AAT.

My time at RGO was not productive. I spent a 3 month sojourn at STScI with Cowie (who hardly spoke to me). The Durham chair was re-advertised. I was re interviewed (with counter offers from Mt Stromlo & University of Hawaii pending) alongside Penston & local rivals. Whether Penston withdrew I'll never know but in 1985 I was made a Professor of Astronomy at Durham.

Peter Gray's fibre coupler FOCAP was now working & I was in full flight measuring faint galaxy redshifts. The only competitors were Koo & Kron who never seemed to publish their data. I put a new student, Broadhurst, on the project after most of the data had been taken. He did a superb job & the discovery of so many emission line galaxies at intermediate redshift was a profound result to me. I spoke excitedly at the IAU Symposium on Observational Cosmology in Beijing in 1986

I believed the faint object spectrographs we were building for La Palma were better for distant clusters and the fibre techniques for field galaxies. Parry was completing his thesis in the instrumentation group and came up with the 'Autofib' concept for fibre positioning. I strove hard to get a contract from Morton for a version for the AAT. Shaples acted as a project go-between as he was a staff astronomer at the AAO

My role in initiating Autofib gained me a valuable reputation as an instrumentalist. I was already on a committee which prepared a plan for future observational facilities & pushed for both a wide field multi-fibre capability and a large aperture telescope. SERC asked me to chair a Large Telescopes Panel with Jim Hough and Mike Edmunds. This was a major undertaking which has occupied me for many years and led to both the Gemini and 2dF projects being funded and constructed. The LTP activities involved me in astropolitics for the first time. I gave numerous presentations and had to tour the UK convincing sceptics. Personally I believe the AAT's 2dF was as important a conclusion to emerge from the LTP as the 8-metre telescope which led to Gemini. At about this time (1986) I attended a conference in Tucson where Jim Gunn listened to my ideas and this led him to embark on the Sloan Digital Sky Survey.

In 1988 I began corresponding with astronomers in Copenhagen who were interested in detecting distant supernovae. I visited and discussed this project with them and got very excited. With Couch, we proposed follow up spectroscopy & light curves, the aim being to demonstrate high redshift supernovae could be found. Several were, in fact, found including one at $z = 0.31$ – the most distant at that time by a wide margin. The project was extremely inspiring and convinced me that the deceleration parameter was within reach if 20–30 such Supernovae could be found. After some years the Danes gave up exhausted and I joined a team based at Berkeley doing similar work. This work continues to higher redshift.

As a new professor in 1985 I secured funds for my first postdoc and appointed Matthew Colless. At the time Keith Taylor and I were constructing a wide field multislit instrument, LDSS. The seed for this new device arose whilst I was at RGO and is based on the remarkable talents of Taylor who could engineer the optical designer, Charles Wynne.

Wynne managed an optical breakthrough – a wide field camera of high quality and throughput. A spectrograph with a 12 arcmin field was some accomplishment. The device was built at Durham & RGO and used at the AAT. Taylor moved to the AAT to exploit it. Colless arrived as the first data appeared. We managed to push the redshift survey a magnitude deeper after endless numbers of cloudy nights. We showed the excess numbers of faint galaxies must arise from some number evolution.

Couch & I had also been working on a catalogue of distant clusters. This was hard work. We selected them from high contrast AAT prime focus plates which we had scanned by eye. Spectroscopy was done on various telescopes.

1988 or so saw the commissioning of the 4.2m William Herschel Telescope. Durham was building a faint object spectrograph (FOS) and a further wide field spectrograph, LDSS-2. I had hired Allington-Smith to do this & both instruments were carefully made. The early observations with the WHT were disappointing. The FOS was not well-suited to my studies but was effective for the QDOT IRAS redshift survey of which I was a minor participant. One night, Rowan-Robinson & I measured 120 redshifts on the WHT. My main conclusion was that the telescope's large aperture was being wasted.

Durham continued to grow & I was enjoying my time as a Professor. Frenk was hired as a Lecturer & I retrieved Lucey, Shanks, Allington-Smith, Parry & others to my staff. When the Large Telescope Panel advertised for a Project Scientist to lead the proposal further for a UK 8-metre, Roger Davies was appointed and, unusually, offered a free choice of institutions to set up the UK LT Project Office. Efstathiou had just been appointed to a chair at Oxford and Davies declined my invitation to come to Durham. This was a bitter setback to what had been going very well

at Durham.

As Broadhurst completed his thesis, he spotted a truly remarkable result – namely an astonishing regularity in our redshift data. I carefully wrote this up for Nature with Koo & Szalay and there was intense media attention & much derision from my colleagues. Curiously, those with imagination (Shanks, Frenk..) were hostile and those with rigour (Lucey, Colless) were supportive. Occasionally I regret writing this paper but believe the text is sufficiently carefully written to cover all eventualities. The data had to be shown but I was happy to leave others to judge its merit.

At about this time (1989) I became interested in gravitational lensing. I attended a conference in Toulouse. Mellier had visited Durham to learn about redshift surveys. These visits led to a long collaboration with Fort & Mellier. My main interest was in using lensing to constrain redshifts of galaxies beyond conventional spectroscopy. At this meeting I first met the lensing community and, with the exception of Blandford & the Toulouse group, was not impressed.

Back at Durham I put a new student, Smail, onto lensing. He made enormous progress. We catalogued arcs in those clusters for which supernovae had been sought, measured the redshift of the giant arc in Abell 963 with the Hersche (my first 'big' result with that telescope) and Smail imaged 3 clusters to unprecedented limits for his thesis work on weak lensing and dark matter mapping.

These were fantastic times at Durham. I had 3 superb students: Bower, who matched my observational interests with Frenk's theoretical ones; Aragon – who studied the evolution of cluster galaxies from infrared-selected samples, and Smail who worked on lensing. So much was happening and HST was about to be launched that I applied again for a SERC Senior Fellowship. I had

tried to get one before I became a professor & apparently got past one hurdle. In 1989 I succeeded and was able to drop teaching (though not administration). Prior to this, when the Large Telescope had seemed financially impossible, I had seriously considered emigrating to the USA. I received offers from JHU and Steward Observatory and I actually applied for a posita at Carnegie, Pasadena but was rejected. Simon White was especially keen I go to Steward. as a close friend of Carlos Frenk this made life interesting at Durham. In the end, I decided I was happy at Durham & never regretted turning down both Steward and Hopkins' offers.

When HST was launched my father died. I had two main programmes - a medium deep parallel survey - a key project led by Richard Griffiths (JHU) involving huge teams of people, and an imaging survey of Couch & Sharples' clusters. The image of AC114, one of these clusters, was spectacular and showed lensing features of great significance. The Medium Deep Survey allowed me to hire a new postdoc, Karl Glazebrook, who helped commission LDSS-2 a new survey spectrograph for the Herschel, built by Allington-Smith & Breare. With this device, we pushed the redshift surveys 1.5 magnitudes deeper than at the AAT. Unfortunately, to this point we had no competitors, but now Lilly & French collaborators had embarked on a more ambitious version. The better weather on Mauna Kea gave them superior results.

During the period of my Senior Fellowship (1989-94) I took on board numerous research projects but they fell into 3 broad classes - the evolution of field galaxies (mostly LDSS-2 surveys and Medium Deep Survey images), the evolution of cluster galaxies (HST imaging and detailed spectral modelling) and gravitational lensing.

Politically I remained in charge of the UK's Large Telescope aspirations and successfully convinced the AAT Board to fund the wide field "2dF" 2 degree field survey spectrometer. The LTP aspect got quite stressful as two factions developed supporting both a US partnership and one based on Spanish participation on La Palma. I was firmly convinced the Mauna Kea site was superior and that Spain would be weak partners intellectually. I was bitterly opposed by many senior astronomers of stature including Rees, Graham Smith & Wolfendale. Eventually Wolfendale was convinced I was right and his change swung the decision I believe, although Corbett & Roger Davies were key figures too in the final SERC vote.

Wolfendale was nearing retirement but remained a formidable figure. I did not respect his scientific intellect much and often became frustrated at the lack of freedom he gave me at Durham. Nonetheless, there was no doubt he was to be thanked for engineering many of my achievements. In 1992 Rees moved to a Royal Society Chair. The Plumian Chair became available and I was interested but would never apply unless invited. In May 1992 at Efstathiou's request, Lynden-Bell urged me to apply. Blandford was offered the position as, possibly, was Sargent. I was visiting Princeton at the time and befriended Ostriker who was very kindly. In late October 1992 I was offered the position. My family debated the situation for 3 months and in February I was about to decline because I could not see reconstruction of my empire in Cambridge given the difficult way the University is structured. It was my wife who convinced me we needed a change & I suspect the move was not scientifically motivated at all. I realised it would be a great struggle and that it would do me good. In April 1993 I found myself entering the Institute of Astronomy at Cambridge alone, leaving over 40 people at Durham who had worked closely with me on research and instrument development.

8 April 1995

SECTION B. (cont.)

5. Pupils and their achievements.

Graduate students supervised (wholly* or in part)

David Axon	1976	Reader, University of Manchester
Steven Phillipps*	1979	RS Research Fellow, Bristol
Tom Shanks	1979	Reader, Durham
George Efstathiou	1979	Savilian Professor of Astronomy, FRS, Oxford
Ian Parry	1986	ADR, Cambridge Univ.
Ian Inglis*	1985	Logica Ltd
Iain MacLaren*	1987	Lecturer, Paisley Univ.
Bahram Mobasher*	1987	PDRA, Imperial College
Tom Broadhurst*	1989	NASA Research Fellow, Johns Hopkins Univ.
Richard Bower*	1990	Lecturer, Durham
Alfonso Aragon*	1991	RS Research Fellow, Cambridge
Ian Smail*	1993	Advanced Fellow, Durham
Francisco Castander*		–
Rafael Guzman	1993	NATO Fellow, UC Santa Cruz
Jeremy Heyl*		–
Amy Barger*		} current students
Tim Ebbels*		

6. Views on education and science policy.

(continue on spare sheets provided)

SECTION B. (cont.)

7. General interests:

Travelling - generally alone

Skiing

Photography

(continue over and on spare sheets provided)