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ANNUAL REVIEWS

Annual Review of Astronomy and Astrophysics

Galaxies, Globular Clusters, and Dark Matter

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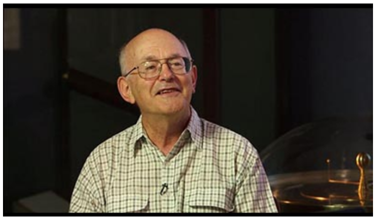

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Annu. Rev. Astron. Astrophys. 2017. 55:1–16

The *Annual Review of Astronomy and Astrophysics* is online at astro.annualreviews.org

https://doi.org/10.1146/annurev-astro-091916-055249

Keywords

biography, stellar dynamics, Milky Way, element abundances

Abstract

This is an autobiographical account of my scientifi career. My main research interest is the structure and assembly of galaxies. The assembly narrative has evolved from the monolithic and baryonic collapse picture of the early 1960s to the current hierarchical scenario underpinned by dark matter, and is still evolving. Technology has changed: CCDs replaced photographic plates and image tubes, large optical telescopes are much larger and instruments are much better, Galactic archaeology is supported by vast stellar surveys, and we have space astronomy and radio synthesis telescopes. The article describes the scientifi areas in which I have worked and the colleagues who have influence my progress. I have much to be grateful for: the people who have mentored and supported me over the years, the privilege of long-term collaborations, and the pleasure of advising many Ph.D. students and postdocs.

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1. EDUCATION IN AUSTRALIA

I grew up in Perth, Western Australia, one of the world's most isolated cities. My mother was a lawyer, and my father was a plumber and factory manager with academic inclinations. Late in life, he studied classics and earned his Ph.D. at the University of Western Australia (UWA) with a thesis on the Roman satirist Juvenal.

At school I did well at mathematics and it seemed likely that I would do something in that area for my career. We had mathematicians going back several generations on my mother's side of the family, and I think she was quite proud of bringing those genes into the family. From the age of 11, I attended a private school, Scotch College. In my last years of school, I was fortunate to have some outstanding teachers: My English teacher was a published short story writer, my Latin teacher became a professor of classics at the UWA, and an inspirational science teacher taught the entire sixth-form double mathematics, physics, and chemistry single-handedly. I could not have hoped for better.

I then went on to study mathematics and physics at the UWA, starting in 1958. It was a good time to study mathematics there. The mathematics department had recently hired four outstanding applied mathematicians, headed by Harry Levey from the Aeronautical Research Laboratory in Melbourne. We were treated to some excellent advanced courses in f uid dynamics, aerodynamics, and partial differential equations. We were also fortunate that a new professor of electrical engineering had just arrived from the United Kingdom. His wife, Joyce Billings, had done her Ph.D. in stellar structure with Hermann Bondi in London. She joined the mathematics department and taught theoretical astrophysics to the senior students, mainly stellar structure and radiative transfer. In physics, the highlights were a comprehensive classical optics course, which I liked very much, and the quantum theory taught by Armin Thellung, a visiting physicist from Zurich who was one of Wolfgang Pauli's last assistants.

By then, I knew that I wanted to make a career in some aspect of applied mathematics or theoretical physics, but I did not know what. During the summer vacations, several Australian scientifi institutions offered internships to science undergraduates. At the end of my second year, I worked in the aerodynamics division of the Weapons Research Establishment in South Australia testing aerofoils. Computers were starting to appear, and working in this large research institute was an exciting introduction to large-scale science. While at the Weapons Research Establishment, I happened to see an advertisement for a vacation scholar program in astronomy at Mount Stromlo Observatory in Canberra. At the time, I was not very interested in astronomy, but it seemed an opportunity to learn something new, so I wrote to Bart Bok, who had recently taken up the directorship at Mount Stromlo after many years at Harvard University. He was an enthusiastic individual and very interested in students, and he sent me back an effusive letter full of the exciting future for astronomy in Australia. So that is where I went for the next two summer vacations. In the f rst summer, I worked with Lawrence Aller, who was on sabbatical in Australia, and I worked with Bengt Westerlund during the second. I also got to know Ben Gascoigne, a distinguished optician and observer, who helped me along throughout my career. Bok suggested that I might consider doing a Ph.D. at Mount Stromlo, but the research and researchers at the time were very observational, and I really wanted to do something theoretical.

2. CAMBRIDGE AND TEXAS

During my fourth year of mathematics at UWA, George Batchelor from Cambridge University visited the mathematics department. Batchelor was an Australian who had become famous for his research on turbulence, and he was head of Cambridge's Department of Applied Mathematics and Theoretical Physics (DAMTP). He was looking for new research students for his department. The research interests at DAMTP were very broad and, at that time, included almost all of Cambridge's theoretical astrophysics. So I applied to Cambridge University and to Batchelor's college (Trinity College) and was accepted. My interests were moving toward galactic dynamics: Leon Mestel was working in that area, and he took me on as a Ph.D. student. **Figure 1** is a photograph of Leon and me about 30 years later, at my house, on the occasion of a visit to Australia (Leon was born in Melbourne and emigrated to England with his family at the age of three).

I began working on the stability of a rigidly rotating cold thin disk, but neither Leon nor I knew that in a nearby offic one of the postdocs, Chris Hunter (now a well-known dynamicist in Florida), was working on the same problem. He solved it very nicely (Hunter 1963), just a few months after I arrived in Cambridge. So I changed topics to work on gas flow in barred spiral galaxies. A year or so later, Leon went on sabbatical at Princeton University, and Donald Lynden-Bell, recently returned from a postdoc at the California Institute of Technology and the Carnegie Observatories and with an already famous paper with Olin Eggen and Allan Sandage (Eggen et al. 1962, ELS), took over my supervision. **Figure 2** is a photograph of Eggen, Lynden-Bell, and Sandage in 1986, at a conference in Cambridge at which the twenty-fift anniversary of the ELS paper was celebrated. Donald had done groundbreaking work on the foundations of stellar dynamics, and my interests moved in that direction. My subject was now the evolution of barred galaxies as they slowly lost mass and angular momentum, and Donald suggested using adiabatic invariants. They were ideally suited to the problem. In the process, I made some simple analytic self-consistent rotating stellar dynamical models of barred galaxies (Freeman 1965). These models are stellar dynamical analogs of the Jacobi flui figure of equilibrium. The integrals of motion for the stellar orbits in these models have associated adiabatic invariants, which I used to calculate how the models evolved.

By 1962, the Cambridge Computer Laboratory was already well into its second generation of computers. EDSAC 2 (Electronic Delay Storage Automatic Calculator 2) was available to all researchers. It had an easy-to-use high-level language called `Autocode` and input and output via paper tape, with very fast optical tape readers. It was a useful machine for various computational tasks. The radio astronomers used it for radio synthesis reductions. I used it to integrate stellar orbits.

I greatly enjoyed my time at Cambridge. For the firs year, I lived in college. That winter was the coldest in 200 years, and the River Cam froze over for more than two months. There was not

Figure 1

Leon Mestel and Ken Freeman, Canberra.

enough electricity in Cambridge to cope with this cold winter, so it was a spartan few months. At the beginning of my second year, I married Margaret.

The DAMTP faculty included George Batchelor, Fred Hoyle, Dennis Sciama, and Roger Tayler who gave memorable lectures and had many graduate students. There was a lot of interaction also with the postdocs, who included Peter Goldreich and Jerry Ostriker. Scientifically it was an exciting time. The Burbidge et al. (1957) paper was only a few years old, and the controversy between Fred Hoyle and Martin Ryle about the steady state theory was going strong. Paul Dirac was Lucasian Professor; his Part III course on quantum mechanics is among my best memories of Cambridge.

At the time, little was known observationally about barred galaxies, and I became interested in knowing more. In 1964, at the International Astronomical Union's General Assembly, I met with Gérard de Vaucouleurs. He was a famous galaxy observer who had worked in Australia early in his career. He was interested in barred galaxies, and we discussed the possibility of a postdoc. It was much easier to get postdoctoral appointments at that time: One or two letters was usually enough. After I finishe at Cambridge, I went to work with Gérard and Antoinette de Vaucouleurs at the University of Texas (UT) for a year, learning how to make and reduce photometric and spectroscopic observations of galaxies with the McDonald 82-inch reflector **Figure 3** is a photograph of Gérard and Antoinette in Paris, in 1962. De Vaucouleurs got me interested in the dynamics of the doubly asymmetric Magellanic barred spirals, and I worked on

Figure 2

Olin Eggen, Donald Lynden-Bell, and Allan Sandage, Cambridge 1986. Photo credit: Prof. Gerry Gilmore.

their orbit structure and stability. We wrote a review on these systems (de Vaucouleurs & Freeman 1972), and our collaboration continued into the 1990s.

The UT had an active graduate program, and I was asked to give some lectures on galactic dynamics. There was no book on galactic dynamics at the time, so I started more or less from scratch. I then went back to Cambridge for a year to take up a research fellowship at Trinity College, continued work on the Magellanic spirals, and further developed the galactic dynamics course. I recall that Martin Rees was a graduate student in the galactic dynamics class.

3. BACK IN AUSTRALIA

After Cambridge, I returned to Australia in 1967 to take up a Queen Elizabeth II Fellowship at Mount Stromlo Observatory at the Australian National University (ANU). Olin Eggen was now director, and astrophysics was more prominent than it was in Bart Bok's time. Leonard Searle

Figure 3

Gérard and Antoinette de Vaucouleurs, Paris 1962.

(who later became director at the Carnegie Observatories), Don Mathewson, Ben Gascoigne, and Alex Rodgers were influentia in definin the kind of science that was being done at ANU then.

At that time, we did not yet have electronic detectors: photomultipliers, image tubes (Ford 1968), and photographic plates were state of the art. Canberra's city lights were not yet a big problem, but Canberra was growing rapidly. Image tubes were good for measuring radial velocities of stars and rotation in galaxies, but it was difficul to make accurate quantitative measurements of spectra until photon counting systems appeared toward the end of the 1970s: These included the IPCS (image photon counting system) on the Anglo-Australian Telescope (AAT) and some homemade 1D and 2D systems built by Alex Rodgers at Stromlo. The advent of CCDs in the 1980s was a huge advance for imaging and spectroscopy. I was very pleased to see the end of the photographic era.

Allan Sandage visited the Mount Stromlo Observatory for a year's sabbatical in 1969. Allan, his wife, Mary, and their two boys lived in one of the cottages on the mountain. My wife and I and our two small children were also living in a cottage on the mountain, and we became family friends. Allan was making a redshift survey of southern galaxies with the 74-inch telescope. He was interested in determining the value of the Hubble constant; this was a focus of his scientifi career, and much of his famous work in stellar astronomy was aimed at calibration of the various distance indicators. He firml believed that H_0 was around 50 km s^{-1} Mpc^{-1}. I had been a postdoc with Gérard de Vaucouleurs. de Vaucouleurs and Sandage had some common interests, including galaxy classificatio and the value of the Hubble constant, which de Vaucouleurs f rmly believed was around 100 km s^{-1} Mpc^{-1}. Allan's tense relationship with de Vaucouleurs at this time was legendary. I was not involved in the Hubble constant debate and remained friendly with them both throughout their lives. I occasionally heard from each about the shortcomings of the other.

Sandage and I got along very well. His visit was a big event in my career. He was intensely interested in the process of galaxy formation. At Stromlo, he was completing some work on the intrinsic shapes of galaxies. He showed that the spiral and S0 galaxies were f at systems, whereas the elliptical galaxies covered a wide range of shapes. He was keen to relate this findin to broader issues in galaxy formation, and we were discussing his draft paper one night. I was young and not as respectful as I should have been, and made some brash suggestions about the broader issues of gas content and angular momentum distribution in the different kinds of galaxies. Allan was patient and generous with me: If I wrote something on these broader issues, he said, we could do a joint paper. This was exciting for me, because Allan knew so much and was so famous, and I did my best. I greatly enjoyed our interaction. We had many discussions to develop the ideas, and they turned into a paper coauthored by Norman Stokes who had done the numerical work on the intrinsic shapes (Sandage et al. 1970).

Sandage was editor of the *Stars and Stellar Systems IX* compendium on galaxies and the Universe (Sandage 1975), and he needed an article on "Stellar Dynamics and the Structure of Galaxies." He invited me to write this article (Freeman 1975). It seemed a good opportunity. I had my Texas and Cambridge lectures to start from, and Sandage and I had talked a lot about problems of galaxy formation. I think that writing this article helped me to get my ideas about disk galaxies and galaxy formation in order. This subject is still my primary interest. Sandage and I remained friends and colleagues throughout his life. He was a guide and patron and did a great deal to help my career along.

4. THE EXPONENTIAL DISK AND DARK MATTER

One of my firs projects in Australia, in 1968–69, was on the exponential disks that are commonly seen in disk galaxies. De Vaucouleurs and I had often talked about the origin of the exponential disks. This was not understood at the time; even now, nearly 50 years later, it remains uncertain, although several dynamical explanations have been proposed. From the limited available surface photometry of spirals, I was surprised to fin that the central blue surface brightnesses of the exponential disks of the brighter spirals did not vary much from galaxy to galaxy, at least for the brighter spirals. I was also interested in the form of the rotation curve for a self-gravitating exponential disk, assuming that mass follows light. It was possible to calculate this rotation curve analytically, using a technique developed by Toomre (1963), and it showed the expected Keplerian decrease beyond about two disk scale lengths. I then tried to compare the expected rotation curves with those observed. I needed HI rotation data extending out into the outer parts of spirals. Single-dish data were available for some nearby galaxies, but HI aperture synthesis was still in its early days and only a few galaxies had been observed. The comparisons did not work out very well: The observed rotation curves did not turn over as expected. If the observations were reliable, then there had to be a large mass of invisible gravitating matter in the outer parts of the galaxies. I commented on this in my paper (Freeman 1970) but then drifted on to other problems and did not return to dark matter until much later, in the context of pure disk galaxies and dwarf galaxies. I think that my comments in this paper about invisible gravitating matter were stimulated by ideas that I had heard from Mort Roberts.

Much has been written about the evolution of ideas about dark matter in individual galaxies in the period between 1970 and 1978. My own recollection is that people were generally fairly sceptical about dark matter in galaxies in the early 1970s. Although the rotation curves indicated that there was more to spiral galaxies than meets the eye, the data were not very convincing. Looking back at the dark matter saga over a longer period, several important papers were not fully appreciated at the time that they were published. Zwicky's (1933) now-famous paper on dark matter in the Coma cluster did not excite much attention. The Kahn & Woltjer (1959) paper on the timing mass for M31 and the Milky Way seems to me to be a convincing early detection of dark matter in individual galaxies, but again it did not excite much interest at the time. Some hints of high mass-to-light ratios in the outer regions of the S0 galaxy NGC 3115 were mentioned at the Santa Barbara conference on extragalactic research in 1962 (Oort 1962). But by the early 1970s, some people were taking dark matter in galaxies seriously, perhaps because a theoretical framework had evolved around the role of dark matter halos in the stability of disks (e.g., Ostriker & Peebles 1973). I think that Bosma's (1978) high-quality Westerbork data on the HI rotation curves of nearby galaxies were very important in convincing people about dark matter in galaxies. The Annual Reviews article by Faber & Gallagher (1979) drew together the loose threads at the time and I think convinced many of the sceptics. Ideas about dark matter and galaxy assembly (e.g., White & Rees 1978, Fall & Efstathiou 1980) and the emergence of the cold dark matter paradigm in 1982 were all influentia in making dark matter in galaxies an acceptable part of the landscape.

I worked again on dark matter in the 1980s with a student, Claude Carignan, who made HI single-disk observations of some nearby disk galaxies and Very Large Array observations of dwarf irregular galaxies. We noted the high dark matter densities of these dwarf irregular galaxies (Carignan & Freeman 1988, Carignan et al. 1990). John Kormendy (1987) had also discussed the high dark matter density in dwarf galaxies, and he and I have worked together for many years on the scaling relations for dark halos of spiral and dwarf galaxies (how the characteristic densities and core radii of the dark halos scale with the luminosity of the visible galaxies). In some of the faintest dwarfs, the baryon fraction is very small and the optical M/L ratios can be in excess of 1,000. It seems likely that some of the dwarf galaxies are entirely dark. Our most recent paper on the dark matter scaling relations (Kormendy & Freeman 2016) confirm that the halo densities increase by about three orders of magnitude from large spirals like the Milky Way to the faintest of the dwarfs.

The faint dwarf spheroidal galaxies have long been regarded in terms of their light distributions; their surface brightnesses are very low, and they look quite fragile, as if they would easily be destroyed tidally if they were accreted by the Milky Way. I think that this impression is misleading. The faint dwarfs have high dark matter densities, about 10 times higher than the stellar density of the Galactic disk near the Sun. Unless the dwarfs migrate right into the inner Galaxy, they are probably robust enough to survive an accretion event, despite their flims appearance.

5. GLOBULAR CLUSTERS

By the early 1970s, I had become interested in theoretical models and observations of globular clusters. Ivan King's work had a significan influenc on me. His combination of observational and theoretical skills appealed to me, and I think I modeled my own scientifi growth at that time on his example. His truncated globular cluster models (King 1966) are based on a simple distribution function and, although they are collisionless and do not include a stellar-mass function, they are a fair representation of the cluster structure and kinematics. I was interested to use the King models to estimate the masses of globular clusters. This required measurement of the central velocity dispersion and observation of the radial surface brightness distribution.

Measuring the velocity dispersion by conventional methods was a large project, even for a single cluster: Precise measurements of the velocities of many individual cluster stars were needed, and there were not much data of this kind available. Would it be possible to measure the velocity dispersions more quickly using integrated light techniques, as was done for elliptical galaxies? The velocity dispersions of globular clusters are typically 5 to 10 km s^{-1}, so high-resolution spectra of the integrated light would be needed. Our coudé spectrograph on the Mount Stromlo 74-inch telescope had sufficien resolution, but would the sensitivity be suff cient to acquire the spectra? A student named Garth Illingworth and I did some tests with the spectrograph that convinced us that the observations were possible. We developed some simple Fourier techniques, building on earlier work by Simkin (1974), for measuring the velocity dispersions from the integrated spectra of the cluster core. This all worked out very well, and the data for several clusters were the basis of Garth's thesis (Illingworth 1976).

At this time, globular clusters were believed to be chemically homogeneous, but hints were emerging that this might not be true. Omega Centauri, the most luminous globular cluster in the Galaxy, seemed a likely candidate for chemical inhomogeneity. Whereas most clusters had narrow giant branches in the color-magnitude plane, its giant branch was broad, and star-to-star metallicity variations were a likely cause. My colleague Alex Rodgers was an expert on measuring abundances in RR Lyrae stars, and we decided to measure Ca abundances for a sample of Omega Cen's RR Lyr stars. It turned out that the Ca abundances of these stars were far from homogeneous: They showed a spread in [Ca/H] of more than 1 dex (Freeman & Rodgers 1975). I think this was the f rst clear spectroscopic indication of chemical inhomogeneity in a globular cluster, and it was the f rst example of the relatively rare clusters that are inhomogeneous in heavy elements. With my colleague John Norris and others, I was also involved in studies of the CN variations from star to star in some of the nearby clusters. We know now that the light element inhomogeneities in globular clusters, involving elements from C up to about Mg, are common.

The idea had appeared in the literature that globular clusters may be the nuclei of accreted dwarf galaxies. I was working at the time on nucleated dwarf galaxies and studied this association. It was consistent with what we knew about nucleated dwarfs and provided a way to generate globular clusters that are inhomogeneous in heavy elements. It made sense within the new framework of cluster formation in dwarf satellites proposed by Searle & Zinn (1978). Some of the most chemically inhomogeneous clusters are now believed to be the nuclei of dwarf galaxies, which had an extended period of chemical evolution before the dwarfs were accreted and tidally disrupted by our Galaxy. Only the dense nucleus survives, and it would look like a globular cluster. I talked about this scenario at a Santa Cruz conference (Freeman 1993); I think it is my most highly cited conference paper.

Globular cluster formation is still a fascinating mystery. These dense systems are believed to have formed in a high-pressure environment. Their free-fall times are short, similar to the evolution time for a single generation of massive stars, and it is not clear how there was time for them to acquire even the light element inhomogeneities that are commonly observed. Zinn (1985) showed that the Milky Way globular clusters come in two populations: those with $[Fe/H] < -0.8$ belong to the slowly rotating halo and those with $[Fe/H] > -0.8$ lie in a rotating disk. The nature of the metal-rich population is still not certain. They were identifie for many years with the Galactic thick disk (Armandroff 1989) but later became associated with the Galactic bulge. I suspect that the thick disk association may turn out to be correct.

For some reason, the globular clusters in our Galaxy are all old, but we do see very young globular-like clusters forming now in the Magellanic Clouds (e.g., NGC 2070) and in some late-type spirals and in some star-bursting dwarf galaxies like NGC 1705. It may have to do with the high local star-formation rates that occur in these galaxies.

I started working on the structure of young globular clusters in the Large Magellanic Cloud (LMC), using deep photographic images from the AAT shortly after it was commissioned in 1974. Because these clusters are so young (only a few $\times 10^7$ years), the stellar-mass function covers a much wider range of mass than it does in old globular clusters. Their relaxation times are short, and I was interested to see if one could detect mass segregation. That project did not work out, but Becky Elson, Mike Fall, and I (Elson et al. 1987) later showed that these young clusters appear to have a power-law radial surface density distribution rather than the tidally truncated King model distributions that we see in older clusters. Globular clusters are apparently born with power-law mass distribution, and it takes some time for them to become tidally limited by their parent galaxies.

6. THE YALE CONFERENCE

This story starts in 1966, when I was a postdoc and Beatrice Tinsley was a graduate student at the UT in Austin. Beatrice and I had common interests, and we had many discussions. We lost contact after I left the UT, and it took 10 years before we met up again. By that time, Beatrice was at Yale University and was doing some innovative work on stellar populations. Unfortunately she became ill around that time and died in 1981, so our second overlap did not last very long. We met several times at conferences, and were planning to write a book together with a few other people, though this did not eventuate. She decided to organize a conference on The Evolution of Galaxies and Stellar Populations at Yale in 1977, and that did happen. Many new ideas were becoming ripe around this time that would transform the subject. It was a truly memorable meeting. People realized how important galactic mergers, interactions, and accretions were, not just for the tidal interactions of pairs of galaxies, which people had been studying for several years, but because these merger events were basic to the whole genesis and evolution of galaxies.

There were two major related ideas. The firs was the new Searle & Zinn (1978) idea that the metal-poor stellar halo of our Galaxy was not really part of a basic monolithic formation process of the Galaxy, as Eggen et al. (1962) had argued. Rather, the stellar halo is the debris of a large number of small (and therefore metal-poor) galaxies that had been accreted by our Galaxy and tidally disrupted, after a short period of chemical evolution that was essentially independent of the evolution of the Galaxy itself. The second came from the Toomre brothers, who had already published convincing restricted three-body models of interacting disk galaxies. They took this story further and considered the likely outcome of such interactions and mergers as elliptical galaxies. This idea, though still contentious in some aspects, became part of the knowledge base of the subject. The important concept that was emerging during the 1970s and I believe crystallized at the Yale meeting was the idea that galaxies are built up through a hierarchy of mergers, including major mergers and minor accretion events. The Press–Schecter concepts were already out there. Dark matter was becoming seriously discussed as part of the galaxy-formation scenario, though I think it was not yet an entirely accepted part of the paradigm. Looking back, it is interesting how the theoretical and observational ideas that led to our present concepts about hierarchical galaxy formation had been proceeding rather independently through the 1970s and only really came together around the time of the Yale conference.

My talk at the Yale conference was on two topics. One topic was the evidence that the stellar initial mass function in star clusters and galaxies was not everywhere the same, and the other topic was the reason for the very low gas content in S0 galaxies. These questions are still interesting and contentious 40 years later.

After the Yale meeting a few of us, rather exhausted, were having a drink, and we all realized that the subject had changed at this conference. This does not happen very often at conferences, but it happened at an earlier meeting on stellar populations that was held in 1957. A select group, including Walter Baade and some of the other great names of his generation, met at the Vatican to digest the ideas that Baade had introduced during wartime about stellar populations. Sandage was there, and I remember him saying how transformative that meeting was. It crystallized the ideas about stellar populations that we are still using today.

7. THE MACHO PROJECT

Bohdan Paczynski (1986) wrote a paper on how to use microlensing of stars in the Magellanic Clouds to detect compact dark objects in the Galactic dark halo. The optical depth to microlensing by massive compact halo objects (MACHOs) is small, and an observational project based on this idea would require many millions of stellar photometric measurements in at least two wavelength bands over several years. I was visiting Princeton University around that time and talked with Paczynski, who was interested in ways in which one might make such observations.

Charles Alcock at the Lawrence Livermore National Laboratory was considering an ambitious project using CCDs to do the kind of project that Paczynski had envisioned, and he was assembling a consortium. Whereas large CCD arrays for wide-fiel imaging are common now, at the time it was not clear that they were feasible. Christopher Stubbs, an experimental physicist on Alcock's team, was confiden that he could build a 32-megapixel array, and he was right. I got involved, with several others from Mount Stromlo, because Alcock's team was in need of a telescope. Mount Stromlo had an old 1.2-m telescope, the Great Melbourne Telescope built in 1868, which was in a state of disrepair. A collaboration followed. The telescope was rebuilt and roboticized at Mount Stromlo, Stubbs built a two-channel × 4-CCD imager for simultaneous red and blue imaging at the prime focus of the 1.2-m telescope, and other members of the collaboration managed the computer hardware and wrote the photometric software and data management software. The project ran through most of the 1990s, observing Magellanic Cloud stars in the summer and bulge stars in the winter. The f rst microlensing event was detected in 1993 but, by the end of the observations, it was fairly clear that the dark halo was unlikely to be made of compact objects in the mass range from about 10^{-7} $M_{\odot}$ to about 30 $M_{\odot}$.

When we started the MACHO project, estimates of the contribution to the cosmic density from dark halos were similar to the baryonic contribution allowed by Big Bang nucleosynthesis (BBNS), so many of us were expecting a baryonic outcome. By the end of the 1990s, estimates of the cosmic density of dark halos had drifted well above the BBNS limit, and the microlensing data had independently excluded most of the baryonic dark matter candidates. I think the MACHO experiment reinforced the view that the dark matter was likely to be some kind of subatomic particle. It was also important in other ways, as a demonstrator of multi-CCD arrays in astronomy and as one of the f rst large-scale automated optical survey projects.

8. DISK DYNAMICS

Piet van der Kruit came to Mount Stromlo for a sabbatical in 1984, and we made some of the firs measurements of the vertical velocity dispersion of the disks of spiral galaxies using the spectra of the integrated light of the disks. The goal was to see whether the velocity dispersions decreased with radius in the way that we would expect. We observed two near-face-on galaxies with the 74-inch telescope and a Boller & Chivens spectrograph with a 2D photon counting array; this detector was good for these low surface brightness observations because the read-out noise was negligible. The observations took several nights, but we were able to measure the velocity dispersion of the disks, and we later showed that the dispersions decrease with radius more or less as expected from simple models of exponential disks with radially constant scale heights (van der Kruit & Freeman 1984).

Studies of the velocity dispersions of disks have continued to the present time, by van der Kruit and his students (Roelof Bottema, Michiel Kregel) and other groups. This takes us back to the studies of dark halos. An important goal of the velocity dispersion observations is to measure the surface density of the stellar disks in order to decompose the rotation curves into contributions from the stellar disk and the dark halo. All of these studies have concluded that the stellar disks are submaximal, i.e., that their contribution to the rotation curve is everywhere significantl less than the contribution of the dark halo. This has implications for the scaling laws for dark halos mentioned in Section 4. I am working on this question again now, with my student Suryashree Aniyan and several other colleagues (Aniyan et al. 2016). We argue that the surface density of stellar disks has been underestimated because of the presence of kinematically colder young stellar population stars in star-forming spiral galaxies. When this colder population is taken into account, the stellar disks are in fact close to maximal.

Observations of velocity dispersions in disks using integrated light spectra become too difficul at radii beyond a few scale lengths. In our Galaxy, where accurate velocities for individual disk stars can be determined, we can extend the observations to larger radii. Jim Lewis and I measured the radial component of the velocity dispersion in our Galaxy, star by star, and were able to cover the whole radial interval from near the bulge out to about 15 kpc from the Galactic Center. The velocity dispersion is close to exponential. In the inner parts of the disk, it is as high as 100 km s^{-1}, and falls to about 15 km s^{-1} toward the anticenter (Lewis & Freeman 1989).

9. EXTRAGALACTIC PLANETARY NEBULAE

For many years, I have collaborated in the use of extragalactic planetary nebulae (PNe) as dynamical probes in galaxies. They are very effective for measuring the internal kinematics of early type galaxies out to many effective radii. When lower-mass stars (<8 $M_{\odot}$) are going through the PN phase of their late evolution, about 15% of the light of the stellar core is reradiated in the [OIII] λ5007-Å line. This makes it possible to detect individual PNe in galaxies and measure their radial velocities out to distances of up to 100 Mpc from the Sun (e.g., Gerhard et al. 2007). I got involved in this work around 1988 in collaboration with Holland Ford, Hui Xiaohui, and (later) Eric Peng using the AAT to observe PNe in Cen A. The program continues to the present time, in collaboration with Magda Arnaboldi (European Southern Observatory) and Ortwin Gerhard (Max Planck Institute for Extraterrestrial Physics) and others, mainly on elliptical and S0 galaxies. For the firs several years, we used narrow-band imaging to detect the PNe and then multiobject spectroscopy to measure their velocities. This two-step process is still needed for PNe in the more distant galaxies, but it involves two lots of telescope time proposals, and several of us were thinking about a counter-dispersed imaging spectrograph that could do both of these steps in a single observation. This spectrograph would have a narrow-band filte at the [OIII] λ5007-Å line, and two gratings and cameras, each of which receives half of the pupil. The gratings disperse the light in opposite directions. When the two dispersed images are combined, PNe appear as a pair of unresolved [OIII] dots, separated by a distance that depends on their redshift. Around 2000, we built this instrument (the Planetary Nebula Spectrograph) in collaboration between the ANU, the ESO, the Kapteyn Institute, the Naples Observatory, and the University of Nottingham. Nigel Douglas at the Kapteyn Institute led the design team, and the instrument was built at Mount Stromlo Observatory and commissioned on the William Herschel Telescope in 2001. We have used it ever since for measuring the kinematics and dark matter content of early-type galaxies (e.g., Coccato et al. 2009) and, more recently, for late-type spirals (Aniyan et al. 2017). The precision of the radial velocities is now between about 5 and 10 km s^{-1}, which allows us to use PNe to measure the vertical velocity dispersions in the disks of nearby spirals, as discussed in Section 8.

10. GALACTIC ARCHAEOLOGY

The Eggen et al. (1962) paper on the formation of the Galaxy appeared early in my career. I liked this kind of scenario building, motivated by dynamical and chemical constraints. I was intrigued by the relationship between the slow-rotating metal-poor stellar halo of the Galaxy and the fast-rotating metal-rich disk that is so much more massive: How could the low-mass low–angular momentum halo significantl affect the chemical properties of the massive disk? Then came the Searle & Zinn (1978) paper with its very different perspective on the role of the halo in

Galaxy formation. Then the thick disk was discovered (Gilmore & Reid 1983) with its metal-weak extension (Bessell et al. 1985). How did this all f t together?

Joss Bland-Hawthorn and I began talking about Galactic archaeology at a conference in Princeton in the late 1980s. What could we learn about galaxy formation from detailed observations of our own Galaxy (near-fiel cosmology)? What kind of information is still accessible, and what had been lost forever in the dissipative processes that occurred during disk formation? We wrote a review for *Science* entitled "The Baryon Halo of the Milky Way: A Fossil Record of Its Formation" (Bland-Hawthorn & Freeman 2000), and then we were invited to write an Annual Reviews article on "The New Galaxy." We were uncertain about what was intended, but we took the opportunity and wrote an article entitled "The New Galaxy: Signatures of its Formation" (Freeman & Bland-Hawthorn 2002). One of our themes was the loss of dynamical information during dissipation and how one might at least partially compensate using chemical data. We introduced some ideas about multidimensional chemical space, chemical tagging, and instrumentation for large-scale high-resolution spectroscopic surveys to measure abundances of many elements in about a million stars. We proposed a notional instrument for doing such a survey, which became a real instrument called HERMES (High Efficienc and Resolution Multi-Element Spectrograph), with which we are now doing such a survey (the GALAH survey) on the AAT. Other such surveys are in progress or in planning.

In the late 1990s, several German and Danish institutes proposed *Deutsches Interferometer für Vielkanalphotometrie und Astrometrie* (DIVA), a small pre-*Gaia* astrometric satellite mission. It would reach stars somewhat fainter than those seen by *Hipparcos*, and the parallax and proper motion precision would be somewhat better. This was an exciting prospect for Galactic science, and it seemed important to fin a way around the lack of radial velocity data, which was an issue with the *Hipparcos* mission. At a conference in Heidelberg, I met with some of the DIVA proponents and proposed that we might consider measuring radial velocities for the southern stars with the UK Schmidt multi-object fibe spectrograph. In the end, the DIVA project was not funded, but the radial velocity project took on a life of its own. It became the RAVE (Radial Velocity Experiment) project, run from the Astrophysical Institute, Potsdam, with Matthias Steinmetz as principal investigator, and involved collaborators from 10 countries. RAVE acquired medium-resolution spectra for about half a million stars between 2003 and 2012. Although the original goal was to measure radial velocities, the RAVE spectra are also useful for measuring stellar metallicities, and RAVE became one of the firs of the Galactic archaeology stellar surveys, reaching out to distances of about 1 kpc from the Sun.

Now we have several large stellar surveys, past and ongoing, at low resolution (SDSS, SEGUE, LAMOST) and at higher spectral resolution (Gaia-ESO, APOGEE, GALAH) and further surveys soon to start (WEAVE and 4MOST). In the next few years, we can expect precise chemical data for several million stars. The vast *Gaia* astrometric mission has already made its f rst data release for relatively bright stars that had been previously observed in the Tycho mission. Subsequent *Gaia* data releases, expected to start in 2017, will give high-precision astrometry for millions of fainter stars extending for many kiloparsecs from the Sun. We can expect many new insights into the dynamics and content of the Milky Way that are currently out of reach. The dark matter content of the inner and outer Galactic disk is just one example.

11. GALAXY FORMATION

What are the essential elements of disk galaxy formation? Disk galaxies have some identifiabl components: the thin disk, the thick disk, the stellar halo, and sometimes a bulge. Which of those are essential? The thin disks are the definin characteristic of disk galaxies. Thick disks are almost ubiquitous; they appear to be an unavoidable part of the formation of disk galaxies. The stellar halo also seems to be an essential feature: A halo is detected in every disk galaxy in which a halo could have been detected. For a long time, bulges were thought to be an essential property of disk galaxies, but this is not so. Over the past 15 or so years, we have learned that some very substantial galaxies, as large as the Milky Way, do not have a significan bulge component (e.g., Kormendy et al. 2010) and, even if they do, their bulges may form via instabilities of the disk and have nothing to do with the usual merger process that we have come to associate with bulges.

Martin Bureau and I (Bureau & Freeman 1999) were interested in the boxy/peanut (B/P)-shaped bulges that are seen in many late-type disk galaxies, including the Milky Way. Numerical simulations had long indicated that B/P bulges are galactic bars, seen edge-on. In the simulations, the bars form through instability of the disk and then buckle vertically to build the B/P structure. We wanted to fin direct observational evidence that B/P bulges were bars seen edge-on. We followed up on an idea by Kuijken & Merrifiel (1995) that the presence of a bar in an edge-on disk galaxy could be detected from the characteristic double-valued pattern of gas f ows within the bar. This worked very well; we were able to show that most B/P bulges are indeed edge-on bars.

More recently, the ARGOS (Abundances and Radial Velocity Galactic Origins Survey) team made a large spectroscopic survey of red giant stars in the B/P bulge of the Milky Way (Freeman et al. 2013) using the AAT. We found that the metallicity distribution function of the bulge stars shows multiple components that we can associate with trapped stars from the thin and thick disk at the time of the disk instability (Ness et al. 2013). I think most people in this fiel would now believe that bulge formation via the classical merger scenario is not an essential part of the assembly of a disk galaxy.

Returning to the broader question of disk galaxy formation, the stellar halo appears to be a minor component and is believed to be the debris of small accreted galaxies, as suggested by Searle & Zinn (1978). The association of B/P bulge formation with disk instability means that understanding the baryon assembly is mainly about understanding how the thin and thick disks are assembled, at least for the late-type spirals. The Milky Way remains a vital part of this process, because so much detail is accessible. The firs step is to build up an observational picture of the current state of the Galaxy. I think this is not far off. With the *Gaia* astrometric data and the spectroscopic data from the high-resolution spectroscopic surveys, the observational goal is to generate a distribution function for Galactic stars over a space that includes stellar age and the six dimensions of phase space (position and velocity), plus several further dimensions in a chemical space based on abundances of up to about 30 elements. Our knowledge about the present state of the Galaxy will be summarized in this multidimensional distribution function.

The next step will be to understand how it got into this state. The narrative about what happened when in the assembly of disk galaxies will be based on what we know about our own Galaxy and what is learned directly from galaxies that are seen at appropriate redshifts. For example, the thick disk of our Galaxy is believed to have formed about 11 to 12 Gyr ago, corresponding to redshifts between about 2 and 3.5.

The number of independent dimensions of the chemical space is believed to be about nine. We do not know yet how many of these chemical dimensions will turn out to be useful for understanding the events that took place between the start of star formation in our Galaxy and the present time, in which most of the visible baryons are lying in the stellar disk. I think that at least four will be useful. The [Fe/H] abundance and the alpha element (Mg, Si, Ca, Ti) abundances [α/Fe] are determined by different kinds of supernovae with different timescales and already provide indicators of the duration and nature of the different chemical enrichment routes that produced the thin and thick disks of the Galaxy. We may get some further constraints on the assembly narrative from the chemical dimensions involving light (Sr, Y) and heavy s-process (Ba, La) elements. These elements come from asymptotic giant branch stars of different masses, so they potentially contain time information additional to that given by the [α/Fe]−[Fe/H] distribution.

12. CONCLUSION

A lot has changed in astronomy over the past 50 years. The changes that have most affected my work include:

- New technology: CCD detectors and CCD arrays, HST, large optical telescopes, "multi-object" f ber spectrographs, integral f eld unit spectrographs, radio synthesis telescopes, and databases like the ADS, NED, SIMBAD, and VizieR.
- The discovery of dark matter in galaxies and its implications for galaxy formation.
- The discovery of thick disks and their α-element enhancement.
- The 2003 bushfir that destroyed the telescopes on Mount Stromlo.
- The opportunities afforded by large team surveys and the computer power to handle the data.

DISCLOSURE STATEMENT

The author is not aware of any affiliations memberships, funding, or financia holdings that might be perceived as affecting the objectivity of this review.

ACKNOWLEDGMENTS

I am indebted to Leon Mestel, Donald Lynden-Bell and Gérard de Vaucouleurs for their guidance and continued support. I am very grateful to Ben Gascoigne, Olin Eggen, Don Mathewson, Alex Rodgers and later directors at Mount Stromlo, and to Allan Sandage, Ivan King, John Bahcall, Riccardo Giacconi, Tim de Zeeuw and Brian Schmidt, all of whom helped me along in different ways. My thanks go to my wife and family for their support, and to my students and collaborators, and particularly (in temporal order) to John Norris, Piet van der Kruit, Michael Fall, Joss Bland-Hawthorn, John Kormendy, Holland Ford, Magda Arnaboldi, Ortwin Gerhard and David Block for long and continuing collaborations.

In 1976, I worked at the Kapteyn Institute in Groningen for a year, to fin out more about radio synthesis. Between 1984 and 2003, I visited the Institute of Advanced Studies in Princeton or the Space Telescope Science Institute in Baltimore for about a month each year, and more recently have visited ESO or MPE in Garching for about a month in most years. These visits have all been exciting and productive, and I am very grateful to the institutes and colleagues for their hospitality and support.

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