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An Accidental Career

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EARLY DAYS

I was born in September 1925 in Chipping Norton Oxfordshire, a small market town in the Cotswolds roughly midway between Oxford and Stratford-on-Avon. The families of both of my parents had lived in Chipping Norton for many generations. My father, Leslie Burbidge, who had served in the First World War in Serbia and Greece, was a partner with his two brothers, Fred and Percy, in a small building firm, Burbidge and Sons, which covered a large rural area, building, renovating, and repairing many kinds of Cotswold buildings using Cotswold stone, etc. I was an only child, but my father had five brothers, one of whom had died during the war, and two sisters. My mother, Evelyn Beechey, who was a milliner, had three sisters and two brothers. Her family was distantly related to the well-known portrait painter of the eighteenth century, Sir William Beechey, R.A., who was born in Burford, about 10 miles from Chipping Norton, in 1759.

I was educated in Chipping Norton, first at the primary school and then at Chipping Norton Grammar School, a small school with about 250 pupils, who came from the town, and many surrounding villages. The Burbidges were very well known in the town, and my parents and many of my uncles and aunts and cousins were prominent in local affairs, particularly in the local Baptist Chapel and in managing the local hospital, in horticulture, and in musical affairs. On my father's side the family was very musical; my father was a good cellist and pianist, and my Uncle Fred and two of his daughters were the organists at the Chapel and also ran the choir.

Fred Burbidge in particular was extremely versatile in many areas. He ran a small market garden in which he grew tomatoes and other vegetables. He and his daughter Hilda had a thriving apiary, and there was a small shop attached to the house where they sold their produce only about half a mile from where we lived, adjacent to their house and the builder's yard.

My father also was a very good sportsman. He had played soccer as a young man and when I was growing up he was the manager and the secretary of the local football team and later a long-serving member and sometime secretary of the Oxfordshire Football Association.

But his greatest ability was in lawn tennis. While he had never had a lesson, he was a truly formidable player with a devastating left-handed service. He won all the local club competitions many times. He taught me to play, dealt with and disposed of my bad temper, and turned me into a reasonable player, although I could not get on even terms with him until he was nearly 50. I was always too erratic. He used to take me to Wimbledon for a day every year and this led me to a lifetime interest in tennis. My father also imbued in me a very strong sense of fair play, something that in the cut and thrust of academic politics and science has turned out to be a mixed blessing.

This is the atmosphere I grew up in. The school was a good one although it was small, and of course I was growing up in the middle of the Second World War, so there was an atmosphere of tension and concern among everyone, though there was no physical damage to Chipping Norton or its environs. Had it been any other time I would probably have left school at 17 and found some kind of a job in the same area. The subjects that I excelled at in school were history and mathematics.

I was lucky to have an extremely good mathematics master, Leonard Miles. For me physics was interesting, but not particularly important. When I was not at school I was reading voraciously without any guidance, all the way from many of the classics, to thrillers, to the plays of George Bernard Shaw, and many other contemporary writers. I discovered accidentally T.E. Lawrence, and his *Seven Pillars of Wisdom* was to me a marvelous and rather mysterious book.

I was also a countryman who spent a great deal of time cycling and walking with my dog in the countryside. I played tennis, and cricket for the school and in some village matches. I was a left-handed batsman.

In my teens I also spent quite a lot of my time helping in the building office and went out with my uncles to the many jobs in the local villages. My father did the books and looked after all of the financial affairs of the business, and of the cottage properties that his cousin owned.

In this period I first learned something about business, about the building industry, about dealing with the bank and awkward clients, and about paying and looking after the employees. This was my only experience of dealing with people rather than with scientific and academic problems, and I didn't have to face these problems again until I took over as Director of the Kitt Peak National Observatory more than thirty-five years later. I concluded then that dealing with people, and in particular managing them, is much more difficult than doing science!

In 1942 the war was at its height and I realized that soon I would be called up to join the armed forces. Since I excelled in mathematics and was competent in physics, I was told that I might be able to get a scholarship (a Bursary as it was called) to a university, which would enable me to study for a Pass Degree in Physics and take a short service commission afterward, delaying my entry into the service for two years. I applied for this and was accepted by Bristol University.

For the family in Chipping Norton this was a great thing, because before me no one in the family had ever had any education beyond grammar school. For me it was a completely new adventure because I had never spent any time away from home except for summer holidays mostly in Dorset and occasionally in Cornwall.

While I knew nothing about it at the time, by coming to Bristol I had come to a very good Physics department. It was headed by Professor A.M. Tyndall. On the staff were two physicists who later won Nobel prizes, Professor N.F. Mott, who came back from war work while I was there, and Professor C.F. Powell, together with quite a few other well-known physicists: Herbert Frölich, W. Heitler, F.R.N. Nabarro, and others. I took lecture courses from most of them, and as an undergraduate I also got to know G.P.S. (Beppo) Occhialini, originally from Milano, and his associate (later his wife), C. Dilworth, who were both working in Powell's Cosmic Ray group. Occhialini was a fascinating man and a great physicist. He never lectured to us undergraduates, but we learned a great deal of physics from him. I found out later, he was one of the very unlucky individuals who worked with two individuals on the problems that led them to get Nobel prizes—P.M.S. Blackett and C.F. Powell—but was never included by the Nobel committee.

After two years, the war was reaching its end, and a very small number of us in the class were allowed to stay at Bristol for another year to complete a special honors degree; I got a very good honors degree in 1946. At this point I thought that I would take a short service commission in the Royal Navy and become a radar or meteorological officer. However, the Ministry of Supply decreed otherwise and I was appointed to work in a ballistics laboratory that was the original Road Research Laboratory in Hounslow in West London.

I was there for about 18 months working on two programs that were both part of the long-term work of the lab. The first was associated with the attempts that were started during the war to provide the Royal Air Force (RAF) with bombs that could penetrate the caves where the U-boats docked and were refueled under the cliffs near Brest. These were the U-boats that were originally playing havoc with the Allied shipping in the Atlantic and the Bay of Biscay. The method of attack being planned was to design bombs that would penetrate great distances through rock and then explode. Thus we were testing various bomb configurations. They were theoretically calculated and then tested. To make the tests, scaled down (if necessary) bomb cases were made (at Woolwich Arsenal) and then loaded into large bore naval guns that were then fired (by us) horizontally (without explosives of course) through thick sheets of steel mounted as sets of horizontal screens. To do this we went down to the Naval Firing Range at Shoeburyness. A series of timing and penetration devices were used and we brought back and analyzed the results.

The second program was concerned with demolition. A scaled-down model of a ballistic laboratory, based on an existing Canadian lab, was built at the Road Research Laboratory and then we systematically blew it up, section by section, using the latest plastic explosives (which then, as now, is very similar to plasticene). On occasion I accidentally left in my pocket some of this when I went down to Chipping Norton for the weekend. When my mother found it, I had to tell her that it was modeling clay. Working on this program was a slightly scary business because the heads of the sections (of which I was one) had to go and investigate, if the plunger went down and nothing happened.

During this period of working in the lab two things happened to me. (*a*) I learned more than I could ever have imagined about guns and explosives. The lab had a long history of experiments throughout the war so that all types of guns were available there: Webley automatics, machine pistols, submachine guns, large machine guns, anti-aircraft guns (Bofors, in particular), and everything up to light artillery. There was a range on which you could try things out, and some of us did. (*b*) I concluded that physics is a completely fascinating field, and I became determined to become a graduate student, get a Ph.D., and do research.

LONDON

After I left the Road Research Lab, I visited a number of physics departments in southern England to see if I could get a scientific bursary to do graduate work. Apparently my credentials as an undergraduate were good enough so that I received several invitations. I finally decided to work with Professor H.S.W. Massey who was at the time head of the Department of Mathematics at University College, London (UCL). Massey was an Australian who had worked in the Cavendish Laboratory, Cambridge, in the 1930s. He was a theoretical physicist and was very well known at that time for his work on atomic collisions—there was a famous book by Mott and Massey that we all studied in great detail.

I started work at UCL in 1947. After a few months Massey suggested that I work on the capture of μ mesons by atoms where the major problem is to calculate the rates of Auger transitions as the μ mesons cascade through the atomic shells. At that time, π mesons and μ mesons and the π-μ decay process were being sorted out through cosmic ray work. My thesis work involved a large number of calculations of hypergeometric functions and this led to a network of results that could be compared with experiments. This led to my first publication in *Physical Review* jointly with A.H. de Borde, who had corrected some of my mistakes. In doing this work I began to realize how important it was to carry out theoretical research that could be compared with experimental results. In the same period I became extremely interested in the new developments in quantum electrodynamics. There was no one at UCL working in this area, but Massey encouraged me to travel regularly to Cambridge and attend the theoretical seminars in field theory that were being run there under the direction of Professor P. Dirac, Dr. N. Kemmer, and Dr. J. Hamilton. Here I made many new friends who were students working on quantum field theory. Two of them were Abdus Salam, one of the great men of modern physics who was later the only Pakistani to win a Nobel Prize, and John Polkinghorne (now Canon Polkinghorne). Later I saw Abdus Salam quite often in London and Trieste.

The colloquia in Cambridge were much more geared toward a very mathematical approach to field theory than most theorists were used to. For them it was much more important to prove the lemmas, etc., than it was to relate the particle physics to experiment. Experimental data relating to theory were rarely if ever discussed, so topics like the kind of problems I was working on were thought to be quite inappropriate there.

However, I became extremely interested in the problems of renormalization and the methods developed by Feynman, and attempted to make calculations on one of the relativistic correction problems. I failed, but I learned a great deal.

I also attended a variety of lecture courses in London. These included a course on the atomic and molecular physics of the upper atmosphere of Earth, a field in which Massey had made considerable contributions, and which was a specialty of D.R. Bates (later Sir David Bates) who was a lecturer at UCL. Other students of note at that time were Michael Seaton and Robert Boyd. Here I met Margaret Peachey who was older than me, and already had a Ph.D. in astronomy from UCL. She was the assistant director of the University of London Observatory, which was situated out at Mill Hill.

In April 1948 we got married, and lived the first year in the home of Clive Gregory and his wife. He was the director of this small observatory, a good classical astronomer, but a difficult man for most of his associates. Astronomy at UCL had been started in the Department of Mathematics, but there was no chair of astronomy.

The main research program at Mill Hill was a parallax program using the 24-inch Radcliffe Telescope. Margaret had done her thesis work using the stellar spectrograph at the Cassegrain focus of the smaller telescope carrying out a study of a bright B emission-line star γ Cassiopeiae. The parallax program required the observation of a set of program stars systematically over about seven observing cycles (years).

I learned to assist in observing in the period when we lived at Mill Hill and we walked or bicycled to the observatory early in the evening, and again several hours before dawn. I was still working on my thesis and had also been appointed as assistant lecturer in mathematics at UCL. My main task there was to tutor undergraduate mathematics students, many of whom were returning ex-service men who were as old or older than me. I started with no experience at teaching, and I learned fast, but realized that I would never make a good teacher (I was and am too impatient, and at that stage I was often quite inarticulate). In August of 1948 I attended my first astronomical meeting with Margaret—the International Astronomical Union (IAU)—which was held in Zurich. At that meeting we were introduced to many astronomers, and Margaret talked at length to Otto Struve, who at that time was the leading astronomical spectroscopist in the world, director of the Yerkes Observatory, and president of the IAU. He suggested to her that she come to the United States to observe using the larger telescopes in better climates.

The following summer, 1949, we went to France where she had been given observing time at the Observatory at St. Michel in Haute Provence. We lived for several weeks in the director's house and worked very hard with the telescope. We then traveled back to Paris and traced the spectra using a microphotometer at the Institut d'Astrophysique. Here we met some of the leading French astrophysicists including Daniel Chalonge and Evry Schatzmann. At the time, a meeting on Astronomical Turbulence was going on there, and I tried to attend as many of the sessions as I could manage. I learned a great deal about astronomical turbulence at a time when turbulence was thought to be the answer to many of the observed properties of the interstellar gas and was thought also to have a strong bearing on star formation and galaxy formation. One of the leading theorists present was C.F. von Weizacker together with other German colleagues like Reimar Lüst and A. Schluter, and probably L. Biermann was there as well, and Bengt Stromgren. Among the English astrophysicists Fred Hoyle and Ray Lyttleton also were present. We got to know them, not particularly for scientific reasons, but because we often had lunch together at a small bistro in the Boulevard Arago very close to the Institute. In those days food, particularly meat, was rationed in England, and we Brits were contrasting this with the situation in France. Much of the highly entertaining discussion was initiated by Ray Lyttleton, who was always coming up with outlandish suggestions of ways in which he could get Fred to help smuggle meat back to England.

Fred Hoyle tended to debate every issue with von Weizacker, and I learned a great deal about the basic problems relating to the condensation of interstellar gas, and the problems of spiral structure.

In the following year, 1950, I obtained my Ph.D. in theoretical physics at UCL and remained on the staff until 1951. Massey had organized a meeting at UCL that was concerned among other things with the nature of the recently discovered radio sources. At that time, apart from the sun, the only optically identified radio source was the Crab Nebula. The most popular idea was that the unidentified radio sources were flare stars. This was very much the position of Bernard Lovell, the director of the Jodrell Bank Observatory. As far as the radiation mechanism was concerned Hannes Alfvén and N. Herlofson in Stockholm had proposed that it was nonthermal incoherent synchrotron radiation. On the other hand Martin Ryle and his colleagues in Cambridge believed that it was the result of some form of coherent plasma oscillation. At the meeting at UCL, which I attended as one of the students who went around getting participants to write down their comments, the issue as to the nature of the sources led to a strong, almost violent exchange.

A young man got up (he was a little older than me) and said in a loud voice that since the unidentified sources showed a roughly isotropic distribution, they might either be quite close to us—as flare stars would be—or they might be very far away, at cosmological distances. He guessed that they might be far away. This sounded very reasonable to me, but he was immediately attacked, on the one side by Martin Ryle, who literally told him that he didn't know what he was talking about, and by George McVittie, a well-known theoretical cosmologist. I was amazed at the vehemence of the attack.

Much later after I got to know him well, Fred Hoyle told me that the original speaker was Tommy Gold. Fred had been at the meeting, and he told me how angry Gold was when they drove back to Cambridge that night. Gold and Ryle both worked in the Cavendish and clearly disliked each other. Ryle was already being favored in every way in Cambridge.

Of course, by 1952 Gold had been shown to be right. I was already learning that personalities are as important as observed facts in the way that we do science.

THE UNITED STATES, 1951–1953

Late in 1950 Margaret and I decided to try to obtain research fellowships in the United States for one or two years. She was immediately offered an appointment at the Yerkes Observatory in Williams Bay, Wisconsin, part of the University of Chicago. This would enable her to get observing time on the 82-inch telescope of the McDonald Observatory in West Texas, which was owned by Texas but operated by the University of Chicago. I was awarded the Agassiz Fellowship at the Harvard College Observatory in Cambridge, Massachusetts.

In September 1951 we sailed on the Queen Mary to New York. There had been some delay in the issuance of my U.S. visa because the United States was still in the throes of the McCarthy era, and one of the first papers that I had published in astrophysics in 1950 was entitled "Hydrogen and Helium Line Intensities in Some Be Stars," based on work we did in Haute Provence. I was told at the embassy that the words "hydrogen" and "helium" had aroused suspicion in Washington. However, with the aid of the attaché for the Office of Naval Research of the U.S. Embassy in London, this hurdle was finally overcome.

We were met in New York by F. Bradshaw Wood and his wife from Philadelphia. He was director of the Flower and Cook Observatory at the University of Pennsylvania and we had met them at the IAU in Zurich in 1948. After a week or two I went to Harvard and Margaret went to Williams Bay.

I very much enjoyed that atmosphere, and all of the new people I met at the Harvard College Observatory (HCO). I took a room across from the Garden Street entrance to HCO. At that time Harlow Shapley was the director about to retire, and other prominent people were Fred Whipple, Donald Menzel, Bart Bok and many others. I also met many graduate students who became friends—Dave Heeschen and Ed Lilley who were studying radio astronomy with Bart Bok, Dick Dunn who designed and built solar telescopes and who died recently after a distinguished career at the Sacramento Peak Observatory, and Arnie Wyller from Sweden and his wife, Ingrid. Everyone was extremely hospitable. The science going on at Harvard was a mixed bag. I nearly started work in Fred Whipple's group. However, since I had come into observational astronomy by way of stellar spectroscopy, I had become very interested in stellar atmospheres and then in radiative transfer, and thus I decided that it was in this arena that I would work at Harvard. This meant that the senior person closest to me was Donald Menzel. Thus I talked to him quite often, but the problems were largely my own.

I did complete an investigation entitled, "The Equation of Transfer and the Residential Intensities in Spectrum Lines," which was submitted and published in the *Astrophysical Journal* in 1952. In the second part of the academic year, 1951–52, I spent time at Yerkes Observatory, and in 1952–1953 Margaret came to Harvard and we took an apartment in Allston just outside Cambridge. I also visited McDonald where Margaret had several observing runs using the coude spectrograph of the 82-inch telescope.

In 1952–1953 Harlow Shapley was about to retire, and there was a considerable discussion going on about his successor. Menzel was finally chosen and Bok soon left for Australia. One of the main issues, which continued on for decades, was associated with the fact that Harvard had no first-class optical telescopes in good climates. Shapley had always gone after many (small) telescopes. At one stage they did very well in South Africa, and earlier in Peru, but they had spent large sums of money on the Agassiz station just outside Cambridge where they had built a 69-inch telescope, with an almost unusable spectrograph. I had heard much criticism of this at Yerkes and elsewhere, but I held the Agassiz Fellowship and they wanted Margaret to use this equipment. She had no intention of doing this. One day I went out to the Agassiz station, on some formal occasion, and when asked by some dignitary what I thought the Agassiz station needed I said, quite facetiously, "A case of dynamite." Word got back to Shapley, who was not amused (I think). Even so he was always very generous and kind to me, and, I still have good memories of Harvard.

BACK TO THE UNITED KINGDOM, 1953–1955

Margaret and I had exchange-visitor visas to the United States and this meant that we had to go back to England in 1953. Nowadays many people simply ignore this provision, and stay on almost indefinitely, relying on the universities that find them so indispensable to fix things by an Act of Congress or other methods. Perhaps conditions were easier fifty years ago.

On our way back (from Yerkes) we attended a summer school on astrophysics at the University of Michigan in Ann Arbor organized by Leo Goldberg. This was one of the best summer schools I have ever attended, and the only one at which I was simply a student and not a lecturer or independent researcher.

The lecturers included Walter Baade, George Gamow, Edwin Salpeter, and George Batchelor, and special seminars were given by Allan Sandage and others. I don't remember many of the details, but the discussions involving Gamow and Baade were particularly valuable, and I met for the first time, and argued with, Allan Sandage, who has been one of my best friends ever since.

The major breakthrough in astrophysics at that time was our understanding of stellar evolution. Theoretical work, particularly that by Schwarzchild and Hoyle, and observational work on color magnitude diagrams of galactic and globular clusters, largely dominated by Sandage and his associates, showed for the first time that not only did we understand the evolution of main sequence stars as they evolve up to the giant branch, but we could age date the clusters, getting an age for the Galaxy. Also we began to really understand how chemical composition affects the structure and evolution of stars and both types of cepheids. Only the very rapid stages of later evolution still were difficult to follow.

Back in the United Kingdom I had been offered positions at the University of Manchester and Cambridge. I accepted a research appointment at the Cavendish Laboratory at Cambridge working as a theorist with Martin Ryle's radio astronomy group. Margaret and I rented a small flat over a tailor's shop in Botolph Lane, not more than 200 yards from the Cavendish.

I was given free rein to do theoretical work, and I considered that the main problem was first to understand the basic mechanism that gives rise to nonthermal radio emission in the Galaxy, in supernova remnants, and in the distant radio galaxies. I soon was convinced that the mechanism was the incoherent synchrotron process that I mastered theoretically, and I very soon learned that in the Soviet Union, Ginzburg, Pikelner, Syrovatsky, and Shklovsky had developed these ideas and were applying them to supernova remnants. I had great difficulty in working with Martin Ryle. He was still convinced that the radiation mechanism was coherent plasma oscillations and my arguments with him were very unsatisfactory. I was clearly too independent to fit into the group, who all treated him like a demigod. I suppose that at bottom we didn't like each other. Also, he had his students all working on the counts of unidentified radio sources, which led him to claim in 1955 that he had demonstrated that the steady state theory of Hoyle, Bondi, and Gold had been disproved. I was inherently skeptical about sources that were unidentified. All of this work was kept entirely secret from me, so much so that, when I was told in the spring of 1954 to go with the rest of the Cavendish group to hear him announce this result when he gave the Halley Lecture in Oxford (it was kept secret until then), I was so angry that I refused to go. Later on I began to find out how secretive the group was. But I liked many members of the group, all students or postdocs, particularly John Baldwin, John Shakeshaft, Peter Scheuer, and Tony Hewish, who was more senior.

I was also working on other problems. In 1952–1953 Margaret had obtained at McDonald high dispersion spectra of one of the well-known magnetic variable stars, α^2 Canum Venaticorum, which has an extremely rich spectrum with hundreds of lines due to the rare earth elements and other heavy elements. We brought back the spectra and tracings to Cambridge and derived abundances of many of the heavy elements. This was the first time that the spectrum of such a star had been analyzed in detail. The results were spectacular, and they showed that when averaged through the magnetic cycle, the abundances of many of the rare earth elements were 10^3–10^4 greater than the abundances of these elements in a normal star like the sun. We wrote this work up and it appeared in the *Astrophysical Journal Supplements* in 1954.

We then attempted to understand anomalous abundances by supposing that they were caused by the acceleration of light isotopes and nuclear reactions in the changing magnetic fields of the magnetic variable stars. I gave a colloquium on this topic in Cambridge at one of the evening meetings, either Δ^2V, or the Kapitza Club. Afterward a very well-known experimental nuclear physicist came up to me and introduced himself. This was Willy Fowler who had a Fulbright Professorship in Cambridge on leave from Caltech. He was very interested and we soon became involved with him in nuclear astrophysics. He already knew Fred Hoyle who had visited Caltech. This was the beginning of the collaboration on the problem of stellar nucleosynthesis.

Of course, it was clear that the results on the magnetic stars were not part of this, but Willy, Margaret, and I began by working on the build-up of heavy elements by slow neutron capture—the *s* process—which takes place in red giant stars. By then I was working on the radio sources, on the galactic halo, and on supernova remnants, and all four of us were beginning to see how far we could go in explaining the abundances of all the elements. For the latter study the ingredients were in place: we had a good stellar observer, Margaret; a great nuclear experimentalist, Willy; Fred Hoyle, who had had the original idea; and me, a reasonably well-informed theoretical nuclear physicist who was trying to think ahead in high-energy astrophysics.

By the spring of 1955 we had many of the problems solved but many to work on, and Fowler realized that the best solution was to get us all together at Caltech. Hoyle could come on leave from Cambridge, and Fowler could offer Margaret a soft money position supported by the Atomic Energy Commission in the Kellogg Radiation Laboratory at Caltech. I applied for, and was awarded a Carnegie Fellowship at the Mount Wilson and Palomar Observatories. I was the first theorist ever to be given such a fellowship. Whether this was due to Walter Baade and Allan Sandage, whom I had met in Michigan, or whether it was chosen in Pasadena as a way to get Margaret observing time at Mount Wilson, I shall never know.

Thus, in the summer of 1955 we left Cambridge and traveled via the IAU meetings in Dublin, to Pasadena. As I left I submitted a paper to the *Monthly Notices of the Royal Astronomy Society* that gave a detailed theoretical analysis of the way in which a blast wave from a supernova could give rise to the radio emitting filaments in Cas A and other supernova remnants. This was my first, and last (for more than thirty years), paper submission to the Royal Astronomy Society (RAS). Why was this? I've often said more recently that the refereeing system is broken. In those days all of the papers submitted could be and were looked at by the RAS Council when they met.

I heard nothing about my paper for about four months, when the secretary of the RAS (C.W. Allen) wrote me a note telling me that they had a problem. It appeared that when my paper was seen by Bernard Lovell, the Director at Jodrell Bank, because there was considerable rivalry between the radio groups at Manchester and Cambridge, he had said that his people should look at the paper. So he packed it up and took it back to Jodrell Bank. The story there was that from that time on the RAS didn't know how to get it back. (Unbelievable but true.) I should add that many years later when I got to know Lovell, he and I got along very well.

Nothing was done about this by the RAS, and I got so angry that I simply withdrew the paper and it never appeared anywhere. Of course, others did similar work that was published later. Nowadays other methods are used to get papers blocked!

PASADENA

Stellar Nucleosynthesis

The major project was the work on stellar nucleosynthesis. Through the rest of 1955 and 1956 into early 1957, Margaret, Willy Fowler, Fred Hoyle, and I worked on many aspects of the problem.

In 1956 I noticed a paper in *Physical Research Letters* in which it was announced that in the Bikini nuclear bomb test the transuranic isotope Californium 254 had been detected. It had a half-life for decay of 55 days and I realized that this agreed with the half-life of decay of the light curve of the supernova in the galaxy IC4182, which had been discovered by Walter Baade. This immediately connected supernova explosions to the rapid buildup of heavy nuclei (the *r*-process) in stellar nucleosynthesis. We were all very excited, although we had previously concluded that the *r*-process must be occurring, and several short papers including Baade were published in 1956. Of course it turned out later that the agreement is spurious and the light curve is due to other isotopes. But this result was a great stimulus to our work.

In the midst of all of this our daughter Sarah was born in August 1956. In the next years she had many well-trained babysitters, including Walter Baade and his wife who lived in the next street behind Caltech, and Alexander Pogo, the Mount Wilson Librarian, a marvelous man.

By the first months of 1957 we had put together a tremendous amount of observations and calculations. Hoyle and Fowler were invited to attend a conference in the spring at the Vatican on stellar populations. Margaret and I began to write a draft of the paper and when they came back we completed it. S. Chandrasekar (Chandra) hesitated to publish B^2FH in the *Astrophysical Journal*. (It was never clear to me why Chandra was reluctant. Possibly he thought it was partly a review, which it was not. Also, it was very long.) But it was taken sight unseen and published in the *Reviews of Modern Physics* late in 1957. I can't imagine how long it would have taken to publish it complete with all of its flaws had it been submitted thirty or forty years later.

Radio Astronomy and High-Energy Astrophysics

In Pasadena I was continuing my studies of physics of radio sources. By 1955 a number of external galaxies had been identified as powerful radio sources. It had been shown by John Bolton and by Hanbury-Brown and das Gupta that the extragalactic radio sources tended to be double, with one lobe each side of the galaxy and very large, with the optical galaxy that must be the energy source roughly symmetrically placed in the middle.

Also the Russians had suggested that the optical continuum radiation from the Crab nebula (the unique Galactic Supernova remnant) is incoherent synchrotron radiation and this had been established conclusively by Walter Baade who showed, using the 200-inch Palomar telescope, that the radiation was highly polarized. A similar prediction was made for the remarkable optical jet in the very luminous elliptical galaxy in the Virgo cluster Messier 87, and this was also confirmed by Baade using the same technique.

Knowing this, I calculated the energetics of radio sources as a function of the one unknown parameter, namely the strength of the magnetic fields in which the electrons are traveling. Thus I made the calculations in a series of papers published between 1956 and 1960, both for M87 and more distant radio sources. The minimum amounts of energy required is obtained when there is rough equipartition between the energy in the electrons plus protons and the energy contained in the magnetic fields. I found that for the extragalactic radio sources the minimum amounts of energy in the form of highly relativistic ($\sim$Gev) particles and fields (with $H_{eq} \sim 10^{-4}$–10^{-6} Gauss) must be very large ranging from $\sim 10^{56}$ erg ($\simeq 100\ M_{\odot} c^2$) for M87 to $\sim 10^{61}$ ergs ($\sim 10^7\ M_{\odot} c^2$) for more distant sources like Cygnus A. These were really startling results.

This work led me to consider that this might be where the primary cosmic rays come from. These ideas were very close to those that had been followed up for some time in the Soviet Union led by V.L. Ginsburg and his colleagues (cf. the monograph entitled *The Origin of Cosmic Rays*, by V.L. Ginsburg and S.R. Syrovatsky). The main difference was that they believed that the sources were supernova remnants in our own Galaxy. They were undoubtedly ahead of me, but I was completely unaware of this in the 1950s.

Of course, the primary cosmic rays are largely protons, and the radio sources only tell us about the electrons (and positrons if they can survive). We know that in our galaxy the ratio $E_p/E_e \simeq 100$, which Fermi before (1949) had explained as being due to the relative vulnerability of electrons compared to protons in traveling through the interstellar gas.

In the Soviet Union, Ginzburg and colleagues took the position that the radio emitting supernova remnants show that the relativistic electrons are contained and/or reaccelerated long after the outburst. Thus these supernovae are likely places of origin for cosmic rays (which are roughly contained to the galaxy and its halo—if it has one). Of course we now know that supernovae sometimes leave behind a rotating neutron star, a pulsar (not discovered until 1968), and this may also be a source of cosmic rays. Thus the idea that galactic supernovae are the source of cosmic rays is a point of view that is widely accepted today.

But from the beginning I had a different view. I was so impressed by the very large amounts of energy that were clearly being generated in the nuclei of the galaxies in the form of relativistic particles that I began to think that perhaps the whole of the flux observed in our galaxy was dominated by extragalactic particles pouring in rather than out.

Over the next decade or more I developed this thinking in several papers culminating with a long paper published with K. Brecher in 1972. I first attended the cosmic ray conference in Jaipur, India, in 1963 and presented my view, and then for many years Ginzburg and I debated these issues at a series of cosmic ray conferences around the world. In this way I got to know and admire Vitaly Ginzburg. We agreed that the very highest energy cosmic rays, if they are protons with energies greater than about 10^{19} Gev, cannot be contained or even accelerated in our Galaxy. Many more high-energy events are now being discovered, and of course, there are new attempts to explain the origin of the highest energy particles using exotic cosmological models, but we have no observational evidence to corroborate them.

This work led me even further afield, to consider the energetics of the intergalactic medium where even less is known. In the same period Fred Hoyle and I set a very severe limit to how much antimatter could exist in the universe, a result that always irritated Hannes Alfvén who had a cosmological theory based on the idea that matter and antimatter coexist in the universe.

People

When we first arrived in Pasadena we only knew Willy Fowler from Cambridge and Allan Sandage from our stay in Michigan. Willy had a marvelous experimental group at Caltech in the Kellogg Radiation Laboratory. The group had been formed by Charles Lauritzen originally from Denmark and Willy had been one of his graduate students at Caltech in the 1930s. At this stage Charles was still there, but it was Fowler together with Charles' son, Tommy Lauritzen, and Charlie Barnes who were the senior members of the experimental group, together with Robert Christy, a great theorist. We got to know all of them, both professionally and at Willy's parties, where I learned to drink martinis out of tumblers.

And then there was Richard Feynman who was very demanding, but who was easy to talk to, provided you really understood what you were talking about. He was quite keen on astrophysics and had the very rare ability to teach you a great deal about your own problems just by talking with you. I took to him all of the results on the energetics of radio sources, etc., that I was so excited about. He really made me realize that the number itself, $\sim 10^{60}$ ergs of relativistic particles, must be telling us a great deal about the properties of the large-scale universe.

The astronomers of the Mount Wilson offices were very different from the Caltech physicists. The older ones, Bowen, Baade, Humason, Minkowski, and Paul Merrill to name some, were all remarkable men.

Ira Bowen, the director who was an astrophysicist and a brilliant optical expert, had been appointed as director in 1946 in large part to get the 200-inch Hale telescope finished and operational. He was very conservative, but very fair. He was also quite taciturn. He had been appointed in the teeth of opposition by Edwin Hubble, who had badly wanted the job himself. Hubble was so upset at Bowen's appointment that he had said he would not let the director have any say in his research program. Thus Bowen always took a very hands-off approach to whatever his staff worked on provided that they did not compete with each other.

Walter Baade was always interested in talking about problems and advising younger people, and he was most enjoyable when he was debating or arguing with us.

Before the war, Baade had collaborated with Fritz Zwicky, the only astrophysicist at Caltech at that time. They had done fundamental work on supernovae together, but by the time that Margaret and I met them they were sworn enemies. Zwicky had quarreled with most of the older Mount Wilson staff, but he was quite pleasant to us. His book entitled *Morphological Astronomy* gave some flavor of his views.

Milton Humason was an observer of the old school. At the time we were there he was trying to obtain redshifts of galaxies so faint he had to offset from stars in the field of view, because the galaxies were too faint for him to see, and exposures of about 20 hours were required. He was the only astronomer I have ever known who had, and continued to use, a spittoon in his office. He used to go to the races at Santa Anita, and he was also a keen fisherman. He was one of the best observers but he had never had any academic training. He had been appointed from the support staff to the scientific staff in 1919. Paul Merrill was also an excellent stellar spectroscopist of the old school—he had little time for theory, but he had recently identified technicium (Tc) in stellar spectra, thus directly demonstrating that nuclear processes beyond hydrogen burning were going on in stars. Most of the older staff were extremely conservative politically and at lunchtimes they were friendly, but had no time for liberals like Sandage, or foreign (socialistic) opinions from me and others.

Through Milton Humason and Allan Sandage we were introduced to Grace Hubble, the widow of Edwin Hubble, who had died in 1953 before we arrived in Pasadena. She became a good friend, and we often visited her for tea at her house in San Marino. She still maintained Edwin's desk and his working equipment, particularly his pipes, and our daughter played with their cat.

The Hubbles did not mix with the other astronomers but they moved in very different social circles in Los Angeles. They were friendly with many of the Hollywood people who were prominent in the 1930s and 1940s—Errol Flynn, Groucho and Harpo Marx, and many others. On one occasion Grace invited us over to tea with Aldous Huxley who was living with Gerald Heard in Beverley Hills at the time.

She and Edwin were very well disposed to the English. I found out later that Fred Hoyle had been given the same treatment as us. Grace was very kind and generous to us.

CHICAGO AND YERKES, 1957–1962

In the autumn of 1957 Margaret, Sarah, and I moved to Williams Bay, Wisconsin, where we had both been given long-term appointments. We were also made members of the Fermi Institute on the campus in Chicago. By then Otto Struve had moved on to Berkeley, and the director of Yerkes had been Bengt Stromgren while we were originally there in 1952–53, but by 1957 Gerard Kuiper had taken over. Other senior astronomers there at that time were B. Stromgren, Chandra who was undoubtedly the king maker, W.W. Morgan, and A. Hiltner; the younger people were Robert Kraft, Kevin Prendergast, J. Chamberlain, and others.

Williams Bay, Wisconsin, is a village on Lake Geneva, attractive in the summer and autumn but an appalling place to live in at other times. We worked very hard there, but escaped to McDonald, to Caltech, and to Europe quite often. Many of our projects started at Caltech were still going on.

This was the era when stellar evolution was much in vogue and we were invited to write a major section for the *Handbuch der Physik* entitled "Stellar Evolution." This appeared in 1958. I also wrote a prize essay on star formation that was published in 1962. I made it my business to read as widely as possible in areas in which I was most ignorant, and having become familiar with the work on stellar evolution, I thought that the best direction to go in next was to consider the evolution of galaxies.

When I started to read what had been done, I found that this was a virgin field of research. Although galaxies were being used for cosmology (redshifts were being measured by Humason, Minkowski, and Sandage in Pasadena and by Mayall at Lick; and photometry was being carried out at Pasadena by a few astronomers, particularly Joel Stebbins and Albert Whitford from the University of Wisconsin), very little had ever been done on deriving basic parameters of galaxies, particularly rotation curves for spirals and irregulars, velocity dispersions of stars in ellipticals, and the properties of the interstellar gas in galaxies. Pioneering work had been done on the rotation of M31 by H.W. Babcock, but there was little knowledge of the masses, angular momentum, etc., of galaxies in general.

Thus we decided to start a program to measure rotation curves. We had considered doing this during our last year at Pasadena, and undoubtedly Margaret could have done this if she (we) had been allowed to. But Carnegie Fellows (and any other nonpermanent staff) were not allowed to work on any programs that staff members were working on, and in any case Palomar was barred to all of us (only permanent staff were allowed to use the 200-inch telescope). However, Horace Babcock had spent a year (1939) at Yerkes and McDonald, and he had built a very fast, low dispersion spectrograph (the B spectrograph) for use at the prime focus of the McDonald 82-inch telescope. The spectrograph had a long slit (290 arc seconds) and was ideal for measuring galaxy rotation curves. It had been used for a program of spectroscopy of double galaxies by Thornton Page when he had been at Yerkes some years before.

After a long hunt at Yerkes and McDonald Margaret found the spectrograph, put it back together and got it mounted at the prime focus of the 82-inch telescope. This was done with the aid of Marlyn Krebs who became a good friend. It was a very difficult instrument to use, and it used film, not plates, but she managed to get good spectra. (It was only during this period from 1957–1962 that I spent a lot of time really acting as a night assistant—doing all of the darkroom work, cutting film, plates, developing, etc.) This was the only time when my experience as a teenager, where I had learned to cut glass at Burbidge and Sons, came in useful.

The spectrograph was very effective in the red, and thus the emission lines Hα and NII $\lambda\lambda$6548-6592 which are emitted by interstellar gas in many late-type spiral galaxies could be measured over a large part of the main body of the galaxy.

Laying the slit of the spectrograph along the major axis of the galaxy and then along the minor axis, we were able to measure the displacement of the emission line(s) and hence the motion of the gas with respect to the center. If the motion is purely due to rotation there will be no displacement along the minor axis. If there is, and we soon found that this was sometimes the case, spectra was taken at many different position angles. In this way we were able to measure the rotation curves of many spiral galaxies, and also show that in some cases there were noncircular motions, which we believed was due to the ejection of gas close to the nuclear regions of the galaxy.

In analyzing the rotation curves we brought our Yerkes colleague and good friend Kevin Prendergast into the group. Kevin devised a way of numerically solving the integral equation involving the rotation curve so that we could obtain the mass distribution as a function of radius and, hence, the total mass and the mass-to-light ratio could be found out to the furthest measured point.

Starting around 1959 and for about 10 years, we carried on this program and measured the rotation of some 30 Sb, SBb, Sc, and SBc galaxies and a few irregulars—far more than had ever been studied before. This was the beginning of a large program of observational studies of galaxies. The limitation of the spectrograph and detectors at that period meant that we were not able to measure the rotation curves much beyond their peaks. It was after we had left the field and moved on that Morton Roberts, and other radio astronomers using the 21-cm line, and Vera Rubin, who had worked with us in San Diego and who had the more modern spectrographs and image tubes, were able at optical wavelengths to extend the rotation curves in general and show that they nearly all tend to become flat beyond the peaks. It is this that has led most people to the conclusion that galaxies contain a large mass of dark matter. In modern times Vera has been given much credit for this. Indeed, she and Kent Ford did a great deal, but the 21-cm radio astronomers deserve equal, if not more, credit. Of course Horace Babcock had originally made such a measurement in M31.

It was our discovery of the presence of noncircular motions that first led to the conclusion that matter is pouring out of the center of galaxies. We did a rotation curve study of one of the classical Seyfert galaxies, NGC 1068, which allowed us to set limits to the mass at the center, suggesting that the very large motions of the gas giving the broad emission lines indicate that matter is pouring out of the center of the galaxies. In the same period Allan Sandage and Roger Lynds had shown that a major outburst was taking place in the nearby irregular galaxy M82.

Putting all of these data together, and including the distant radio sources, we, together with Sandage, published in 1963 what turned out to be an influential review article entitled, "Violent Events in the Nuclei of Galaxies," which was published in the *Reviews of Modern Physics*.

UNIVERSITY OF CALIFORNIA, SAN DIEGO

In 1962 we finally left Chicago and moved to the Physics Department of the La Jolla campus of the University of California (UCSD). The circumstances of our move will be described later.

Following the discovery of the powerful radio galaxies in the 1950s, accurate enough radio positions led, starting in 1960, to the identification of the quasi-stellar radio sources (quasars, or as I prefer to call them quasi-stellar objects—QSOs) by Allan Sandage and Tom Matthews, Cyril Hazard, and other radio astronomers. They were real enigmas, and it was not until 1962 that Maarten Schmidt and J.B. Oke showed, starting with 3C273, that QSOs have large redshifts. Very soon, redshifts as large as two were being detected and the race was on to understand them. The observers at Palomar, Lick (including Margaret and Tom Kinman), and Kitt Peak (with Roger Lynds) were observing redshifts at an increasing rate.

It soon became apparent that for QSOs there was no significant correlation between redshifts and apparent magnitudes, as is the case for galaxies, and from this the idea that the universe is expanding was found. Also they were the only extragalactic objects that varied in time. However it was clear that most astronomers wanted to believe, without proof, that the redshifts were cosmological in origin, and thus that QSOs could be used for cosmological investigations.

The major argument for this point of view appeared to be, first and foremost, continuity—because first, the QSOs have spectra very similar to those of the nuclei of Seyfert galaxies, so perhaps they are all the nuclei of active galaxies further and further away; and second, if they are closer by, there was no theory to explain the large redshifts. J. Kristian published an observational article supporting the continuity argument, but he excluded 3C273, which did not fit. The belief that the absence of a theory was evidence against the existence of noncosmological redshifts was, and still is, given considerable weight! For me, such an approach is unbelievable, and a bad way to do science.

I began to work extensively on the QSOs. In 1967 Margaret and I published the first monograph in which all of their known properties were discussed. With Fred Hoyle I began a long series of investigations. In 1966 with W. Sargent we showed that the rapid optical variability of the optical synchrotron radiation in these objects led to a real paradox if the QSOs lie at cosmological distances. This could only be got around by supposing that the radiating sources are surfaces that were expanding at highly relativistic speeds, an approach that was taken by L. Woltjer and Martin Rees, and a point of view that was immediately accepted by what was already developing into an "establishment" view. Hoyle and I were more skeptical, and also in 1966 we wrote a paper making the case that the QSOs might well be comparatively nearby objects, ejected from active galaxies [originally we believed that NGC 5128 (CentaurusA) was very important] and that the redshifts were intrinsic and had nothing to do with cosmology. It was already clear, as had been pointed out by Peter Strittmatter, that the redshifts could not be due to Doppler motions, because if they were, blueshifts would predominate, but there were, and are, no blueshifts.

But we had no new redshift theory and, as I have learned, if you make a new observational discovery, it is very helpful if a theory is ready and waiting to explain it. By the same token it is perilous to discover a phenomenon that is in direct conflict with a well-believed (but not necessarily established or correct) theoretical model! It simply needs to be believed by the right people.

Other observational phenomena associated with QSOs and radio sources that bear heavily on our understanding were soon discovered. They include the existence of very rapid radio variations in compact radio sources that are associated with angular motions of the order of 10^{-3} milliarc seconds per year, absorption lines in QSO spectra, and QSOs that appear very close to comparatively nearby galaxies.

The first of them was found by groups of radio astronomers led by Ken Kellermann, M. Cohen, and others using very long baseline interferometers. They showed that if the redshifts of the sources are small, the corresponding motions are less than or only a fraction of the velocity of light. However, for large redshifts the same motions translate into what have come to be known as superluminal motions with values of $\gamma = (1 - v^2/c^2)^{-1/2}$ often of the order of 5 to 10 or more. I have always considered it likely that superluminal motions as large as this are an artifact and that the belief that the redshifts are measures of distance is wrong. I have shown in two papers published in recent years that if the nearest low redshift galaxies next to the radio sources are the real physical sources, the velocities are reduced to values less than or of order of c. This of course means that the redshifts of those sources (often QSOs) are intrinsic.

However, once again the establishment point of view is to accept the reality of superluminal motions. I'm still highly skeptical, as I have never been able to convince myself or others that coherent motions corresponding to large values of γ can ever be maintained and certainly not over timescales of years, as is required from the observations. In fact, no one has been able to demonstrate this.

The absorption in the spectra of QSOs was discovered by us with Roger Lynds and Alan Stockton in 1966. Because the absorption redshift was very close to the emission redshift in the discovery of the effect in the QSO 3C191, it was obvious that the absorbing gas was part of the source. But very soon after this was discovered, it was shown that absorption was a general property of the spectra of high redshift QSOs, with many hundreds of lines present in the UV spectra, nearly all of them with absorption redshifts much less than emission redshift.

The strongest absorption line is Lyman alpha; this led Lynds to call this effect, when there are many lines, the Lyα forest. Of course, there are two possible explanations of the effect. The first is that the absorbing gas lies at many places between us and the source, and thus is direct evidence of the widespread existence of intergalactic gas. Alternatively, the absorbing gas may have been ejected from the QSO. In this case the differences between the redshifts of the different absorption components and the emission line redshift of the source are measures of the speeds of ejection. These can be quite high, but not highly relativistic. Also, if the first explanation is correct and the QSO redshifts are cosmological, this is direct evidence for the existence of intergalactic matter. However, if the QSOs are comparatively nearby, the ejection hypothesis must be correct, and in any case, even if the QSO redshifts are cosmological, the evidence for the widespread existence of intergalactic matter would disappear, because it would only show that individual QSOs eject gas. It soon became generally accepted that this is true for a minor class ($\sim$10%) of QSOs in which the absorption is intrinsic with ejection speeds up to 0.1–0.2c.

However, the majority opinion, based initially on a flawed statistical argument of Sargent and his associates, was, and is, that the absorption is extrinsic and this has become an important observational datum completely tied into the standard big bang cosmology (see later sections). I remain skeptical.

The third line of evidence concerning the distances of the QSOs was first produced by H.C. (Chip) Arp, a veteran staff astronomer in Pasadena who, in the late 1960s, began to find evidence that appeared to show that high redshift QSOs and radio sources often lie so close to low redshift bright galaxies that he argued they must be physically associated. This work, which went right in the face of the cosmological redshift hypothesis, was extremely unpopular. Chandra, at that time the editor of the *Astrophysical Journal*, asked Margaret and me to referee some of Arp's early papers because Chandra believed that we would indulge in fair play.

I remember an early paper that we heavily refereed, but in reworking it I became convinced that his evidence could not be ignored. The revised paper was published. Arp persisted in his observing program until, in the 1970s, he was forced off the telescopes at Mount Wilson and Palomar by his colleagues and so left to work in Germany. For me this was as far away from fair play as it was possible to go in professional astronomy. As time went on we and a few others became collaborators with Arp, and the evidence for noncosmological redshifts has grown stronger, as I shall describe below.

But for the community that was not prepared to accept these results, the treatment of Arp by his colleagues worked—an example had been made. Nowadays none of the younger generation is prepared to work on such a radical proposition because they know that if they do, they will get no support from their peers, no funding, and no observing time—the lifeblood of astronomy. Despite this atmosphere of conformity, the problems raised by the QSOs have led me on a long trail of investigations, one that is still far from complete as I shall discuss below.

LEIDEN, CAMBRIDGE, AND MUNICH, THE 1960s

For some years after we moved to the University of Chicago we often gravitated back to Caltech in the summers. A frequent pattern was to drive from Chicago to the McDonald Observatory in May, have a long observing run, and then drive directly out to Pasadena for some weeks in the summer.

Later in the 1960s, we were invited to visit Leiden where we stayed in the home of the Director, Jan Oort. These were always exciting visits with many scientific sessions with Oort and his colleagues. In those days he dominated a large part of European astronomy, and though he was very conservative in his approach to the new astrophysics, he was also a great pioneer in radio astronomy, and in Lo Woltjer, Maartin Schmidt, and Gart Westerhout, he had produced some of the best astronomers of my generation.

Jan Oort, to me, was one of the very few senior astronomers capable of considering new phenomena and changing his mind. I recall an occasion when I went to Leiden and made the case for explosive events in galaxies—even our own. Oort was totally opposed to this idea, but six months later I found that he had changed his mind. Of course in other ways he remained very conservative.

We also made several visits to the Max Planck Institute für Astrophysik in Munich. Rudi Kippenhahn was the director and he became a good friend. I also became very close to Judith Perry, a very bright theorist from New York. We worked on several problems involving QSOs together and I like to think that I had a positive effect on her life and career. Later she moved to Cambridge and finally gave up astronomy in favor of architecture.

In the spring of 1968 we spent a quarter in a sabbatical at Harvard (GB) and MIT (EMB).

X-RAY ASTRONOMY

While we were resident in Cambridge we became consultants for the X-ray Astronomy Group led by Ricardo Giaconni and Herb Gursky at American Science and Engineering (AS&E). When the binary nature of the galactic X-ray sources became clear Kevin Prendergast and I made a first calculation of the importance of viscosity in the transport of energy in accretion disks around neutron stars or black holes in binary systems. I became interested in the X-ray emission from extragalactic sources. Over the years I have concluded that this X-ray emission is associated with QSOs being ejected from centers of active galaxies. It appears to me that as the QSOs are ejected most of the energy comes out in the form of X rays, later on it will be optical radiation, and finally radio frequency energy. The mechanism of the production of X-ray photons is either due to the synchrotron process or it is inverse Compton radiation.

ASTRONOMICAL POLITICS

Like most young scientists, when I started I did not realize how much of your success or failure depends on the reactions of your colleagues, or where you come from. Of course this happens in all professions since we are all people and, whether we like it or not, scientific ideas and even observational results are never uncoupled from what others think about them, and about you.

Yerkes

My first real experience of astronomical politics came when I was appointed to the faculty at the University of Chicago. There Otto Struve had built up a great department with Chandra, Stromgren, Morgan, Kuiper, Hiltner, Bidelman, van Biesbroeck, and others. They had very different personalities, and not very much respect for each other, but Struve had been able to handle them.

After he left, things became more difficult. When we first arrived in 1952 Bengt Stromgren was director. Although he was an outstanding scientist he was far more interested in doing science than in being director. This is probably why he moved from Copenhagen to the United States. Consequently at Chicago things weren't done very well, particularly at McDonald, where there was no resident astronomer, but only a very competent lay superintendent, Marlyn Krebs. If you were down there observing and needed to have something done you soon learned that the way to get it done was to call Chandra, who would tell you that he didn't know how to do it but somehow it got done.

At the end of his term as director, Chandra recommended that Stromgren not be reappointed, and told Stromgren that that was his recommendation. Not surprisingly (except to Chandra) Stromgren left Chicago. Kuiper was appointed on Chandra's recommendation, as Stromgren had been before him. As we all soon learned, this was another mistake.

Kuiper was a great astronomer but he was extremely self-centered and made many bad decisions. Around 1959 things came to a head. We, the younger people—Margaret, me, Kraft, Prendergast, and Chamberlain, at least—went down to Chicago and told the Dean that we would all resign if Kuiper were reappointed as director. The Dean listened to us (none of us had tenure) and then he talked to Chandra, who told him that we were reliable. It turned out that the dean, Zachariasen, already had a poor opinion of Kuiper. He, Kuiper, had no idea that this revolution was occurring, and Chandra suggested that I tell him. So I did. Kuiper was not reappointed as director, particularly after he had contacted Texas telling them that he would move to the University of Texas in Austin if they would take over the McDonald Observatory! He had probably never heard of Machiavelli!

In the next two years all of the younger faculty at Yerkes left, and Kuiper went to the University of Arizona. I had had my first taste of astronomical politics. As an aftermath three years later, Kuiper sent the FBI after me because he concluded erroneously (as usual) that I had had something to do with the damage to a piece of equipment owned by the Air Force that he was using at the University of Arizona. Of course, he was wrong.

U.S. Astronomy in General

When I first arrived at the University of California (UC) in 1962, I was asked to serve on various UC committees. Many academics consider service on committees to be time wasting. At UC, the famous Harold Urey, who had recently moved from the University of Chicago, told me how to handle this problem. He said that if asked, you should always agree to serve on a committee, then attend the first one or two meetings and understand how the committee operates, the chairman's likes and dislikes, etc. Then at the next meeting you should be totally obnoxious, attacking everyone and generally disrupting the meeting. Then you should leave abruptly, and never resign or attend another meeting. This way you will not be bothered again, though you will still be on the committee. I didn't completely follow his advice, but I found that the method could be very effective, in one or two instances!

In the 1960s and 1970s, I served on several committees advising the National Science Foundation on research grants, etc. (I also served on several visiting committees.) I was Chairman of the Astronomy Advisory Committee for the NSF in 1966–1967. I also served on the Association of Universities for Research in Astronomy (AURA) board, which is responsible for the running of Kitt Peak National Observatory (KPNO), Cerro Tololo Interamerican Observatory (CTIO), and

Sacramento Peak Observatory from 1970–1974. In 1972 I was appointed to serve on the board of Associated Universities Inc. (AUI), which at that time was responsible both for the National Radio Astronomy Observatory (NRAO) and the Brookhaven National Laboratory. I served on this board until 1982. During this period the Very Large Array of NRAO was proposed, funded, and built near Soccoro, New Mexico. I was very pleased to have helped in a purely advisory capacity to get this done, though the real heroes were Dave Heeschen, the director of NRAO, and his staff.

Serving on both of the boards governing the National Optical Observatories and the National Radio Observatory in overlapping periods was highly instructive. AUI was much the better organization. It had a much better chairman in Gerry Tape, and its superiority was mostly due to the fact that most of the scientific members of the board were not as closely coupled to what was going on at the observatory, or laboratory, as were the astronomers on the AURA board, where there were many conflicts of interest. The optical astronomers on the AURA board were often intimately familiar with the observatory's telescopes and the AURA staff members. Some members of the AURA board came from observatories that felt they were in competition with KPNO and CTIO, and there was considerable jealousy over funding.

However, on the AUI board there were very few radio astronomers, and many of the physicists and others on the board did not use Brookhaven. Thus board members were always looking at problems with a view to doing their best for the whole organization and not looking out for themselves.

Director of KPNO

In 1978 I took a five-year leave of absence from UCSD to become the director of KPNO. I was not the first candidate asked to do this, but when I was invited I decided to accept, partly on the grounds that I had been telling my friends for a long time that such observatories were not being run well. I remained director until 1984 and it was the hardest job, but in some ways the most satisfying job that I have done so far in my life. I was the fourth director of KPNO, after Aden Meinel at the beginning, Nicholas Mayall who had been an observational astronomer at Lick for most of his life until he went to KPNO, and Leo Goldberg who had been director of the Michigan and then the Harvard observatories. In Goldberg's term the largest telescope, the 4-m, had been put into operation and, together with the 2.2-meter and many smaller telescopes, the Observatory could for the first time really provide good observing opportunities to a large community of astronomers. They could compete on roughly equal terms with the astronomers of the private observatories (Mount Wilson and Palomar) and the universities (primarily Lick and McDonald). It was for this purpose it had been built.

When I took over it was clear that my predecessor had not yet been able to make the observatory visitor friendly. This was largely owing to the fact that the staff had grown to believe that because they had helped in a major way to build it, they could use the observatory in the same way that staff members used the Lick Observatory and the

Mount Wilson and Palomar Observatories (largely for themselves). My instruction from the board was to make the astronomical community in general believe that it was their observatory, as it was, because the NSF had paid for it.

I succeeded in doing this, but it had to be done in the teeth of opposition from some very selfish staff members. I chose new managers for the different divisions, put two key individuals, Dale Schrage, an engineer, in charge of the engineering division, and Buddy Powell, in charge of the whole of the Kitt Peak mountain operation, and removed major responsibility from the senior astronomers. Neither Schrage nor Powell is an astronomer, but they both were dedicated to the view that we should provide first-class facilities for the optical astronomers. I also realized that, though many of the astronomers had tenure, the technical staff were the backbone of the establishment, and so I did everything I could to improve morale and conditions for them.

I also brought in Adelaide (Del) Hewitt as assistant to the director. She had graduated in engineering at Berkeley and had worked for many years with us as a key member of the research group at UCSD. She and I had become very close and she happily moved with me to Tucson.

For a year or so after appointment I had, as every director has, a honeymoon period when his decisions are fully supported and even praised by the outside world. But inevitably, and particularly when the operating budget does not keep up with inflation (and mine never did), and he is making many internal changes and usually having to say no, internal opposition that is immediately transmitted to the outside world by the staff grows. Inevitably he becomes less popular.

Despite the difficulties, I found that I could make decisions rapidly and usually correctly, and deal with people and crises on a day-to-day basis. This was largely because I had appointed a good staff of senior people who I always backed and we had mutual trust. There were many crises. At one stage the Air Force announced that they planned to build a new airstrip, which would only grow, very close to Kitt Peak. This clearly had to be stopped. The president of AURA, John Teem, and his assistant Kelly Welch, who was a retired Admiral and had run the U.S. Antarctic Base, obviously wanted no part of a political fight. So I went to our Congressional representatives (in the House) for help but got nowhere with them. However, we had a good friend in Senator Barry Goldwater, who at that time was the senior senator from Arizona and Chairman of the Senate Armed Services Committee. He was keenly interested in technology and periodically visited Kitt Peak along with his friends—many generals and other high ranking people. They came up to the mountain in a large bus with some aides and much alcohol. One of Goldwater's friends was a radio ham on our staff, and he and Barry often talked. On one occasion my reluctant role was to give an impromptu lecture on black holes to all of them when they were visiting the mountain. At his invitation I had visited Goldwater at his Senate office on several occasions when I was in Washington on NSF business.

In one attempt to stop the Air Force, I went with an astronomer from the University of Arizona, Roger Thompson, to the Pentagon, and we met with an undersecretary of the Air Force to plead our case. Of course, this didn't work either. So I finally went to Barry Goldwater and after I had assured him that the long-term effects of an airfield so close to Kitt Peak would be very bad, he simply said, "Don't worry Geoffrey, we'll stop it." Just like that.

About a week later I was sent a blind copy of an official letter written by Goldwater, as Chairman of the Armed Services Committee, to the Secretary of the Air Force telling him that the airstrip must not be built. The following week we had a call from a company in Oklahoma who told us that they had been funded to carry out an Environmental Impact Study of the Effect of an Airfield Near Kitt Peak. This study had been commissioned and paid for by the Air Force, and it was clear that the report would show to build it would be a mistake! We never heard anything more about it! This was, and is, my only experience with real politicians with real power. I always liked Barry Goldwater because he was absolutely straight in his approach to anything we ever discussed.

In my period as director of KPNO we were involved in the plans to build a much larger national telescope. A great deal of time and money was spent on this, as well as much committee work. Finally there was a report in which we advocated the construction of a giant (15 meters) multi-mirror telescope. But it was not to be.

The annual budget crunches went on, and we had to lay off many very good people. During my tenure, the staff was reduced from more than 350 in 1978 to about 260 in 1984. As many outside had claimed, there had been some overstaffing, but nothing approaching 20–25%. The trauma associated with firing good people involving a face-to-face discussion was to me the most painful thing I ever had to do. But I felt that in many cases I had to do it myself, and so I did.

Of course, there were the usual personnel problems. The most public and sensitive one for me was when a staff member and his wife tried to get me indicted in Federal Court for discrimination concerning a job that she had had, had been dismissed from legitimately in a lay-off period, but had been denied a new position by me when one opened up. This was all true, but my decisions were all based on the fact that she had been a very unsatisfactory employee. The accusation was embarrassing at least, and AURA hoped that it could be resolved quietly. But this was not to be. The staff member involved told a friend on the AURA board about the action. The member of the board raised it in an open meeting, by describing it with names in open session, and soon everyone in the community knew about it. In such Federal cases, the State Attorney General's office is first asked to investigate and see if a real case can be made for a Federal indictment. This was done in Tucson where many members of my staff gave evidence, myself included.

After several months, I was very pleased when I was asked to visit the assistant attorney general together with the AURA lawyers and was told that while some witnesses had likened me to Attila the Hun, I had been completely cleared. It had been a difficult time.

At the end of five years I was up for a second term as director and so I resigned as a professor at UCSD. I was then reappointed for a second term, as the director of Kitt Peak. But AURA was planning a reorganization of the structure of the observatories. The three observatories—KPNO, CTIO, and Sacramento Peak—were all funded by the NSF, so that when we went in with our financial requests every year we were all competing for the same funds. I was one of the people who had long thought that this was wrong; it would be so much better if AURA were given one budget and it decided how to divide it up between Northern Hemisphere astronomy, Southern Hemisphere astronomy, and solar astronomy.

The answer was clearly to put it all under one director and let him (or her) decide how to divide it up. AURA made these changes, and a new overall director had to be chosen. For a number of reasons, including obviously, but not completely, my own ego, I felt that I was a strong candidate for the job. However, several administrative members of the AURA board who supported me and knew that I supported the scheme warned me ahead of time that, for me personally, this was a dangerous situation.

And this turned out to be true. A search committee was set up, with both AURA staff and outsiders on it and they deliberated for several months. As a good friend of mine, Peter Strittmatter (then and now the Director of the Steward Observatory of the University of Arizona—he has served as director there for longer than anyone else I know) told me somewhat cynically that it appeared that the committee's main job was not to choose the next director, but to see that neither of us got it. [Peter is a much better politician than me, but he lives next door to KPNO and was quite critical of the way we spent our (NSF) money.]

And this is what happened. The person chosen was John Jeffries from the University of Hawaii, a contemporary of mine and a member of the AURA board. He and I overlapped uneasily for about a year, and then he chose his former administrator at the University of Hawaii, Sidney Woolf, to work under him as KPNO director. Thus in 1984 I resigned as director and came back to UCSD with Del Hewitt who had been given a position at UCSD. Of course, Jeffries almost immediately got into trouble with the NSF, and only lasted about two years and Sydney Woolf was appointed to succeed him.

Our plan to build a giant multiple mirror telescope (MMT) as the U.S. National telescope never went forward. It was cancelled, and National Optical Astronomy Observatory (NOAO) finally decided, with several collaborators, to build the two 8-m telescopes, one in Hawaii and one in Chile (the Gemini project). This is what the NSF wanted them to do, and it has left the NOAO with only part ownership of an observatory that does not even have the largest telescope. Had I stayed in Tucson I would have probably fought this decision but, of course, I might have lost. However, I did have one good idea that might have worked. I had always thought (chauvinistically, though I'm a Brit) that the United States would do best if it could build the largest telescope in the world. I also knew that Barry Goldwater was about to retire from the Senate. Thus I was planning to propose that the money could be raised by a vote of Congress to build such an instrument in honor of Goldwater. Whether or not this political argument would have ever flown we shall never know.

Finally, it is fair to point out that during my period as director I was not able to do much research.

Cambridge and English Politics

Starting in the early 1960s we spent parts of summers in Cambridge, where we rented a flat in Churchill College. We were continuing research with Fred Hoyle and Willy

Fowler. The Science Research Council (SRC) was prepared to support an institute for theoretical research in astrophysics and Hoyle, who had been elected Plumian Professor, proposed that this be set up in Cambridge. All kinds of objections were raised, but the Institute went forward. We helped with the planning, and with the help of Sir John Cockroft, the Master at Churchill College, and Lord Todd, and the SRC, the funding was arranged. Hoyle chose a building design based on the Institute of Geophysics and Planetary Physics where we then had offices at UCSD. Finally the Institute of Theoretical Astronomy (IOTA) was built and completed in 1966 and it was an instant success.

This was the era where it became clear to many astronomers around the world that there was a pressing need to build more large reflecting telescopes in good climates, similar to those that existed in the United States at Palomar, Mount Wilson, Lick, Kitt Peak, and other sites.

After the war in the 1940s, Harry Plaskett, then professor at Oxford, had proposed that a large reflector be constructed by the British government. This suggestion had been taken up and implemented, but for political reasons the telescope, the Isaac Newton 98-inch telescope, had been put on the site of the Royal Observatory at Herstmonceaux Castle in Sussex. For decades it had the distinction of being the only large modern telescope ever built and placed at sea level! On some evenings the sea mist could be seen rising over the telescope dome. It was the ultimate white elephant, but many leading British astronomers strongly defended it.

In the 1960s Fred Hoyle had a major role in planning for the future in the United Kingdom, and he was made chairman of a committee called the Northern Hemisphere Review Committee, which was charged by the SRC to make recommendations for future observational facilities of optical wavelengths. The committee consisted of Hoyle as chairman; the Astronomers Royal for England and Scotland (Richard Woolley and Herman Bruck); several professors of physics; Bernard Lovell, the director of Jodrell Bank; and an extremely able civil servant with a mathematical background, James Hosie. Hoyle then added two expatriate astronomers to serve, me and W. Sargent, who by then was at Caltech. Of course, the two Astronomers Royal immediately objected to our participation as far as voting rights were concerned, but their objections were overcome.

We had a long series of two-day meetings for which the two of us traveled from California to London or Edinburgh. Our theme, as Hoyle was aware, was that the future should involve building a British national observatory with large telescopes at or very near a good observing site. We discussed in detail the kind of observatories that had been built in the United States in Arizona and in Chile. Possible sites were in the southwest of the United States, in Hawaii, northern Chile, southwest Africa, the Canary Islands, and possibly Australia, although it has no high mountains.

Already, across the Channel, the Europeans under the leadership of people like Jan Oort from the Netherlands, and Walter Baade and Otto Heckmann from Germany, were planning the European Southern Observatory (ESO), which was to be built in Chile.

Most members of the committee were convinced by our arguments, but there were strong objections from the directors of the two national observatories, the

Astronomers Royal. They obviously saw that if this were done, much of the power, prestige, and money that was spent on the Royal Observatories would be diverted, but it was clear to all of us except the Astronomers Royal that there was no way in which an organization like the Royal Observatory Greenwich (RGO) at Herstmonceux could be transformed into a national observatory that could serve everyone.

Thus, the final majority report, in which we described to the SRC how the new structure could be set up, was accompanied by a minority report from Woolley and Bruck advocating that anything new be controlled and run by the existing Royal Observatories.

Practically all of the astronomers in the United Kingdom were consulted at our various meetings, and my impression was that many of them were supportive of the new proposal. But some still wanted things the way they had always been.

The report was never published by the SRC. My belief is that, though they wanted to implement our recommendations, Brian Flowers, at that time the chairman of the SRC, was not prepared to stand up and deal with the critics. John Maddox, the editor of *Nature* at the time, and a friend of mine, wanted the report leaked but I wouldn't do it. Now I wish I had. A year or two after this episode, Richard Woolley, the Astronomer Royal and director of the Royal Observatory at Greenwich (RGO), reached retiring age and had to be replaced.

The SRC had to make a decision, and finally Brian Flowers nominated the leading expatriate observational astronomer, Margaret Burbidge. She reluctantly accepted the appointment that, for the first time in 300 years, did not carry the title of Astronomer Royal. This was given to Martin Ryle. We were told that if we both came back to England I would be given a senior civil service appointment at the RGO. For Margaret things did not work out well, because the SRC was not prepared to support her in most of the changes she wanted to make, and my hands were tied when I tried to make improvements in the British plans for the future. Many of our contemporaries in England simply resented expatriates, even Margaret, and showed it.

What followed was a typically British solution to a difficult problem. The SRC realized that reform was required, but made too many compromises to individuals whose egos were being badly bruised, and from whom decisions would be taken away. Thus their solution failed.

In the summer of 1972, the frustrations associated with the reception of the Northern Review Committee report and the difficulties associated with attempts to improve the situation at Herstmonceaux led me to decide to go public. Thus I published a highly critical analysis of British astronomy in a letter to *Nature* and also a letter to the *Times* of London. This led to a great deal of publicity and great anger from the establishment, but support from many of the younger British astronomers and from many overseas. No one had ever stated these things publicly before.

After spending one summer in Herstmonceux Castle I returned with my daughter to La Jolla. Margaret remained as director of the RGO for two and a half years and returned to California in 1972. Many things had gone wrong. Our effectiveness with Fred Hoyle had been reduced because, by 1972, he had had so much difficulty in Cambridge that he resigned his chair and the directorship of IOTA and left Cambridge for good.

The one good thing that came out of this failed piece of politics was that Margaret and Fred Hoyle were very influential in the planning of, building up of, and choosing staff for the Anglo-Australian Telescope, which was the first modern major venture of the British astronomical establishment after the war. For much of the time Hoyle was chairman of the responsible committee and Margaret was a British member. Together with Hosie and T. Bowen, one of the Australians (from Wales), they pushed though major design changes for the Anglo-Australian Telescope and ultimately appointed the first director, J. Wampler from Lick Observatory.

Thus a first-class observational astronomer became the first director of a modern observatory in a good climate, and half of it, at least, was British. Of course, it has turned out to be very successful. This gave me some personal satisfaction, but overall our attempt to return to the United Kingdom turned out to be a mistake.

MORE SCIENCE

Extragalactic Energy Sources

The results that I had obtained in the 1950s and 1960s concerning the very powerful extragalactic sources—radio galaxies, Seyfert galaxies, and QSOs—led me to ask what energy sources could possibly be responsible, and also to ask how efficient they must be because a large fraction of what we detect is a huge flux of relativistic electrons with an assumed, but undetected, flux of protons. This was a burning question, and it still is.

In 1965 I was invited to discuss this general question at the Solvay Conference in Brussels. At Solvay meetings, presentations are made before the Solvay Commission, which was made up of very distinguished physicists; at that time the commission consisted of Robert Oppenheimer, Werner Heisenberg, Emilio Segré, Christian Møller, and others.

Like the other speakers I was subjected to a detailed cross-examination, the main issue being the efficiency with which nature could give rise to the colossal amounts of energy in the form of relativistic particles as was manifested by the synchrotron radio and optical sources. The minimum total energies, conservatively, for the high redshift sources are 10^{60}–10^{61} ergs and there is no real evidence even today that the conservative models are correct. (The minimum corresponds to rough equipartition. Any departure will lead to more energy in the particles or the magnetic fields.)

At the meeting I was asked if I knew how efficiently beams of relativistic particles could be generated on Earth in particle accelerators. I didn't know the answer, but with the help of Emilio Segré at the meeting and afterward, I found that the efficiency, measured in terms of the energy in the beam divided by the energy input, is about 1% (10^{-2}) for a linear accelerator and only 0.1% to 0.01% (10^{-3} to 10^{-4}) for a synchrotron.

Thermodynamics shows that the bulk of the power must ultimately end up in very low-energy photons—in the acceleration process and in heat that is dissipated in the magnets. In my discussions with him, Feynman had also been worrying about this problem and he also pointed out that as far as man-made accelerators are concerned, it would be wrong to omit the energy used up in building the accelerators.

This all left me with what I still believe is a major problem in astrophysics. How can nature be more efficient than this? And, if it is not, what does it tell us? Of course, if the answer to the first question is no, then there must be much more energy than we can use being released elsewhere, in the form of radiation from lower temperature gas, or even neutrinos. It also means that the sources may be generating 10^{63} ergs or more in the most distant and powerful cases.

How efficient can nature be? My first idea was that perhaps the radio galaxies were releasing energy because in the nuclei of the parent galaxies the stars are so close together that one supernova will trigger others and there will be a chain reaction of supernova explosions (*Nature* in 1960). Later on, after pulsars had been identified as energetic supernova remnants, I wondered whether the extended radii lobes were powered by large numbers of ejected pulsars.

We know that stars do evolve and explode as supernovae. The total amount of energy that is available for release in a supernova per solar mass is about 10^{53} ergs, but most of this comes out in the form of neutrinos. Only 10^{52} ergs or less is left and what we see is 10^{50}–10^{51} ergs as ejected gas and high-energy particles with a rapidly rotating neutron star or black hole left. Most of the energy of the pulsar in turn is rotational energy.

Fred Hoyle and Willy Fowler did not like my supernova chain reaction idea and began to work on the idea that massive stars (10^6 $M_\odot$ or larger) were releasing gravitational energy as they collapsed. They ignored the question of where the massive stars came from in the first place. It was obvious by this time that the only possibilities were the energy released in gravitational collapse, or the energy was due to creation in the center of galaxies. In 1964 Hoyle, Fowler, Margaret, and I discussed all of this in a paper published in the *Astrophysical Journal*. Hoyle and I were quite keen on the creation idea, but it was soft pedaled in that paper. By then, following the publicity generated by the Texas meetings on Relativistic Astrophysics, the field was swamped by theorists working on gravitational collapse, which was generally adopted as the solution, though it is usually stated, incorrectly, that Donald Lynden Bell and Martin Rees in 1970 were the primary movers.

We all know that the basic problem is that enough energy is available in gravitational collapse, but that there are severe limits to how much of the gravitational energy can be extracted, only a few percent, unless we go to the Kerr solution for a rotating black hole where the limit usually used is about 8%. Although much work has been done by many people since then on this problem, largely pioneered by Rees and Roger Blandford, I believe that this model really doesn't work because (*a*) it is not at all obvious why the energy will ultimately end up in the form of Gev energy particles, and (*b*) no serious suggestions have been made that deal with the problem of the low efficiency.

On the observational side, there have been many observations of comparatively nearby galaxies including our own that suggest they contain a massive black hole. Thus it is argued that matter spiraling into the hole is responsible for the outbursts we see.

Although there are many nearby galaxies in which there is some evidence based on the stellar dynamics that there is a dark mass in the center, it is not possible to test this directly in an active galaxy, let alone a QSO. The evidence that is used to conclude that QSOs always lie in galaxies is also flawed, if not plain wrong. Never mind! What is now done is to estimate the rate of energy release we can detect in the active system, and then use that to estimate the mass of the black hole—assuming a very high efficiency of energy conversion. This, of course, is a bootstrap argument of the worst kind, but it is generally accepted. It is taken even further by those who feel that a relationship has been established between the size of a galactic bulge and the mass of the central (completely unseen) black hole. By this means it is argued that we can understand the energetics of even the largest redshift QSOs, which are the most luminous if the redshifts are of cosmological origin.

After 40 years of work I still believe that the large energies in the form of relativistic particles and magnetic flux comprise a major unsolved problem, and that ultimately we shall find that these sources are telling us something new about physics.

Dark Matter

In the universe most galaxies lie in groups or clusters. They range all the way from very small groups to rich clusters that contain hundreds or thousands of galaxies.

In 1958–1959, Margaret and I made an observational study of a remarkable cluster in Hercules, which contains many spiral galaxies as well as ellipticals and irregular systems. The cluster has an asymmetrical configuration. We measured the redshifts of many of the brighter systems, and having obtained a mean redshift of the cluster we could measure the motions in the line of sight of each galaxy. By assuming mass-to-light ratios for the galaxies based on data for nearer systems, and making reasonable assumptions about the velocity components of each galaxy perpendicular to the line of sight, the total kinetic energy of the galaxies can be obtained. Also, by measuring their projected separations in the cluster the potential energy can be computed. When we did this we found that twice the kinetic energy $\Sigma_i M_i v_i^2$ was much greater than minus the potential energy Ω, which is

$$G\sum_{ij} \frac{M_i M_j}{r_{ij}}.$$

Clusters of galaxies have been known to exist since the late 1920s when Fritz Zwicky discussed them based on Mount Wilson observations. Although there were then very few clusters known in which many individual galaxies had had their redshifts measured, Zwicky, and those following him, always assumed that the clusters are relaxed and stable, so that the virial condition, that averaged over time, $2\,E_{kin} + \Omega = 0$ is fulfilled, i.e., the clusters are bound.

However, when clusters are studied in detail, it appears that the result we found for the Hercules cluster is generally true—the visible kinetic energy dominates over the potential energy, i.e., the virial condition is apparently violated, and this normally would mean that the cluster must either still be forming, or coming apart, or that the galaxies do not make up a single physical system. However, because for many clusters the distribution of the galaxies suggests that the cluster is relaxed and stable, the solution has been to suppose that the clusters in general contain large amounts of dark matter, enough to satisfy the virial relation. This frequently means that 90% or more of the mass must be dark, or else it is in the form of very hot diffuse gas, or something else. This solution has largely been advocated by theorists who have never really looked at clusters.

However, starting in the 1950s the Armenian astronomer Victor Ambartsumian, an extremely good theorist, began to make the case that many types of systems, particularly irregular systems, were unbound, and were coming apart. He stressed his argument by simply pointing out that there are many configurations that appear from what we see to be expanding, and so perhaps they just are doing this. He stressed that it was very important just to look at the observations.

At an IAU symposium in Santa Barbara, which I attended in 1961, with all of the protagonists present, he made this case as he had made it at the Solvay Conference, and elsewhere, using the Hercules cluster as an example. However, other establishment figures, particularly Jan Oort, would not have it—they could not understand what happened to the galaxies after clusters had disintegrated, and in any case they believed that galaxies were all old and that they must have formed soon after the big bang! This might mean that some galaxies are young as would be expected if the steady state model was correct. In other words, Ambartsumian's suggestion was much too radical, as it cast doubt on the general belief in a beginning soon after which galaxies formed.

For many people this was the beginning of the idea that the universe must largely be dominated by dark matter, which we could not detect directly. Of course, it is not unreasonable to argue that there is much dark matter around—because the end product of stellar evolution is dead stars (very old white dwarfs, neutron stars, black holes, etc.); but this is very far from accepting the view that whenever it appears necessary, the virial condition can be invoked.

Certainly the flat rotation curves of spiral galaxies indicate the presence of dark matter in individual galaxies. But as soon as we go to pairs of galaxies the virial problem arises. Are they all embedded in halos of dark matter? I doubt it.

We also began to find that there are observations of a number of very small compact groups of galaxies with one member that has an anomalous redshift. We had already found in 1959 that this was true for the fifth galaxy in a famous quintet—Stefan's quintet, which has a much smaller redshift than the others. As soon as this discovery was announced there was a flurry of papers; most tried to argue that the small redshift galaxy is much closer than all the others, but this question still remains open.

In fact, it turns out that out of about 100 very compact groups of galaxies catalogued by Paul Hickson (quartets and quintets) about 40% have one galaxy with a highly discrepant redshift. This either means (*a*) that one of the galaxies is literally being ejected from the remaining group, which may be expanding, or it might be bound if enough dark matter is present, or (*b*) that the discrepant redshift or blueshift is not a Doppler shift, or that the discrepant galaxy is either a foreground object or a background object.

The establishment point of view for all of these groups is that one should exclude the discrepant object, on the assumption that it is a foreground or background object, and that the virial condition can be used on the other members of the group. This nearly always leads to the conclusion that there is much dark matter in the group.

However, for me, as well as for Ambartsumian, statistical arguments and the observed morphology suggest that these systems have positive total energy and are coming apart. Most people believe that the virial argument can be applied in all cases, and that the total mass (luminous and dark) is proportional to the size of the system. I attempted to rebut this circular argument, which was first made by the Princeton group in 1974. My paper showing that the result is simply reached by always assuming that the virial held was published in 1975, but it has been largely ignored.

From the largest scale starting with the universe itself, I believe that expansion, sometime explosive, is an important cosmological feature of the observable cosmos. Expansion is going on in some clusters and groups of galaxies. It is also a common feature going on in the centers of many individual galaxies. This suggests that galaxy formation is often due to explosive events, and not always, if ever, simply a result of gravitational collapse.

It is probable that the general belief, that the formation of all condensed structures is a result of gravitational forces alone, stems from the fact that gravity is a well-established attractive force that works on many scales, but we have no theory to help us to explain expansion. When we see evidence that might suggest that one galaxy is being ejected from another, this possibility is ignored in part because we have no theoretical understanding of how such a process could occur. Pairs of interacting galaxies are always assumed to be merging. When this is the case we should be able to see tidal tails, as the Toomres pointed out in a brilliant theoretical analysis in 1972. Where such tails can be seen this is good evidence for mergers. But often there is no such evidence, though mergers are always assumed to be taking place. This is yet another bandwagon belief. I shall return to this question when I discuss cosmology and cosmogony in a later section.

INDIA

I made my first visit to India to attend the International Cosmic Ray Conference in Jaipur in November 1963 and present my work on the origin of cosmic rays. This was a delightful meeting at which I met many people, but particularly the very powerful group of Indian cosmic ray physicists who were working at The Institute for Fundamental Research in Bombay (the Tata Institute, now Mumbai). Many of them, and particularly Yash Pal, became good friends. I attended the cosmic ray conferences after that for many years and also visited and occasionally lectured at the Tata Institute and at various summer schools and elsewhere. In 1966 I spent about six weeks lecturing at a summer school in Bangalore, and I have lectured in Goa, Uddar Pradesh, and elsewhere.

In Cambridge while working with Fred Hoyle, I began to work with Jayant Narlikar who had been a student of Hoyle and later his collaborator in research into theoretical cosmology. When Narlikar moved back to India and became head of the astrophysics group at the Tata Institute, I began to make visits to work with him and also to consult with the cosmic ray group at Bombay.

In the early 1990s Narlikar was asked to become the first director of IUCCA, an institute set up and funded by the Indian government at Pune to engage in research in astrophysics and cosmology and build connections with the Indian universities. This led me making regular visits to Pune to work with Narlikar and his colleagues, something that I have continued to do ever since. All of my work in cosmology, which will be discussed later, has been carried out with F. Hoyle and J.V. Narlikar. In 1989–1990 I spent about two months at the Tata Institute working with Narlikar and A. Hewitt. Pune contains many first-class scientists including T. Padmanabhan and Dadhich who succeeded Narlikar as director in 2004. I was made an Honorary Fellow of IUCCA in 1999. I have the highest regard for Indian science and scientists and always enjoy my visits to Pune, Mumbai, Bangalore, and elsewhere.

EDITORIAL WORK

The Annual Review of Astronomy and Astrophysics

In 1960 the nonprofit corporation Annual Reviews, Inc., decided to start the *Annual Review of Astronomy and Astrophysics* (ARAA). Leo Goldberg from Harvard was chosen as the first editor-in-chief, and together with an advisory committee of six, of whom I was one, topics and authors for the first volume were chosen. The first volume appeared in 1963. I served on the editorial committee for the first five years. In 1973 Goldberg resigned, and I was appointed editor-in-chief, a position I held for more than thirty years, until the end of 2004. For most of that time I had two associate editors. We met once a year, often, but not exclusively, in California with an editorial committee (the members of the committee have staggered terms so that one retires each year).

I have always wanted to learn as much as I could about almost all fields of astrophysics, and I found that this editorial position was very much to my liking, because it led me to correspond with, get to know, and meet many astronomers who work in fields very far from my own. Over the years I have read nearly all of the reviews that we received from the contributors, all of whom are chosen as leading experts in their field at our annual meetings. Goldberg had adopted a policy of not refereeing the reviews and I continued in that tradition.

The reviews are edited mostly by the associate editors who sometimes produce long reports that the authors receive, but they know that they do not have to make any changes if they don't want to. Of course, occasionally we would receive a review that was too narrow or did not cover enough of the recent work, but we always published it. Over the years I received very few complaints from the readers.

In the 1970s we first decided that each year we would invite a distinguished astrophysicist to write an introductory review on whatever topic he/she chose. The first one by E.J. Opik was published in 1977.

Over the years ARAA has become a valuable review journal for astrophysics. It appears to be well thought of professionally (it is very high in the citation rankings) and it is used extensively by those who want to move into a new field, by graduate students, and others. The contributors get no payment, and the editors and associate editors receive very small honoraria. During my years as editor, I attempted to improve that situation particularly for the editors who are the most important components if a good volume is to appear. Here my attempts completely failed. Probably I was not very diplomatic about this.

I have always believed that it is very important to understand as much as possible of the background to any scientific problem and the efforts that have been made to explore it in the past. ARAA tries to provide this material for many areas of astrophysics. I am very satisfied with what we have achieved in more than 40 years since the first volume of ARAA appeared. My successors will undoubtedly maintain high standards.

The Astrophysical Journal

The correctness and worth of any scientific result can only be evaluated by scientists who are able to properly study and understand it. This is what is often called peer review. Astrophysics is an observational (not an experimental) science and a theoretical science. For peer review to be practiced all astrophysical results must be described in enough detail, either orally or written (nowadays by electronic means), so that others can evaluate the work before publication. The results (the paper) must be submitted to a reputable journal, anonymously refereed by other (competent) scientists, and then published. Nowadays the trend is to send out the paper by electronic means, often before refereeing and long before the paper is officially accepted and published. This is done in part for competitive reasons, but also, we are told, to get reactions from others before publication. In my old-fashioned way of thinking this is a mistake, but the practice has grown to the extent that in many fields working scientists get all of their information from electronic preprints and don't read journals. But as Chandra once put it to me, research is not completed until it is written up and published in a refereed journal. Because I believe in this principle, in 1995 I volunteered to become one of the scientific editors of the *Astrophysical Journal* (the editor-in-chief at that time was Helmut Abt) and I did this work until 2001.

I found that the most important aspect of the job was to choose competent referees who would review the paper thoroughly and write a clear and dispassionate report in a reasonable time. In time I built up a good list of such individuals. After receiving the report I would edit it and send it to the author of the paper. I believe in deliberately editing referees' reports because they sometimes (often) contain remarks that the author will see as personally biased, reflecting on the author's institution (we—the referee's facility, could have done a better job, etc.), or insulting. Sometimes the flavor of the old song, "Anything you can do I can do better; I can do anything better than you," would come through, as well as jealousy of some older scientists toward the younger ones.

Having received the author's reply to the report I would try and make a judgment, and as often as possible (sometimes after revisions) I would accept the paper and it would be published. Of course, some of the time the report was so critical that even after the authors' reply, and in some cases after a second referee was consulted, I would reject the paper.

The most important aspect of this kind of editing is that the editor must act as a judge, and not an advocate or a defense lawyer. This is where my very strong belief in fair play enters in. As an editor I always tried to be fair, even in situations in which my own interests were involved. If the editor allows his own point of view to enter and sends the paper to a referee who agrees with him, this is the kiss of death for any authors who don't also agree with the editor and referee. It is largely for this reason that the peer review system—involving the anonymous evaluation of astrophysical projects, be they papers, the assignment of observing time on large telescopes, funding, etc—is badly bent, if not completely broken. This is because many factors other than the worth and correctness of the scientific paper or project are being weighed when decisions are made.

This is also one of the reasons why the bandwagon effect, which has reached enormous proportions in some areas of astrophysics, prevails. Minority views are not given fair weight. Public relations departments in universities, and government agencies (particularly NASA), where decisions are made not on the basis of science but on the basis of who you are, and where you are, are particularly guilty. And of course, we are all individuals who often find it difficult to control our own feelings of superiority or jealousy when someone else makes a great discovery, particularly when it doesn't fit in with our own beliefs.

SOME THOUGHTS ON COSMOGONY AND COSMOLOGY

When I was at the Yerkes Observatory in the 1950s and 1960s, I had many long discussions with Chandra who was keenly interested in the behavior patterns of well-known astronomers, both living and dead. One of his themes was that many go in the wrong direction, toward origin problems and cosmology after about the age of 50. Chandra was 49 at the time. For example, he had told me that he thought that both Eddington and Jeans had gone off the rails late in their careers—in the case of Eddington because of his devotion to his "Fundamental Theory."

Chandra obviously believed that the danger lay in trying to solve origin problems, and certainly cosmology and cosmogony, after one had worked on more down-to-earth investigations. Of course, Chandra himself in his later years did some of his best work in gravitational theory and general relativity.

I was always skeptical of the approaches that were taken and the strong beliefs that were held concerning cosmological models. The only serious debate, which has been going on for the last fifty years or more, has been that between those who believe in a beginning (the big bang) and those who believe in the classical steady state model. In 1970 I attempted to review all of the observational evidence (*Nature* 1970) and concluded that the agreement between the timescales for the ages of chemical elements based on radioactivity, stellar evolution, and the inverse of the Hubble constant ($H_o{}^{-1}$) was a powerful general argument for a beginning in cosmology, though Fred Hoyle always emphasized to me that the average age in the steady state is $(3H_o)^{-1}$, which is not so different. However, for me the greatest unsolved problem was why the matter that we can see—the visible universe—is made up largely of galaxies (lumps) and is not a smooth continuum of matter and radiation.

Of course, there has been much new cosmological information obtained since then, but it soon became clear to me that it would be very difficult to make real progress in understanding because the leading practitioners in the field had already made up their minds. I believe that I know or have known most of the leading cosmologists of my generation and younger, but I only knew one, Dennis Sciama, who ever changed his mind. He went from a passionate believer in the steady state when I first knew him in Cambridge to an equally convinced believer in the big bang. In my view (based on belief and not on evidence) this attitude has contributed to the situation we have reached today when it is stated with great confidence that the universe did have a beginning and that a complicated sequence of events, all of which can be understood in terms of the known laws of physics, has taken place.

In the early 1990s, when I first began to study the situation in detail, there were two factors that I thought were very important. The first was the observational evidence and how it was interpreted. The second was the view of Fred Hoyle who I worked with on so many astrophysical problems.

As I have said many times, in many places, progress in cosmology is entirely dependent on observational discoveries. The most important of these, made in the 1920s, was first that the nebulae (galaxies) are indeed systems similar to our own Milky Way, but much further away, and second, that that they followed a tight redshift apparent magnitude relation. Both of these discoveries, largely attributed to Edwin Hubble, led almost immediately in 1929–1930 to theorists relating the observations to the Friedmann-Lemaître solutions to Einstein's equations and hence to the realization that the universe is expanding. Given this situation, the only interpretations that were thought to be possible were that (by reversing the time axis) it was clear that there must have been a beginning, that the expansion would continue, and only observations could determine whether it was slowing down or speeding up, or that the observed expansion continued without changing in any way.

Of course, there was also the possibility that the redshifts that were measured were not due to expansion at all. Some very well-known physicists—Fritz Zwicky, Edwin MacMillan, Max Born, and others—suggested this, but the known laws of atomic physics suggested that this would not work.

For about 20 years after the discovery, the only interpretation that had any credibility was that the Friedmann expansion is taking place, and the most interesting aspect of this to most physicists is to ask what took place at the beginning. However in 1948, based largely on Tommy Gold seriously proposing that the expansion that we observe might be independent of epoch, he and Hermann Bondi, and independently Fred Hoyle, proposed that the universe has always been expanding in a steady state. This hypothesis requires that creation of mass-energy takes place at a rate determined by the expansion. Of course, in principle there is no difference between the proposition that creation was a single event in the past, and that creation takes place continuously.

But from the sidelines it very soon became clear that most astronomers disliked the steady state idea. The best way to test it was to see if there was any evidence for evolution as a function of epoch. Ryle and his colleagues in Cambridge tried to do this by studying the counts of extragalactic radio sources (nearly all of which were unidentified), and Ryle announced with a great deal of publicity in 1955 that the so-called LogN/LogS curve showed that the steady state could be ruled out. I was working in the Cavendish at the time, nominally with Ryle and his group, but he was highly secretive and kept me outside the cosmology group. Hoyle and Narlikar attempted to dispute his conclusion and a very unpleasant dispute went on for several years, but Ryle had already convinced many of the leaders of opinion, particularly Jan Oort, that he was correct. It was not generally known for many years that in Australia Bernard Mills, at the same time, had shown that this approach was very difficult, and at that time the statistical evidence based on his counts was unreliable and Ryle's results could not be trusted. If this was raised Ryle would simply deny it, and he always got away with it. In the climate of the time Ryle was believed. (To be fair I should point out that my good friend Allan Sandage, who has debated these issues with me for 30 or 40 years, maintains that in Pasadena the observers never believed the steady state for reasons to do with the timescales and the ages of galaxies, etc.)

But the more interesting development was associated with the physics of the big bang. While in 1936 Lemaître wrote a paper entitled "The Primeval Atom," at that time nuclear physics was in its infancy, and little could be said about the physics of the very dense state out of which would ultimately lead to the visible universe.

In the late 1940s after the war some of the greatest physicists of the day, E. Fermi, G. Gamow, E. Teller, Maria Mayer, R. Peierls, and others, considered that one of the major unsolved problems was the origin of the chemical elements, and they concluded that to build them up from hydrogen a site was needed where there was a large supply of neutrons. George Gamow, Ralph Alpher, R. Herman, and others realized that this must be the early universe (the only place they could think of), and thus attempts were made to build the heavier elements in that phase. They soon found that only the lightest isotopes, D, ^{3}He, ^{4}He, and ^{7}Li, could be built in this way because there are no stable elements with mass 5 or 8.

Earlier Gamow had concluded that the amount of helium in the universe, already known to be about 25% by weight, could not have been made by hydrogen burning in stars because there was not enough time. At that time the large value of the Hubble constant given by Hubble and Humason gave an age of only about 2×10^9 years for the universe. Gamow concluded from this that the helium must have been made in the early universe. To do it he found that he had to assume that the initial ratio of photons to baryons, $\gamma_r/\gamma_b \gg 1$, whereas up to that time it had been supposed that $\gamma_r/\gamma_b \ll 1$.

Thus he made an arbitrary choice to get the right answer. The numerical value of this ratio used by Gamow, Alpher, and Herman is very close to the one used today to derive the abundances of the light isotopes and to compare them with the observed values.

Thus there is nothing fundamental about the agreement that has been reached between theory and observation, because it depends completely on the belief that Gamow had that there indeed was a big bang and that most of the helium was made in the first few minutes. This was a belief based on an incomplete and incorrect result.

However, since the early 1960s the main pillars of belief in the big bang have rested on (*a*) the expansion, which is real, (*b*) the belief that the light isotopes were made in the big bang, and (*c*) the discovery of the microwave background, always claimed to have been made by Penzias and Wilson in 1965 and predicted by Alpher, Herman, and George Gamow in 1949.

The microwave background measurements made in the 1960s have certainly dealt a severe blow to the original steady state cosmology (SSC). Almost immediately after they were interpreted, the argument against the SSC (based on the radio source counts from Ryle) was replaced by the background radiation discovery. Although some of us tried to understand this radiation field in terms of large numbers of discrete sources the solution seemed quite contrived. So much for the classical version of the SSC.

But how strongly do the major pillars of belief in the big bang based on (*b*) and (*c*) stand up? As far as (*b*) is concerned, the fact that Gamow originally chose an initial ratio of baryons to photons that would allow the production of the light elements indicates that making the observed ratios of those isotopes fit the theory is of no fundamental significance as far as testing the big bang hypothesis is concerned.

As far as (*c*) is concerned, it is certainly true that Gamow, Alpher, and Herman argued that if there was a beginning there would be a fireball that would expand in the form of a black body.

In the early 1960s Robert Dicke at Princeton reworked these ideas, and because he was an experimental physicist he attempted to go further and detect the radiation. He and D. Wilkinson nearly did this, but Penzias and Wilson serendipitously found the background radiation in 1965. It has now been found to be of almost perfect blackbody form. This had been predicted, and is a major triumph for the idea.

But the earlier history, which the Princeton cosmologists and others had forgotten or did not know, is important.

In the late 1930s optical astronomers, particularly Adams and McKellar, had detected absorption lines caused by interstellar molecules in the interstellar gas, and from molecular physics they were able to show that these molecules CH and CH^+ are sitting in a low-temperature radiation field.

In 1941, more than a decade before the work of Alpher, Herman, and Gamow, Andrew McKellar at the Dominion Astrophysical Observatory in Canada published a paper in which he showed, using the CH and CH^+ lines, that the temperature of this blackbody radiation must lie between 1.8 K and 3.4 K. Although there were no direct observations of the radiation field, this was a very accurate prediction, because we now know from the direct observations that $T = 2.726$ K. Although there is some uncertainty in the estimate of the energy density, it should be remembered that the blackbody temperature T is proportional to (energy density)$^{1/4}$.

In the big bang theory it is not possible to predict a temperature at all, and when Gamow and his colleagues, and later the Princeton group, speculated on the value of T they frequently assumed much higher values. These values were wildly wrong. Because the energy density of blackbody radiation is proportional to the fourth power of the temperature, even a value of T of 5 K, suggested by Gamow at one time, leads to an energy density more than ten times the measured value.

What happened of course was that Penzias and Wilson were given a Nobel Prize for the direct discovery of the blackbody radiation though McKellar had got the right answer twenty years before. (At least this shows that cosmologists don't read the literature!)

But there is more. Has this discovery really established that the radiation originated in a beginning?

In the 1950s I became interested in the origin of helium, and I realized that if it came from hydrogen burning I could calculate directly how much energy had been released. I made this calculation and published the result in 1958, and speculated on the various places where it might have occurred.

Unknown to me (I don't read literature either) Gold, Bondi, and Hoyle had done similar calculations in 1955 and had speculated that the hydrogen burning takes place in giant K stars.

Using the known values for the mass density of matter in the universe and a He/H ratio of 0.24, it turns out that the energy density is about 4.5×10^{-13} erg/cm^3. If it is assumed that this has all been degraded into blackbody energy, the blackbody temperature is $T \simeq 2.75$ K.

This is a remarkable result because it agrees so well with the observed temperature. For cosmologists who believe in a big bang it must be a coincidence!

Over the years Hoyle and I became convinced that it really does suggest that all of the helium has arisen from hydrogen burning in stars somewhere. Accepting this, we have a stellar source for all of the helium, so it is now hard to argue against the view that all of the isotopes including ^{2}D and Li are also produced in stars. The low temperature at which D will burn in a star's interior means that it must be produced in stellar flares, and there is some observational evidence that this does occur in the sun and elsewhere. In the mid-1990s we wrote a short paper on this, which was rejected by *Physical Review* because it got into the hands of Dave Schramm and others with passionate beliefs in the big bang. We finally published the paper in *Astrophysical Journal Letters* in 1998.

In the papers of 1955 and 1958, Gold, Bondi, Hoyle, and I made one critical omission. In neither paper did we point out that the calculated energy density meant that the blackbody temperature would be about 2.75 K. If we had done this there would have been a direct prediction of the blackbody temperature available (admittedly made by unpopular people) when the first measurements by Penzias and Wilson were made. This might have led to a different chain of argument and belief in the cosmological community.

As it is, the discovery of the microwave background has led to a tremendously detailed set of sophisticated observations from space of the structure of the radiation, all based on the absolute belief of the observers (who keep mistakenly calling themselves experimentalists) that they are observing phenomena that are closely related to the early universe. Most of them believe that there is only one viable cosmological model and no attempt is made to consider origins that do not start with a big bang. The reader can see from this that I believe that the pillars (*b*) and (*c*) on which much of big bang cosmology rests are not as strong as is generally advertised.

Some of the classical cosmologists, including W.H. McCrea, showed in the 1940s and 1950s that forming galaxies by gravitational collapse in an early phase of an expanding universe would not work. In order to construct a scenario in which it is possible to form galaxies, a whole series of assumptions have to be made.

These start with the idea that there are small density fluctuations present in the original material—possibly with a quantum origin. Then it is necessary, in order to get rid of the horizon and flatness problems and to do away with a high density of magnetic monopoles, to develop the concept of a very rapid, very early expansion phase—inflation. This idea, due to Guth and Linde, is very attractive, but it does not come out of pure theory and can never be directly tested.

In order to get the gravitational instability argument to work, it is necessary to invoke the presence of large amounts of cold dark matter, and because it is found that the amount required is in conflict with what is indicated from the ^{2}D abundance in primordial nucleosynthesis, it has had to be argued that this is a new kind of matter, nonbaryonic matter, for which there is no direct experimental or observational evidence.

Given all of these assumptions, extensive numerical studies have been made to attempt to get agreement between what is seen in the large-scale structure of visible galaxies in the universe and the theoretical distribution of matter that must largely be nonbaryonic.

Also, in the last few years it has been claimed that the universe is accelerating, and this has led to the revival of the idea that there must be a positive cosmological constant and what is called dark energy. (This claim is based on observations of supernovae of Type Ia at large redshifts. Though prizes have been given for this type of work, some very good observers are very skeptical of the results. But this is the basis for all of the excitement about dark energy.) In fact, long ago it was shown by Bondi and by Hoyle and Sandage in 1956 that any steady state universe would be accelerating and not decelerating, essentially because energy is being continuously created as the expansion continues. Thus, overall, I believe that the current bandwagon view is a much too contrived solution to the puzzle of the origin of galaxies to be correct.

What has been entirely left out of this scenario are the many observations, some of which I have mentioned earlier, which show that individual galaxies are ejecting large amounts of energy that we do not really understand. Also, there is much evidence that suggesting that the high redshift QSOs are being ejected from the active galaxies. This result is one that the conventional cosmologists have continuously ignored, but by now the evidence that this effect is present is overwhelming. Another huge problem it raises is that the redshifts of the QSOs do not have a cosmological origin.

The fact that there is no understanding of these redshifts is almost certainly one of the reasons why the data have been ignored. There is some evidence in favor of cosmological redshifts, but we are at one of those times when contradictions are rife; but for me, that is not a good reason to reject good data because we don't understand it.

All of these data led Fred Hoyle, Jayant Narlikar, and me in the early 1990s to propose a cosmological model, which is a modified version of the original steady state. We have argued that we live in a cyclic universe that is now in an expansion phase and has a period of about 20 billion years. It is largely driven by the creation processes, which take place in the nuclei of galaxies where we see all of the activity taking place, and where creation is occurring very close to the centers—the theory is that developed by Hoyle and Narlikar in the 1960s. Basically, galaxies beget galaxies. This can explain nearly all of the phenomenon that we observe, though we have not yet been able to understand the anomalous redshifts of the QSOs. This was all laid out in a monograph we published in 2000.

Although this is a very unpopular point of view, I believe that it is probably closer to the truth than the standard model. Only time will tell. Unfortunately, though it is generally believed that as science evolves, in time truth will tell, there is such a heavy bias against any minority point of view in cosmology that it may take a very long time for this to occur. This is because anyone who takes a university position will get no research support or time on telescopes, and the young people are well aware of this.

EPILOGUE

Ever since I started to do research in astrophysics I have become more and more aware that among all of the branches of physical science this one is least understood. This has meant that it is dominated by observational discoveries with theory always trying to catch up. It has also meant, in turn, that it is easy for simple and often superficial ideas to take hold. We all try to understand without violating the laws of physics. But any time that there is a real conflict between accepting these laws and suggesting that we are seeing something really new that will tell us we can learn more about physics than we knew before (often derogatorily called new physics), the preferred solution containing no new ideas is then built on so that the final model looks like an inverted pyramid. I have always tried to avoid this, but it is an unpopular way to go.

But I really enjoy the field that I accidentally fell into, possibly because I have been influenced by so many friends and colleagues along the way. As well as my father, Leslie Burbidge, Margaret Burbidge, and my daughter Sarah, many come to mind. I thank them all; and I may have omitted many by accident (the list is alphabetical and not chronological).

Chip Arp, Ken Brecher, Chandra, Dave de Young, Kate Ericson, Willy Fowler, Claude Gabriel, Riccardo Giacconi, V.L. Ginsburg, Bob Gould, Ellen Grabanski, Del Hewitt, Fred Hoyle, Vesa Junkkarinen, Ken Kellerman, Marlyn Krebs, Harrie Massey, Allan Maxwell, Peggy McCoy, Leonard Miles, Jayant Narlikar, Steve O'Dell, Beppo Occhialini, Jan Oort, Yash Pal, Pat Patterson, Judith Perry, Buddy Powell, Kevin Prendergast, Abdus Salam, Allan Sandage, Dale Schrage, Gian-Carlo Setti, V.C. Shetty, Wayne Stein, Peter Strittmatter, Dick Thompson, Betty Travell, A.M. Tyndall, Marie Helene Ulrich, Harold Urey, and Artie Wolfe. They have all helped me in different ways.

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