8. Distance Scale

Walter Baade’s most famous result, to the readers of the popular scientific magazines and newspaper articles of his time, was “doubling the size of the universe”. It was especially newsworthy because in the 1940s and 1950s the age of the Earth, based on geological evidence and radioactive dating, was believed to be 3 to 3.5 billion years, while the age of the universe derived from the velocity–distance relation (the “Hubble constant” of today) was thought to be only 2 billion years. Doubling the size of the universe brought the two ages into much closer agreement. Probably in absolute terms Baade’s new distance scale was not as intrinsically important as his population concept, which opened the way to the study of stellar and galactic evolution, as his far-ranging work on supernovae, or as his leading the way in the identification of the radio sources, but “the universe” is always newsworthy, and doubling its size is a catchy thought. To him it was just part of his long-term project of trying to understand everything in the universe in quantitative terms, which included getting the numbers right.

By 1947 Baade realized from his observational data that there was something wrong with the period–luminosity relation which was used to determine the absolute magnitudes of Cepheid variables from their periods. He had found that the “W Virginis Cepheids”, those with periods between 14 and 19 days and whose light curves had different forms from the “classical Cepheids” like δ Cephei and η Aquilae, were members of population II. The W Vir variables were all either high-velocity stars, located at high galactic latitudes, or members of globular clusters. Yet they were the link between the RR Lyrae variables, the “cluster-type variables” with periods less than a day, which were so numerous in globular clusters, and the classical Cepheids which until then had always been considered to lie on the same period–luminosity relation with the W Vir Cepheids. Baade realized that since the W Vir and classical Cepheids belonged to two different populations, there was no good reason to suppose that they were related to each other at all, or fell on a common period–luminosity relation. The measured distances to galaxies depended on the absolute magnitudes of classical Cepheids within them, while the measured distances within our Galaxy, especially to globular clusters and the galactic centre, depended on the absolute magnitudes of the RR Lyrae variables. Baade was
suspicious because the globular clusters in M 31 (whose distance depended upon classical Cepheids) as a group seemed to be considerably fainter (in absolute magnitude) than the globular clusters in our Galaxy. The period–luminosity relation had originally been determined by Henrietta Leavitt decades earlier, in terms of apparent magnitudes of classical Cepheids in one galaxy, the Large Magellanic Cloud (LMC), and it had been confirmed, extended and strengthened by later measurements in both Clouds and in M 31, M 33, NGC 6822, and IC 1613, by Edwin Hubble and by Baade. In each galaxy the apparent magnitudes were measured; they were then shifted in magnitude together and did all fall on a common period–luminosity relation, determining the relative distances of the various galaxies. The absolute values of the distances depended on the “zero-point” adopted; in other words, on the distance of one galaxy or of one Cepheid variable. Baade’s careful analysis revealed to him that the zero-point then in use came largely from assuming the RR Lyrae variables and W Vir variables lay on that same relation.

He found that the discrepancy between the absolute magnitudes of the globular clusters could be removed if the zero-point of the period–luminosity relation were shifted by 0.6 or 0.7 magnitudes; he did not regard this as proof, because there was no reason to believe the globular clusters in the two galaxies should have the same absolute magnitude. However, it was suggestive, and various other, more indirect comparisons he could make tended to agree with that figure. In his 100-inch telescope survey, Baade had found four W Vir variables in the central, population II bulge of M 31, and tried to compare them directly with the classical Cepheids in it, but the region was too crowded for accurate photographic photometry. He planned to return to the problem as soon as the 200-inch was ready for operation.80

In the meantime, Baade wanted desperately to go to the Southern Hemisphere, to search for RR Lyrae variables in the Magellanic Clouds, and compare them directly with the classical Cepheids in those same, closest galaxies. The Harvard College Observatory experts had not found a single RR Lyrae variable in the Clouds; that was an interesting clue, Baade knew, but perhaps they had not searched hard enough.81 He thought he had a chance to do so himself with the 60-inch reflector of the new Bosque Alegre station of Cordoba Observatory in Argentina. Its director, Enrique Gavio, was an astrophysicist who had studied and gained research experience in Germany and the United States, and had worked for a time on the 200-inch project as an optical physicist. When Bosque Alegre was dedicated in 1942, he had invited Walter S. Adams, the Mount Wilson Observatory director, to come to Argentina to take part in the ceremonies, or at least to send a paper. Adams had sent the paper, clearly based almost entirely on discussions with Baade, outlining possible observing programs that would take full advantage of the telescope’s location in the Southern Hemisphere. They were studies of the Magellanic Clouds, of the bright, relatively nearby globular cluster ω Cen, and of the centre of our Galaxy. The most important program, Baade declared through Adams, would be to find RR Lyrae variables and long-period variables, both supposed to have absolute magnitudes $M_{pg} = 0$, and thus
well within reach of the Bosque Alegre 60-inch, which should go as faint as +2.5 at the distance of the Clouds. Actually that was a very optimistic estimate, based on assuming near-perfect conditions of the telescope, sky and observer. Gaviola not only translated and published the paper, but wrote Adams to say that he would put some of his staff members to work on the program, and asked for data on Baade’s magnitude sequence in SA 57, to use in this study.82

However, the Argentinean astronomers were inexperienced, their telescope was new, and it was almost impossible to buy fast photographic plates during the Second World War; as a result, there was no progress. With peace restored, in 1946 Gaviola, at the suggestion of Baade, invited Ira S. Bowen, now director in Pasadena, to send “an astronomer” to Bosque Alegre, to use the 60-inch, and train the Argentineans “in the study of variables in the Magellanic Clouds”. Clearly he had Baade in mind, and the German astronomer jumped at the chance. He was prepared to go, and work with Martin Dartayet, the Argentinian astronomer Gaviola had assigned to this project. However, Baade’s German citizenship, which had kept him out of the American weapons development programs and thus allowed him to use the Mount Wilson 100-inch telescope so effectively during the war, now kept him from using the Bosque Alegre 60-inch after the war. The United States still had not signed a peace treaty with Germany, and as a result Baade’s passport was not valid. He could remain in the country, but if he left for Argentina or anywhere else, he could not legally return. Hence the idea of his working visit to the Southern Hemisphere fell through and although Dartayet continued observing the Magellanic Clouds, he did not succeed in photographing RR Lyrae variables in them.83

The only other possibility was Harvard’s 60-inch in South Africa. Baade pressed Harlow Shapley to have his observers there mount an all-out search for RR Lyrae variables in the LMC, but the Harvard director could not get enthusiastic about it. He did not believe that there was any problem. When he did write Baade in 1949 to say that his assistants could not find any RR Lyrae variables, even on the longest exposures, Baade knew that statement was highly significant. At that time he thought the LMC might be a pure population I system, without any population II, because no “real” globular clusters (in his terms) had been discovered in it. That would agree with the absence of RR Lyrae variables, but so would a fainter absolute magnitude for them; he was uncertain which explanation was correct.84

At Mount Wilson Alfred H. Joy had begun taking spectra of the W Vir variables in our Galaxy, and was finding them different from the spectra of classical Cepheids. This cast considerable doubt on the idea that all these stars could form a one-parameter sequence with a common period–luminosity relation. Shapley would not hear of it; thirty years earlier he had been the great pioneer of pulsating-variable research, but he had not kept up to date and could not bear to think that any of his results needed revision in the light of more recent data. He argued rhetorically with Joy, insisting that W Vir variables were Cepheids and that therefore they all fit on one common period–luminosity relation. Likewise, after Baade called into question...
Fig. 4. The staff of the Mount Wilson and Palomar Observatories, 1955, taken in the library of the Observator offices in Pasadena, with a portrait of the Founder, George Ellery Hale, looking down in approval. Seated (left to right): William A. Baum, Fritz Zwicky, Milton L. Humason, Irwin S. Bowen, Jesse L. Greenstein, Walter Baade; standing (left to right): Armin J. Deutsch, Robert S. Richardson, Donald E. Osterbrock, Oliphant C. Wilson. Courtesy of the Observatories of the Carnegie Institution of Washington.
the idea that the RR Lyrae variables were on the same period–luminosity relation, at a symposium on the structure of the Galaxy at Ann Arbor in 1950, Shapley sent him a plot of his data, which he believed ruled out this idea.\textsuperscript{85}

By then the 200-inch was completed and tuned up, and Baade was obtaining direct photographs of M 31 with it every night he could. As the giant spiral was well placed for observing from Palomar, he could take several exposures each night, to search for RR Lyrae variables, but he did not find any. Baade knew M 31 contained many population II objects; the RR Lyrae variables had to be too faint, not absent. At the then adopted distance of M 31, they should have been well above the faint magnitude limit of the 200-inch, and hence observable. Clearly it was more distant than earlier believed; the classical Cepheids were more luminous than previously assumed and the zero-point of the period–luminosity relation was wrong. Baade had hoped to measure the distance of M 31 from the W Vir variables in its central bulge, but found that even with the 200-inch the field was still much too crowded with stars for accurate photometry. However, by now Allan Sandage, using plates Baade had taken with the 200-inch, had measured the colour–magnitude diagram of the globular cluster all the way down to the main sequence. He found it fitted well with the colour–magnitude diagram of the nearby stars, with known distances. Hence Baade had an absolute-magnitude calibration for population II, which he assumed held for all globular clusters and population II everywhere, including in the central region of M 31, in the outer parts of it, and in its dwarf elliptical companions. This assumption seemed valid as the colour indices and apparent magnitudes of the brightest population II stars in these same regions were the same, and hence their absolute magnitudes were also. Baade’s figures gave $M_{PG} = -1.5$ for the brightest population II stars, and as he had measured their apparent magnitudes in M 31 as $m_{PG} = 22.4$, its distance modulus was $m - M = 23.9$, not the formerly accepted 22.4. The difference, 1.5 magnitudes, corresponds fairly closely to a factor of two in distance. Henrietta H. Swope was measuring the plates, and all the numbers would be refined slightly, but it was clear to Baade that the previous distance scale was wrong. Furthermore, there was an independent confirmation. At Lick Observatory, Joel Stebbins, now retired but still active, and his former student Gerald E. Kron had carefully measured photoelectrically the colours of the classical Cepheid $\eta$ Aql, and using its well-determined radial-velocity curve, had applied the Baade-Wesselink method to measure its radius, and hence luminosity, as a function of phase. This was an absolute measurement of the luminosity of the star, independent of any distance scale, and it agreed with Baade’s new zero-point for the period–luminosity curve, not the old one.\textsuperscript{86}

That summer of 1952 Baade took part in the International Astronomical Union General Assembly in Rome, the first IAU meeting he had been able to attend since the war. He reported his new results on the distance scale, which he had not published, at a meeting of the commission on “extragalactic nebulae”, of which he was acting chairman, filling in for Hubble. When Baade finished his report Andrew D.
Thackeray rose and announced that he and A. J. Wesselink had succeeded in finding a few RR Lyrae variables in NGC 121, one of the clusters in the SMC! They had observed with the 74-inch Radcliffe telescope, moved from England to South Africa after the Second World War, and with it they found the RR Lyrae variables not at $m_v = 17.5$, as the old distance scale had predicted, but at $m_v = 19$. Thus they had confirmed Baade’s revision of the distance scale. His presentation had been convincing in itself, and this additional direct evidence from one of the Magellanic Clouds sealed the case, as far as most of those present were concerned. However, Shapley, who was there, remained a sceptic. He asked for more details, which Baade provided at length. At that same Rome General Assembly, Baade also gave a long lecture on “basic facts of stellar evolution”, in which he described fully Halton Arp, William A. Baum, and Sandage’s measurements of the colour–magnitude diagrams of M 3 and M 92 as samples of the “old” population II, the fitting procedures Sandage had used on M 3, and the resulting absolute magnitudes of the population II stars. Baade contrasted them with young population I O and B stars; he now fully understood and accepted stellar evolution as responsible for the difference between the two populations.

Back in California that autumn, Baade continued photographing three fields in M 31, plus two more centred on its companions, NGC 185 and NGC 205, and Swope tried to keep up with measuring the variable stars he had found on them. Although Baade had not yet published anything about the revised distance scale, most active research astronomers who were working on galaxies were well aware of it.

Then suddenly a bombshell burst in Baade’s world. On 5 January 1953 the New York Times published an article reporting that Shapley, using data obtained at Harvard’s observatory in South Africa, had doubled the scale of the universe. The story was quickly picked up by newspapers everywhere, and by Time and other mass-circulation magazines. It did not mention Baade, and Shapley was quoted as saying that the data were sparse, so his own drastic revision should not be considered final until it had been tested appropriately by other investigations which he said were underway at Mount Wilson, Palomar, Leiden, and elsewhere. The story was easily traced to a press release from Science Service, written by Charles A. Federer, Jr, the editor of Sky & telescope, whose offices were at Harvard College Observatory. Shapley had given him the story, based on a paper he had presented orally at the American Astronomical Society (AAS) meeting in Amherst, Massachusetts, just a few days before the New Year. The old Harvard director had finally abandoned his resistance to the revision of the period–luminosity relation, and was trying to pretend that he was the one who had proved it wrong. According to the release, he based his reasoning almost entirely on the discrepancy between the absolute magnitudes of the globular clusters in M 31 and in our Galaxy, a difference that workers in the field, including himself, had long known but had not accepted as firm evidence.
Baade was furious. He fired off a letter to Sergei Gaposchkin, denouncing Shapley’s performance at Amherst as “simply shameless”. He had “lifted wholesale” Baade’s “remarks at Rome, without any acknowledgements”. There the Harvard director “had no relevant data of his own and ... was taken by complete surprise”, yet five months later, back in the United States, he had tried to pass off Baade’s conclusion as his own. In other letters the angry Palomar astronomer characterized Shapley as a “wind bag” and a “carnival barker” who had simply “repeat[ed] his old sales talk”. Learning that the Harvard director planned to publish this same paper in the *Proceedings of the National Academy of Sciences* (where it would not be refereed), Baade called it “a full discussion of his trash”. He wanted the IAU and the AAS to censure Shapley, and at least one important astronomer, Gerard P. Kuiper, agreed with him and wrote to the IAU general secretary to say so.90

However, cooler heads prevailed. Shapley had just retired after thirty-two years as director at Harvard, and no one wanted to attack him. Some believed that he was worn down and losing his grip. Otto Struve, then president of the IAU, and a former president of the AAS, thought the “whole matter [was] very delicate” and he did not “advise any action that would stir up excitement”. Bowen told Baade that when the minutes of the IAU commission meeting were published, everyone could read that he had revised the distance scale and that Shapley had asked questions showing that he either did not understand it or still doubted it after hearing the presentation, four months before his “paper” at Amherst. Bowen did ask Donald H. Menzel, the acting director at Harvard, to make sure that no more popular articles or news releases were issued crediting the discovery to Shapley, and none was. However, Menzel did not have the courage to tell the old ex-director to his face that he had been revealed as a plagiarist, although he freely admitted it to Bowen and Baade. Cecilia Payne-Gaposchkin, who had idolized Shapley when she first came to Harvard, but had soon learned to view him critically, now denounced him in a long letter to Baade. She had been at the meeting at Amherst but had deliberately not gone to hear Shapley’s paper, she said. She could not “endure listening to his unjustifiable boasting”, but neither could she bring herself to tell him that to his face. The one person who did beard the lion was Bart J. Bok. He had heard Shapley deliver the paper in January and “did not like it”; he thought at the time that “Shapley was trying to get on the band wagon” but said nothing. After the storm broke, however, he wrote to Struve about it, and then confronted Shapley, telling him that in his paper he should have referred to Baade’s prior announcement. The ex-director responded with a brief letter to Baade, saying he “hope[d] it was OK” to refer to the Palomar astronomer’s unpublished discussion at the Rome meeting. With it Shapley enclosed a revised abstract of his Amherst paper, briefly mentioning previous work by Baade, Joy, and Payne-Gaposchkin, but keeping most of the credit for himself. This revised version was published in the *Astronomical Journal* with other abstracts from the meeting. Baade did not reply and apparently never wrote to Shapley again.91

In the end, it all came out just as Bowen had said it would. Although Shapley
published his paper with what Baade considered quite inadequate references and a
slanted argument, no controversy erupted in print. Struve devoted two of his regular
monthly articles in Sky & telescope to “the distance scale of the universe”, the first
describing Shapley’s important early work on the problem, the second giving Baade
all the credit for the revision. Also, Struve’s quick letters to Federer had kept any
story crediting Shapley with the discovery out of the Sky & telescope post-meeting
column, “American Astronomers Report”. News of Shapley’s attempt to steal Baade’s
idea, work and results spread quickly through the little circle of senior American
astronomers, largely by word of mouth, emanating chiefly from Harvard itself, where
Menzel, Payne-Gaposchkin, Bok and Fred L. Whipple all supported the Palomar
observer. Shapley had held them all down for years, and had overbuilt and over-
staffed the observatory; now they were reaping the consequences in the form of
cutbacks ordered by the Harvard University president, James B. Conant. They felt
they owed little loyalty to their former director. Shapley did not help matters when
he presented another oral paper on the distance scale at the AAS meeting in Boul-
der in August of that same year. In it he went back to resting his case on the globular
clusters, this time in the LMC, and on the brightest stars in them. Again he gave
himself nearly all the credit, and this time he did not mention Baade at all. How-
ever, astronomers everywhere had accepted that the revised distance scale was
Baade’s work, not Shapley’s, long before the IAU transactions finally appeared in
1954, and anyone could read the report of the Commission 28 meeting.92

Before that, however, the public had to be told. The Rockefeller Foundation had
paid for the 200-inch Hale telescope; its trustees expected it, not Harvard’s anti-
quated and rickety telescopes in South Africa, to reveal the true distance scale and
the age of the universe. George W. Gray, the Rockefeller Foundation’s chief science
writer, had first called Shapley’s story in the New York Times to Bowen’s attention,
asking what was new in it that had not previously been known. Bowen tried to
explain the point at issue, assuring him that this was one of the most important
programs for the 200-inch, and one that Baade had been working on since the teles-
scope went into operation. It was not completed, but Baade planned to published it
“soon”. Because of the “difficult situation” created by Shapley’s release, Gray should
write an article for Scientific American, not attacking him, but giving “the truth”,
which the astronomers already knew, to the public. This was just what Gray and the
Rockefeller Foundation trustees were eager to do. Bowen briefed Gray, including
Hubble’s role in the early planning of the 200-inch program (no doubt at the elderly
scientist’s suggestion). Gray then interviewed Baade in person, and followed up
with written questions and a draft of an article. Their attempted collaboration quickly
turned into a disaster; Gray was a fine popular writer but did not understand as-
tronomy very well, and Baade refused to accept simplifications and demanded that
every sentence in the article be scientifically correct. He crossed out most of what
Gray had written, largely based on glowing material from Hubble, and substituted
his own technical, precise verbiage. Baade hated publicity, and showed it; he and
Gray were soon exchanging angry telegrams and letters. Bowen quickly stepped in, writing to Gray that "[u]nfortunately while Baade is a very brilliant astronomer, and has made great contributions to the field he does not always use the best judgement in these personal problems", a correct but understated analysis if "personal problems" is taken to mean publicity! With Bowen's help and sensitive suggestions, Gray was able to complete a well-written, understandable article which appeared in the June 1953 *Scientific American*, nearly a year before the *IAU transactions* were actually published. All the major newspapers had carried long stories in May, no doubt based on an advanced release of the article. Baade was the astronomer who had changed the size and age of the universe in the public mind.93

Actually, even before Baade's report to the IAU, Alfred Behr had published a paper on the distance scale of the galaxies in 1951, in Germany. It was based almost entirely on Baade's two 1944 papers, incorporating his revisions of the magnitudes of the photometric standard stars he used for M 31, and the absolute magnitudes for the most luminous population II stars he then used, somewhat different from his later, better determinations with the 200-inch. Behr's result was a change in the distance scale by $1.7 \pm 1.1$ magnitudes, corresponding roughly to a factor 2.2 in distance, which taken literally translated into changing the expansion age of the universe from $1.7 \times 10^9$ years (Hubble's old value) to $3.8 \times 10^9$ years. Baade ignored this paper, at least in print, no doubt because by the time it appeared he had much better values for all the numbers Behr had used in it. Since the paper appeared in the *Astronomische Nachrichten*, few American astronomers of the time noticed it, but some theoreticians did, including George Gamow, who liked the larger value for the age of the universe and questioned Baade about it. Unfortunately, if he replied, his answer has not turned up.94

Dartayet and Jorge Landi Dessi published a paper announcing their discovery of twenty faint variable stars in the Small Magellanic Cloud, on plates they had taken with the Bosque Alegre 60-inch. Probably only two of them were RR Lyrae variables, right at its faint-magnitude limit, but two others were evidently related, somewhat longer-period population II Cepheids. Baade's suggestion, transmitted through Adams's 1942 paper, had paid off. In fact, Dartayet and Landi Dessi had completed their paper and submitted it in late 1951, but neither got to Rome in 1952. Thackeray and Wesselink published their discoveries of the three RR Lyrae variables in the Small Magellanic Cloud, and of twenty-three more in two globular clusters in the Large Cloud, in 1953. Baade's revision had been confirmed in Argentina and in South Africa.95

Baade did not publish his own paper until 1956, as the written form of an invited talk he gave in Pasadena the previous year, when he received the Catherine Wolfe Bruce Gold Medal of the Astronomical Society of the Pacific. In this paper he described his reasoning and methods very clearly. He showed that the previous attempts to calibrate the absolute magnitudes from proper motions and radial velocities of the nearest classical Cepheids in our Galaxy had gone wrong, both because
of the smallness of their proper motions, and because the extinction by interstellar dust had been badly underestimated. On the basis of all his data then available Baade gave $m - M = 24.2$ as the distance modulus of M 31; that figure, slightly modified by his later work to 24.25, corresponding to a distance of 590 kpc, was used for many years as the key ingredient of the distance scale of the universe.  

9. Lectures and Courses

Although Baade never had a regular teaching position at a university, he was an excellent lecturer, and after the success of his Princeton lectures in May 1950, he received many invitations to be a visiting professor. Most he declined, or more diplomatically allowed Bowen to kill, as when J. Allen Hynek, an astronomer who was temporarily a dean at Ohio State University, requested the great observer to be the chief speaker at a three-day "astronomical festival" there, his travel to be financed
by Mount Wilson and Palomar Observatories. But one invitation Baade did accept was to spend two weeks in 1953 at Swarthmore College, an elite undergraduate school near Philadelphia. Peter van de Kamp, the professor of astronomy, did most of the teaching in the course, but invited several guest speakers, including Bok and Fred Hoyle, to give one or two lectures each on their research specialties. Baade prepared his seven lectures carefully, and in addition gave a Sigma Xi lecture, intended for students and faculty members from all fields of science. It, like all his lectures and short courses, was on his one subject, the population and structure of galaxies. Each time he gave it, he updated it to include his latest results and interpretations, and the related work of others. Probably it was a little above the heads of most of the listeners who had not studied astronomy recently, but he surely gave them a glimpse of a dedicated scientist who was supremely interested in understanding the universe. On the way back to California Baade repeated the Sigma Xi lecture at Case Institute of Technology in Cleveland, a chance for a visit with his long-time friend, Jason J. Nassau, its observatory director. Nassau, about the same age, height and stature as Baade, liked to dress just like him at astronomy meetings, to the confusion of those who did not know them well. 97

The most productive series of lectures Baade gave was in a month-long symposium on astrophysics in the summer of 1953, at the University of Michigan. The recently appointed director, Leo Goldberg, modelled it after the highly successful “summer schools” in astronomy and astrophysics that Shapley had organized at Harvard from 1935 to 1942, and the famous pre-war physics ones at Ann Arbor, at which such luminaries as Enrico Fermi, Wolfgang Pauli and Arnold Sommerfeld had lectured. Goldberg was one of Baade’s favourite young theoretical astrophysicists, who, as a Harvard graduate student, had visited Mount Wilson and discussed research with him there. He persuaded Baade to come to Ann Arbor that summer to help put Michigan back on the map as an astronomical research power. The other visiting lecturers were Geoffrey K. Batchelor, from Cambridge University, who spoke on the turbulence and magnetohydrodynamics that might be applicable in stars, nebulae, and galaxies; Gamow; and Edwin E. Salpeter, whose lectures on nuclear physics and element formation were relevant to stellar evolution and the two populations. Baade himself had urged Goldberg to bring the physicists to the symposium. 98

The National Science Foundation supported the symposium generously, and the cream of the crop of young postwar American graduate students and postdoctoral fellows were there. In addition to Gamow and Kuiper (who also lectured on the solar system), both then members of the National Academy of Sciences, eight of the attendees were later to be elected to it: Lawrence H. Aller, Margaret Burbidge, Goldberg, Donald E. Osterbrock, Eugene N. Parker, Vera C. Rubin, Salpeter, and Sandage. They, and an exceptionally high percentage of the other forty-odd young scientists who were there, had long and fruitful research careers.

In Ann Arbor Baade lived with most of the single students and postdocs in a
fraternity house which the symposium leased for the month. He thrived in the atmosphere, talking and discussing astronomy almost constantly, drinking too much coffee, smoking too many cigarettes, and getting too little sleep, but it was wonderful for the attendees. All of the participants in the symposium, whether they lived in the house or elsewhere, were greatly inspired by Baade’s lectures, and nearly all of their subsequent research was related in one way or another to his population concept.99

Baade repeated this course, updated, as the Hitchcock Lectures at the University of California, a prestigious endowed lectureship, in 1954. Although they were intended by the terms of a bequest for “the educated public”, Baade actually aimed them at the Berkeley graduate students and faculty. In addition he gave one popular lecture, and a colloquium on a two-day visit to Lick Observatory. At Berkeley Baade especially inspired Harold F. Weaver, by then a faculty member there, and Morton S. Roberts, a graduate student, and encouraged them to go on in galactic research.100

Back in Pasadena, Baade gave a graduate course on “extragalactic nebulae”, the old-fashioned name for galaxies which never quite died out of his mind, at Caltech in the winter quarter of 1956. With more time than he had previously had for his series of lectures, he began with an historical overview going all the way back to 1750 and the ideas of Thomas Wright on the Milky Way as revealing the shape of the Galaxy. As always he described his own growing realization of the population concept, starting with his and Hubble’s work on the Sculptor and Fornax systems, and then his own resolution of M 31 and its companions with the 100-inch during the Second World War. But he got up to his most recent results on the distance to M 31, and to Martin Schwarzschild and Sandage’s interpretation of the colour–magnitude diagrams of globular clusters in terms of stellar evolution. Several younger faculty members sat in on Baade’s course. They, and some of the students who took it, were led into galaxy and stellar evolution research by it, or were strengthened in their resolve to go on in that subject. Baade was an excellent lecturer, knowledgeable, a lively speaker whom it was impossible to ignore (he once described his voice to me, quite correctly, as “like a barking dog”). He always prepared carefully, and presented his material logically, although sometimes in oversimplified form, to get his point across clearly. Baade was evidently master of his subject, and loved to discuss any detail of it in answer to questions.101 Yet oddly, he thought he had failed in his lecturing, and believed that most of the students were “obviously bored”, and thought “there must be something about teaching which [he had] never grasped”. He could not have been more wrong, but he evidently expected more from the students than he saw them giving.102

His two Ph.D. thesis students, Sandage and Arp, who finished in 1952 and 1953 respectively, were an altogether different story. He expected much of them, and they gave it, and more. They were both supremely interested in astronomy; he taught them to observe, and the techniques they needed, and they did the rest. Baade was generous in his praise of their research, and they deserved it. Both of them admired
him greatly, not only for what he had taught them, but for his warm, friendly personality and fatherly interest in their careers. Sandage went directly onto the Mount Wilson and Palomar Observatories staff after earning his Ph.D.; Arp became a Carnegie postdoctoral fellow for two years, carrying out a search and statistical survey of novae in M 31, as a typical spiral galaxy. Then, under an NSF grant administered by Indiana University, he went to South Africa and observed southern hemisphere Cepheid variables, globular clusters, and a region near the galactic centre. In 1957 Arp returned to Pasadena as a member of the Mount Wilson and Palomar Observatories staff, and, like Sandage, became a leader in applying Baade’s ideas to galactic and extragalactic research.\textsuperscript{103} They were Baade’s closest scientific heirs, but there were many, many others into whom he had breathed the excitement of his kind of research in his courses and lectures over the years.

10. Symposia and ESO

Baade published considerably fewer papers than Jan H. Oort, Subrahmanyan Chandrasekhar, Struve, or any of the other great research astronomers of his time. Yet he spread his ideas, concepts and results very effectively in lectures, courses and symposia. His heart was in Germany and in Europe; once he could travel freely after the war, he liked to go there and participate in international research conferences, and the Europeans, hungry to learn of the latest observational results from Palomar, were eager to have him come. Oort, who had been Secretary-General of the IAU from 1935 to 1948, was a key figure in organizing most of the Continental astronomical meetings in the 1950s, and he naturally included Baade in all those that involved galactic structure. As soon as Oort learned that the Palomar astronomer would accept an invitation to Holland in 1953, he began pulling strings to get the necessary travel funds for him. Baade’s visit quickly turned into the occasion for the first IAU symposium held separately from a General Assembly. The subject was to be coordination of galactic research, and the place Groningen.

Baade, who believed in research conferences for experts only, wanted to keep this meeting as small as possible; Oort wanted to be sure many countries were represented, particularly European ones; and Bok, the most vocal member of the organizing committee, wanted everyone working on galactic structure to come. In the end they compromised on twenty-five scientists from ten countries, led, not surprisingly, by Holland and the United States, with five each.

At one point Baade almost backed out of attending, because his prior commitment to lecture at the Michigan summer school conflicted with the dates first proposed for the Groningen symposium. But Oort moved it up to the end of June, and Baade came. Although he had to delay his trip for several weeks, waiting for his re-entry permit to the United States, in the end he was able to spend nearly two months in Holland. He, Oort, and Adriaan Blaauw (who returned to Holland briefly that summer) discussed M 31 and our Galaxy, radio and optical results, and new programs almost daily. They also planned the symposium, the order in which the various
topics would be discussed, and even began to prepare a draft report.  

During the symposium Baade gave the keynote opening talk summarizing recent extragalactic research. He was at his best in the kind of discussions that went on all week, describing his results and throwing out ideas for future programs. For many of the older Europeans, this symposium was their best chance to learn what the most important fresh new research areas were, and to try to carve out parts of them for their observatories and their countries. As soon as the symposium ended, Baade had to fly back to America to teach at Ann Arbor, but he had made a powerful impression on the European astronomers and revitalized their research interests. One of the important future programs Baade emphasized in the discussions, sampling the density of RR Lyrae variables (and thus of population II) along several well-chosen lines through the Galaxy at high galactic latitude, was later carried out by Lukas Plaut, the local (Groningen) secretary at the symposium. It required the wide-field 48-inch Schmidt, which was nearly completely tied up with the Palomar Sky Survey until 1956, but Baade was able to get a few plates before that to pick out the best fields. Then Plaut came to America, took hundreds of plates, returned to Groningen, found the variables, estimated their magnitudes, and published the data years after Baade’s death. Plaut’s results were somewhat inconclusive because of problems with the corrections for interstellar extinction; all the other concepts in the program went back to Baade’s surveys of the fields around high-latitude globular clusters with the Hamburg 1-metre telescope in the 1920s, and of his ‘window’ near the galactic centre with the 100-inch in the mid-1940s.

In the summer of 1954 Baade took his wife back to Germany to visit their surviving relatives and friends. It was a “strict family visit” to Minden and Hamburg; his mother had died during the war as Richard Schorr had in 1951, but Baade’s brother and two sisters were still alive.

In 1955 he returned to Europe for three meetings. The first was more or less of a lark, a celebration of the fiftieth anniversary of Albert Einstein’s publication of his first paper on relativity theory, organized by Pauli, Baade’s friend since their Hamburg days. The meeting was held in Bern in July; many outstanding physicists, a considerable fraction of them German or German emigrés including Max von Laue, Max Born, Walter Heitler and Eugene Wigner, were present. Robert Trumpler, the Swiss who had been Leopold Ambronn’s student at Göttingen just before Baade and had emigrated to America in 1915, gave a paper on the observational evidence for the predicted gravitational light deflection and red shift. Baade himself spoke on “the expansion of the universe” and gave his best current value of the Hubble constant, which corresponded to an age of the universe of about 5.4 × 10⁹ years, with an uncertainty estimated at “still perhaps 20%” according to the report of the meeting. Baade had agreed to speak on this subject at Pauli’s urging, although he privately wrote to his old friend saying that in his opinion the history of the universe would really come from understanding star formation and evolution, not by “chasing the cosmological problem”. Baade never submitted his manuscript, and Pauli
Fig. 6. Baade on a visit to the Hamburg Observatory in 1955. Courtesy of Hamburg Observatory.
eventually published the symposium proceedings without it, including only a two-sentence statement that the paper had been presented orally.107

The main events for which Baade had come to Europe that summer were the dedication of the new Hamburg Observatory 32-inch Schmidt telescope in August (Figure 6), followed by the IAU General Assembly in Dublin. Otto Heckmann, whom Baade and Schorr had both favoured, had finally been named director of the observatory in 1942, after a long bureaucratic struggle between the advocates of “German science”, who preferred a real Nazi, and the bulk of the Hamburg University faculty, who wanted a true scientist. Heckmann, a Catholic and a relativity theorist, made only the minimum concessions to Hitlerism necessary to keep the observatory alive, and came out of the war with a relatively untarnished reputation. After the war, Baade supported him in every way, as he had promised Schorr he would. Baade helped arrange a tour of U.S. observatories for Heckmann in 1950, when most Germans were still not welcome, and made sure that he was invited to every important international astronomical conference, beginning with the IAU Symposium 1 at Groningen.108

West Germany had prospered in the 1950s under Konrad Adenauer, and Heckmann persuaded his government to provide the funds to build the 32-inch Schmidt, which Schorr had originally gotten into the budget, as a symbol of peaceful science. Baade had supported it strongly, and Bowen had provided the plans for the 48-inch Schmidt, which the German engineers used as a starting point. Baade’s homecoming to Hamburg was a sentimental event. He was photographed at the 1-metre reflector he had used so well in the 1930s, and gave an invited talk comparing, as always, the structure of M 31 and our Galaxy. Oort gave another on the spiral structure of our Galaxy, summarizing the latest Dutch and Australian 21-cm results, and Arno A. Wachmann, another Hamburg astronomer, reminisced on Bernhard Schmidt’s life and career. Along with numerous German astronomers, several Americans who had used Schmidt telescopes were present and gave papers.109

Just a week later Baade was at Dublin, for the IAU General Assembly. He, Oort and Blaauw had outlined the preliminary program for the symposium on the comparison of the structure of our Galaxy with other galaxies. Most of the world experts on galaxies were there, including Viktor A. Ambarzumian, Grigory A. Shajn, and three other Soviet astronomers. Baade gave the keynote paper for the whole symposium, on the large-scale structure of spiral galaxies (which he still referred to as nebulae). He was the acknowledged leader of the field. Again, he did not submit a written manuscript, but Nancy G. Roman, the editor of the symposium, reconstructed one for him from a tape recording, which he then revised for publication.110

In 1956 Baade stayed in the United States, but took part in a conference on the cosmic-distance scale “from trig parallaxes to galaxies” at the University of Virginia, sponsored by the National Science Foundation. Although Oort could not come, Heckmann and Wilhelm Dieckvoss were there from Germany, Ben Gascoigne from Australia, and many American astrometrists and galactic-structure researchers.
Baade's paper began with his discovery of the two populations, continued through his revision of the distance scale, and ended with his and Swope's latest results on the Cepheid variables in his outermost field in M 31. He had picked it in the hope that extinction would be small or non-existent there, but they were finding it was still present, though not as strong as in the fields closer to the nucleus.

In 1957, the last year before he retired, Baade gave several lectures in England, and then took part in the most important conference of his career, at the Pontifical Academy of Sciences in Vatican City. The Royal Astronomical Society awarded him its Darwin Medal in 1954 but he waited until May 1957 to go to London to receive it and deliver the Darwin Lecture to the society. He was also in England to give the Halley Lecture at Oxford, its highest recognition for an astronomer. In this lecture he included, along with his newest stellar population results, a tribute to Hubble, who had been a Rhodes Scholar there for three years before beginning his graduate work in astronomy in America. On that same visit to England Baade also gave colloquia at Herstmonceux, the new site of the Royal Greenwich Observatory, and at Jodrell Bank, the big radio observatory near Manchester. By the time he left for Rome, almost every research astronomer in England had had a chance to learn from him about the two populations, stellar evolution, and the structure of galaxies.

With Daniel J. K. O'Connell, the Jesuit priest who was director of the Vatican Observatory, Oort and Baade had been planning since 1955 for the "study week" on stellar populations, which was to be held under the Pope's auspices. The idea was that a group of world experts would immerse themselves in discussing the problem in detail for a week at the Vatican, and emerge with a series of conclusions which the Supreme Pontiff would endorse, as in the two previous similar conferences, on cancer in 1949, and on microseisms in 1951. The number of participants was limited to just over twenty top scientists; at Baade's insistence several of them were theorists, including Hoyle, Schwarzschild, Salpeter, Lyman Spitzer, Jr, Bengt Strömgren, and also two young American observers who were already world experts, Sandage and George H. Herbig. William A. Fowler, the Caltech nuclear physicist who had become a leader in understanding element formation in stars, was also there.

The main subject was stellar evolution, the field that had not existed before Baade's discovery of the two populations. By 1957 it had become clear that population II stars were not only old, but had relatively low abundances of elements heavier than hydrogen and helium ("metal-poor"). This was clearly an age effect, resulting from nuclear transmutations, and at first Baade had believed that age and heavy-element abundance were strictly correlated; however William W. Morgan (who was at the Vatican conference) and later Arp and others had produced strong observational evidence that they were not, and that there were old stars with nearly normal "metal content", particularly in the plane of M 31 near its nucleus, and in our Galaxy. Oort and Blaauw had spent considerable effort on preparing a detailed scheme for dividing
stars up into six populations instead of two, but after prolonged discussion the con-
feres compromised on five, with an associated evolutionary picture. Baade ac-
cepted the evidence for old, high metal-content stars, but continued to think in terms
of just two main populations. The book that came out of this conference, which
includes Baade’s three papers, along with all the others presented and edited tran-
scripts of the discussions following them, became an important reference for the
next decade.114

After the conference, Baade, Oort and Blaauw went on to Amalfi, on the Gulf of
Salerno south of Naples, to discuss ideas for the symposium on galactic structure,
to be held at the IAU General Assembly in Stockholm in 1958. Baade was not to
attend it, however, for he would have to retire in June of that year, and had other
plans.115

In addition to his many lectures, invited papers, and discussions with European
astronomers, Baade played a key role in getting the project that became the Euro-
pean Southern Observatory started. He and Oort probably first conceived and dis-
cussed this concept not in Groningen in 1953, as the official history of ESO states,
but in Pasadena in 1952.116 Oort taught a graduate course at Caltech during the
winter quarter of that year, and spent much of his time at the Mount Wilson and
Palomar Observatory office before going on to lecture at Princeton in the spring;
years later Horace W. Babcock remembered hearing about Baade and Oort’s dis-
cussions at the time.117

Baade had long realized the advantages of an observatory south of the equator.
Back in 1927, when he returned to Germany from his year in America on a
Rockefeller Fellowship and reported on it to the Hamburg board that governed the
observatory, he strongly advocated moving its 1-metre reflector to South Africa or
South America. After seeing the big Mount Wilson telescopes in their far superior
California climate, he had concluded that it would be impossible to compete with
them in Germany. The Americans were already planning a 300-inch (which was
later built as the 200-inch Hale reflector). Either give up modern observational
astrophysics and stick to second-rate projects or move the biggest telescope in Ger-
many to the southern hemisphere, was Baade’s youthful, impetuous advice. Ham-
burg Observatory could seize the initiative in the southern skies, but if the Germans
waited too long the Americans would ship their big telescopes south and beat them
there too.118

Hamburg had not moved its 1-metre reflector south nor given up astronomy, but
Baade had departed for Mount Wilson four years later. His friend Erich Schoenberg
had headed the German astronomers’ drive to build an observing station in South
Africa, but nothing significant resulted.119 A quarter of a century later, Baade could
no longer envisage a Hamburg southern telescope, nor a German one, but he still
considered himself more of a European than an American, as did Oort, who had
deprecated several proferred directorships in the United States, including those at
Yerkes and Harvard.
Certainly the Groningen symposium in 1953 was where the ESO idea came out in the open. Very probably Oort had insisted on inviting some of the superannuated European directors like Sir Harold Spencer Jones and Paul Couderc, who were not galactic structure experts, to participate in it in the hope of enlisting their countries in the project. Baade was the one person present at the symposium with unrivalled big-telescope experience and a strong sympathy for the idea. Talkative and insightful, he dispensed advice freely to all of the Europeans, then and later. Baade was never an official member of any of the ESO committees, but he frequently advised Oort, who pushed the idea through to reality. Baade always referred to it as the “European” or “southern” observatory, and more than once as one “we” need; Oort usually called it the “co-operative” observatory. Probably Baade’s biggest contribution was to emphasize how important it was to make long, careful seeing tests at possible sites everywhere in the southern hemisphere, rather than rushing into an existing one in South Africa, as all the European directors, including Oort, had first intended to do.\textsuperscript{120}

In 1957, when an American astronomer publicly proposed that the U.S. build a large telescope in the southern hemisphere, Baade described him to Oort as “a pretty dumb fellow with an outsize ego”. All the real leaders in American astronomy were committed to building the National Radio Astronomy Observatory and “the National Observatory (probably in Arizona)” first, Baade reassured Oort, and “any further projects in the foreseeable future [we]re definitely out”. Baade was “very strong” for ESO, he wrote Oort, for without it astronomy in Europe would remain “too stunted”, and he was “far too much the European to give in without a last ditch fight”. Oort incorporated relevant paragraphs from Baade’s letters in his reports to the other ESO Committee members, and by the end of 1957 felt that it would become a reality, although the United Kingdom had decided to go its own way.\textsuperscript{121}

11. Harvard and Australia

Baade attended his last international scientific meeting in early June 1958. It was a Solvay conference, the eleventh in a series dating back to 1911, in which a small group of experts, mostly theoretical physicists, discussed a particular topic. This one was on “the structure and evolution of the universe”. The week began with several abstruse “general statements of cosmological theory” by George Lemaître, Otto Klein, John A. Wheeler, Heckmann, and others. Then Oort, Baade, Sandage, and the Jodrell Bank radio astronomer Bernard Lovell filled them in on “experimental data on the universe”. Pauli and Robert Oppenheimer dominated the questioning, and Baade hardly paid any attention to the cosmologists, but praised Lovell’s results on the strong radio sources that he and his collaborators were beginning to find in clusters of galaxies. As at the relativity conference in Bern three years earlier, Baade did not bother to submit a manuscript for publication, but took a lively part in the discussions on supernovae, nucleogenesis, and stellar populations.\textsuperscript{122}

Then at the end of June, Baade was in Madison, Wisconsin, to give the Henry
Norris Russell Lecture of the AAS. It is the highest honour of the Society, awarded for “a lifetime of eminence in astronomical research”. Fittingly, he gave an inspiring review of galactic structure and stellar evolution as they were then understood through his concept of the two stellar populations. But 30 June marked his official retirement, and he was never to observe again with the 200-inch. Under the strict Carnegie Institution of Washington policies then in effect, a staff member had to retire at the end of the fiscal year in which he reached the age of sixty-five. He could keep an office (perhaps a smaller one), continue research with the data he had, and even observe with the 60-inch or occasionally even the 100-inch when no regular staff member wanted them, as Joy and Paul W. Merrill had done, but the 200-inch would be out of the question.\textsuperscript{123}

Baade did not want to do that; he intended to return to Germany and work up all his data there. First, however, he was going to do some teaching. Partly this was to earn money; Baade was a well-paid senior staff member but also a free spender, and the CIW retirement pensions were low. Under the option he chose, he would receive $236 each month for the rest of his life, approximately the same salary a young assistant professor then earned. After his death his wife, Muschi, would receive half that amount.\textsuperscript{124}

Long before Baade’s retirement, Cecilia Payne-Gaposchkin conceived the idea of inviting him to lecture for a semester at Harvard, and he had indicated that he would like to do so in the fall semester of 1958, just after he retired. Menzel, who had finally been named director of HCO in 1954, enthusiastically endorsed the idea. Dean McGeorge Bundy allotted $6,000 for Baade’s salary for the one semester, a quite significant addition to his retirement income. Payne-Gaposchkin’s plan from the beginning was to record and transcribe Baade’s lectures, which he could then use as the basis for a book. An experienced author and editor herself, she realized how hard it was for Baade to write for publication, but she was determined to get his results into print. She knew how important they were.\textsuperscript{125}

Payne-Gaposchkin arranged for Baade to stay in a visiting faculty apartment in one of the student houses at Harvard, and although he postponed the start of his lectures to remain in Pasadena to lobby for ESO with two prominent visiting German politicians, once he got to Cambridge and began teaching he found that he enjoyed it. He thought that Harvard had “a remarkably good bunch of able students, much better than the mediocre fellows, which usually crawl around at the Caltech”\textsuperscript{126}

For Baade’s first lecture on 29 September, forty listeners turned up, including three professors and eight Ph.D. research associates. Payne-Gaposchkin gave an emotional welcome, saying Baade’s lectures were “the fulfillment of a dream, deeply fraught with all sorts of things”, and Baade began with his historical introduction. The attendance gradually tailed off until it reached about twenty-five halfway through the course, and then remained constant; Payne-Gaposchkin, her husband Sergei, Hynek, and David Layzer attended nearly every lecture. Baade flew home to California for the three-week Christmas vacation, returned to Cambridge after the New
Year, and finished his last lecture on 13 January 1959. It was an excellent course, covering galactic structure and stellar evolution thoroughly.  

Then Baade went on for a month at the Institute for Advanced Study in Princeton, where Oppenheimer was director and Strömgren the professor of astronomy. Several of the theoretical physicists who dominated the Institute were then interested in stellar evolution, especially supernovae and nucleogenesis. Baade had been the first author of a paper with Geoffrey and Margaret Burbidge, Hoyle, Fowler and Robert Christy, all then at Caltech, in which they tried to interpret the near-exponential decay of the light curve of type-I supernovae in terms of the recently synthesized artificial radioactive isotope californium 254, so his name was well known to the physicists. Baade gave four lectures, one each week, and his notes show that he was right up to date on the latest details of the abundances of the elements in various samples of populations I and II. The Princeton institute had a handsome endowment, and Baade probably received a generous stipend.

After Princeton, the “itinerant preacher”, as Baade jokingly called himself, returned to Pasadena for little more than two weeks before departing for Australia. Bok had left Harvard in 1957 to become director of Mount Stromlo Observatory, with its 74-inch reflector, operated by the Australian National University (ANU) only a few miles from its campus in Canberra. In 1958 he arranged for Baade to come to Australia for six months of the next year to lecture and consult with the Mount Stromlo astronomers, and the “radiophysicists” of the Commonwealth Scientific Industrial and Research Organization in Sydney, one of the leading radio-astronomy groups in the world. Baade’s stipend was to be £2,000 (roughly $10,000) plus travel expenses, “just about tops” for Australia, according to Bok. The radio astronomers were especially anxious to talk to Baade, and he was keen to form new contacts with them.

Baade flew from Los Angeles to Sydney, with a stop in Honolulu. He arrived in Sydney in early March, and gave a colloquium on radio sources, then plunged into scientific discussions with the radiophysicists. A week later he went on to Canberra, but he had come down with pleurisy and bronchitis, which kept him out of circulation nearly a month.

Bok had organized a symposium on cosmology at the ANU, coinciding with a visit by the English theorist Hermann Bondi. To Baade it was a waste of time; he consistently thought and said in his lectures that it was much more important to get better data on galaxy magnitudes, populations, forms, spectra and colours first, before discussing the universe theoretically. Nevertheless, Bok signed him up for a talk on the cosmic distance scale at the symposium, telling Baade he could “leave cosmology out of it” altogether. Baade was still sick when the symposium began, and William G. Tifft, a visiting American postdoc who had taken his graduate course at Caltech, substituted for him. The next day Baade was well enough to put in an appearance.

He gave a series of seven lectures at ANU in July and August, and discussed
research constantly with all the staff members and students, particularly emphasizing important new southern-hemisphere observing projects. They found him ebullient, colourful, and inspiring. While he was at Mount Stromlo, Baade took the opportunity to observe with its 74-inch reflector himself. It was "the most uncomfortable instrument with which [he] ever observed", but nevertheless Baade obtained several series of plates of NGC 6522, the globular cluster in "his" window near the galactic centre, to check on Sergei Gaposchkin's periods of the RR Lyrae variables in it. By then Baade was relaxed about observing; one night he arrived so late at the observatory, after a good dinner, that Bok exploded that if he were a graduate student he would fire him! Very probably Baade had gotten there early enough to start as soon as NGC 6522 was sufficiently high in the sky so that he could get good plates of it, but Bok was a fanatic on using every minute of telescope time. At Mount Stromlo Baade studied all the available plates of the Magellanic Clouds also; they fascinated him because he was learning to see the past history of star formation in them.\textsuperscript{132}

Soon after Baade's lectures ended in mid-August, Bok hustled him off to the annual meeting of the Australia-New Zealand Association for the Advancement of Science in Perth, in distant Western Australia. There they took part in a symposium on "Radio and optical studies of our own and other galaxies", together with two visiting Americans then at Mount Stromlo (Tifft and Hugh M. Johnson) and a number of Australian astronomers and physicists. Immediately following it, Bok took Baade on a search for an observing site in Western Australia, seven days of jolting travel in a crowded automobile, sleeping in back-country accommodations in a land Baade had written reminded him of the California of thirty years earlier. Bok was a compulsive talker and doer; his heart was in the right place but he did not realize that he was wearing Baade down. A picture of the German astronomer, taken on a rocky desert somewhere in Western Australia, shows a tired, travel-worn man.\textsuperscript{133}

From Perth Baade flew straight back to Sydney, where he enjoyed a more relaxed two weeks repeating the gist of his Mount Stromlo lectures in three shorter talks, and discussing their research with the radiophysicists individually. In mid-September he left Australia; during his six months there he had seen and talked with practically every astronomer and radiophysicist in the country, and they all loved him and had been inspired by him. En route he stopped for two days' visit in Honolulu with his old friend, Kenneth Mees, now retired, who as head of the Eastman Kodak Research Laboratory had provided him with the best, most sensitive plates they could produce.\textsuperscript{134} Then it was on to Pasadena, where, as on each of his earlier brief stops, Swope reported to him on her progress in measuring the light curves of Cepheid variables on his plates of M 31 and the Draco dwarf system. He also packed a few plates and the mass of papers containing the notes, measurements, reductions and calculations he had accumulated in his twenty-seven years at Mount Wilson. His wife Muschi was already in Germany, looking for a permanent home for them; on 21 October 1959, after a flight across the country and a night's rest in a hotel on Long Island, Baade flew on to join her there. It was to be his final destination.\textsuperscript{135}
12. Return to Germany and Death

Baade arrived back in Germany little more than two weeks before he was to begin lecturing again. Göttingen University had appointed him its Gauss professor, a prestigious and well-paid short-term lectureship named for the outstanding mathematician and Göttingen Observatory director, Carl Friedrich Gauss. Baade was to give his course on “Evolution of stars and galaxies”, this time in German for the astronomy graduate students at his own alma mater. But he only got through the first lecture, on 7 November 1959. From the summer of 1958, when he had retired, Baade had begun confiding to his closest friends that he felt old and tired. The strenuous lecturing schedules at Harvard and in Australia, and the constant travel, had worn him down further. Living on his own for long periods of time with students had not helped; he got little sleep and to some of the more strait-laced of them seemed to exist on a steady diet of cigarettes, coffee, and scotch whisky or brandy, with few healthy green vegetables. His congenital hip defect had worsened with age, and bone spurs had grown inward from two of his vertebrae, pinching the nerves in his back and causing intense pain. By late 1959 Baade could no longer stand nor sit; he could only achieve partial comfort by lying prone, and he had to cancel his lectures.136

Muschl had begun house-hunting in the summer, and had found an apartment for them in Bad Salzuflen, a pleasant little spa close to Herford, not far from Minden, where Baade’s favourite sister, Kaethe Wehmer, still lived with her family (his brother, Martin, had died). It was a two-hour journey by train or automobile from Göttingen. They had planned to move into it after Baade’s lectures were to end in March 1960, but because of his painful condition they left Göttingen and went to Bad Salzuflen just before the end of 1959. Baade had spent much of his time since arriving in Germany on his back, but he believed his problem was a “harmless but painful affair” which the doctors would soon “fix”. However, he was never able to write again after his first Göttingen lecture; he dictated the few letters he sent after that to Musch, who typed them neatly or wrote the shorter, more personal ones by hand.137

Baade’s physicians tried massage, then radiation (probably X-rays), but neither helped. In January his condition deteriorated seriously, and he suffered from palsy, then “lameness” or paralysis of his legs. His ingrowing vertebrae were pressing on his spinal column and pinching his nerves, and the only possible remedy was a serious operation. Baade underwent it on 27 January, in the neurosurgical clinic at Göttingen, one of Germany’s best medical schools. The operation was described as successful but in order to heal, Baade was ordered to remain prone, in bed in the hospital, for many months, and his recovery was slow.138

He tried to keep his illness secret, and so Bowen, Swope (who was reducing and analysing her measurements of his plates), and his other friends in Pasadena did not know why they had not heard from him, but they feared the worst. In February Baade allowed Musch to send Swope an upbeat report on the operation and his condition. The previous year, while Baade was in Australia, he had asked the new
National Science Foundation if it would provide travel support and research support for him at Pasadena, Leiden and Hamburg Observatory to finish analysing his observational data and write the results up for publication. The infant agency was flush with post-Sputnik funds intended to “restore” America’s “lead” over the Soviet Union, and Geoffrey Keller, the Ohio State theoretical astrophysicist who was its temporary program director for astronomy, well aware of the “very high ... scientific merit” of Baade’s work, had assured him there would be no problem at all in meeting his modest request for $3,500 for three years. An American institution would have to administer the funds, so if he were appointed a research associate at Mount Wilson and Palomar Observatories, even if the CIW, with its well-known reluctance to touch federal grants, would not accept one for him, Caltech no doubt would do so. Not to be outdone, Caryl P. Haskins, the CIW president, acting on Bowen’s recommendation, not only appointed the retired German astronomer to the research associateship, but offered him $5,000 for four months in Pasadena, if he would do the work there and repeat his lectures as a course at Caltech in the fall quarter of 1960. Bowen, writing to wish Baade a rapid recovery, urged him to accept the appointment; according to Muschi he intended to do so.\textsuperscript{139}

Menzel, at Harvard, also hoped to get Baade to come back there for another year; no doubt Payne-Gaposchkin had initiated this idea, planning that with her assistance he would convert the lecture notes of his 1958 course into a book. Although Bundy, the Harvard dean, had no available funds for a special appointment in 1960–61, he suggested that since Baade seemed to be “perennially young”, Menzel could try again another year.\textsuperscript{140}

Muschi had also given Oort a guarded report on her husband’s condition. Baade had agreed to be one of the main lecturers in a summer course on the structure and evolution of our Galaxy, to be held at Nijenrode in the Netherlands in July and August 1960. Oort hoped that Baade would be able to come by then; he also asked him “if you can easily write in bed” to let him know his “exact plans” and how soon he would come to Leiden to work on his data. Oort thought Baade should also begin writing his papers at Hamburg Observatory, and even wanted to get him involved in an ESO committee meeting in Heidelberg in July. Fowler, on the other hand, from Pasadena sent Baade the latest Caltech nucleogenesis results and only wished that “you were here to keep me on the straight and narrow path”.\textsuperscript{141}

Alas, none of their plans was to come true. Baade’s doctors finally allowed him to sit up in a wheelchair for a few minutes late in June, nearly five months after his operation. Just a few days later, only the third time he sat up according to one report, he collapsed and died suddenly. Undoubtedly the months of lying prone had thrown his circulatory system into complete disarray, and probably a blood-clot or embolism was the immediate cause of death. It was “the saddest event in astronomy for years to come” in the words of Heckmann, the Hamburg Observatory director who was soon to become the first director of ESO. It was a terrible surprise to all of Baade’s friends, only a few of whom knew how ill he had been. His body was
buried in Bad Salzuflen, on a gentle slope with a view over a pleasant valley, under
a massive boulder bearing only his name, WALTER BAADE.

Muschi lived on for many years after her husband,treasuring his memory to the
end. In 1963 she moved to Bad Oeynhausen (also near Herford), then in 1965 to
Bremen, in 1967 to Neumünster, and finally in 1976 to Lübeck, where she eventu-
ally entered a nursing home. She died there on 31 August 1988, one day before her
ninety-fifth birthday.142

13. Baade’s Legacy

Baade had expected to live many years in Germany, working up all the observa-
tional data he had accumulated and publishing a long series of post-retirement pa-
ers. Oort, not realizing the seriousness of his friend’s condition, had begun press-
ing him to get to work on his research even while he was still in the hospital. Very
soon after Baade’s death the Leiden director urged his widow to turn over her hus-
band’s notes and work in progress to him, his Dutch colleagues, and Heckmann’s
group at Hamburg, who would convert them into papers and publish them. In
Pasadena, Swope had been tirelessly measuring magnitudes, colours, and Cepheid-
variable light curves on Baade’s 200-inch plates of M 31 since his departure for
Germany. She had slowly come to recognize how ill he must be from his failure to
reply to her letters reporting progress and asking for further guidance. Swope was a
close friend of Muschi, and now, almost simultaneously with Oort, she wrote to the
grieving widow, asking her to send Baade’s observing records, notes, papers, and
the few plates he might have back to Pasadena. She, Minkowski, Sandage, and Arp
would use these materials, together with the measurements she already had, and
others she would continue making on Baade’s plates, to prepare and publish papers
in his name, giving his results.143

Swope, although undoubtedly sincerely moved by Baade’s death, and doing her
best to console her friend Muschi, had clearly written parts of her letter with Bo-
en’s advice. As director of Mount Wilson and Palomar Observatories, he wanted
his best-known staff member’s posthumous papers published from it, and he had
written so to Muschi, in a letter of sympathy the day after Baade’s death. Oort on
the other hand thought that the California astronomers had more than enough data
of their own; he believed that the Europeans would do a better job of working up
Baade’s material and would publish it more quickly. Bowen and Oort jostled po-
litely and gingerly over who would do what, but the Palomar director held all the
cards; Baade’s plates, which belonged to the observatory where they had been taken,
were still in Pasadena. Baade’s notes were in “a big wooden box” in their garage in
Bad Salzuflen, and though Oort had them moved to Leiden, inspection revealed
that it would be impossible to publish anything without going back to the plates.144

Both Bowen and Oort regarded Baade’s ideas as his intellectual property, and
took it for granted that his widow, his sole heir, therefore at least in principle owned
them and should ultimately control the publication of his unfinished work. No doubt
Muschi, depressed and alone, felt caught in the middle between these two quiet, polite, determined directors. She came up with a plan under which Woltjer, whom Baade had regarded as “the best among the younger astronomers”, should be given “all of Walter’s unfinished work” unconditionally. He was to do what he thought best with all this material, using whatever he wished from it for his own research, and giving other parts of it to those he thought best qualified to make good use of it in theirs. Oort was surprised; clearly he felt that he himself was the obvious best choice for this role, not his young former student who was “primarily a theoretician and no expert on photometri[c] problems”, but he was “a first rate man” and the Leiden director “would fully trust him even for a task like this”. Oort and Heckmann “would continually discuss matters with Woltjer and keep a general eye on” him. Bowen was sceptical, but Woltjer was then working at Yerkes Observatory, and Minkowski, who did not know him well and feared he would be “as stubborn as a Dutchman”, met him there and discussed the matter with him thoroughly. Minkowski then concluded that no real problems would arise, and on his advice Bowen did not object to this plan but made it clear that any of Baade’s data that Mount Wilson and Palomar astronomers wanted would remain in Pasadena.¹⁴⁵

In the end, publication of Baade’s remaining research results went smoothly. Swope completed her measurements of his plates and published them in three long papers (listing him as the first author and herself as second), summarizing his conclusions on the Draco dwarf system, one of the lowest-luminosity population II galaxies then known, and on three of his variable-star fields in the Andromeda galaxy. Arp published the coordinates of the H II regions in M 31 he had measured as a graduate-student research assistant from Baade’s direct photographs, outlining its spiral arms, in another joint paper. Arp showed how well the spiral arms they defined matched Baade’s sketches, and included a map of them. He emphasized that these arms did not fit a logarithmic spiral, and showed that the distribution of the H II regions could best be understood in terms of a warp (or bend) in the disk of M 31.¹⁴⁶

S. L. Th. J. van Agt, Oort’s student, published the one paper that came from Leiden, on the Ursa Major dwarf galaxy. One other direct fruit of Baade’s observational data was Virginia Trimble’s Caltech thesis on the expansion of the Crab Nebula. Mayall, Baade, and Oort had all used John C. Duncan’s 1939 measurements of the proper motions of the filaments, from blue-sensitive and orthochromatic plates he had taken with the 100-inch telescope. Baade had realized that red-sensitive plates, obtained with his red filter that isolated Hα and the [N II] lines, suppressed the continuum radiation of the “amorphous mass” (the relativistic electrons) and made possible much better measurements of the filaments. He had taken first-epoch plates this way with the 100-inch, beginning in 1939, and with the 200-inch, in 1950; in the mid-1960s Guido Münch, Trimble’s thesis adviser, repeated these exposures and she measured the proper motions from them. Her paper, published eight years after Baade’s death, immediately became the definitive reference on the subject.¹⁴⁷
Baade’s magnum opus appeared in print years before these last papers based on his observational data. After his death, the 1960 summer course on the Galaxy had been held in Nijenrode without him. No single lecturer could take Baade’s place, but several of them, especially Oort and Plaut, expanded their previously planned series of talks to include subjects that would have been his.\textsuperscript{148} Quite a few of the participants, mostly advanced graduate students, postdocs, and young faculty members, were from the United States; one of them had brought along a copy of the mimeographed notes of Baade’s lectures at Harvard in 1958–59. They provided a much better statement of his ideas and results on stellar populations, stellar evolution, and galactic structure than anything then in print. These notes were kept in the reading room at Nijenrode, and Blaauw suggested that they might be copied and distributed to the participants. He and Oort had a secretary begin retyping them on mimeograph stencils, while the student, John Gaustad, wrote to Harvard to request permission to distribute them. The reply that came back was pleasant but firm; because of “certain references, especially to other astronomers, which might be misunderstood unless the context was known”, Payne-Gaposchkin and Baade had decided long ago not to distribute the notes except to those who had attended the Harvard course. Furthermore, Frances W. Wright, who replied to Gaustad, made it clear that Payne-Gaposchkin had been responsible for bringing Baade to Harvard to give the course, and had seen that his lectures had been recorded and transcribed, so she certainly deserved to be the one to edit a book based on them.

In fact, Payne-Gaposchkin had already begun converting the notes into a book-length manuscript, working sporadically in her “spare time”. Now this exchange galvanized her into action. She was in Pasadena on sabbatical leave that autumn, and switched to full-time work on the volume. She planned to publish it as a book by Baade, edited by herself; everyone recognized that she was the right person to do it. Harvard University Press wanted to publish the book, and Oort suggested that the royalties be split equally between Baade’s widow and Payne-Gaposchkin. This was just what Muschi had thought appropriate, but evidently Payne-Gaposchkin insisted that her own share be reduced to twenty per cent, with the rest to Muschi, for that is what the final contract specified. Payne-Gaposchkin finished the manuscript by March 1961, cutting out all Baade’s criticisms of other astronomer’s work (which she considered appropriate in a lecture but not in a book), somewhat rearranging the text, and rewriting it completely. She corrected a few actual errors that had crept into the notes, but remained true to Baade’s ideas and concepts, not trying to recast his thoughts to fit her own.\textsuperscript{149}

Harvard University Press did not publish \textit{Evolution of stars and galaxies} until 1963, but it became an immediate success among astronomers. It was the first clear, book-length exposition of stellar evolution, galactic structure, high- and low-velocity stars, the two stellar populations, the types of galaxies and how they perhaps had formed and would evolve that had been published. All astronomical libraries ordered it, and many astronomers bought their own personal copies. For years teachers
of galactic-structure courses assigned it for supplementary reading. The first printing of 2500 copies sold out completely, and a second printing in 1968 sold nearly eight hundred more copies. Then in 1975 the MIT Press reissued it as a paperback. It had a great influence on research astronomers in the United States and abroad for almost two decades.\textsuperscript{150}

Besides his papers and his book, Baade’s legacy included not only his two Ph.D. thesis students, Sandage and Arp, but a host of European and American astronomers he had taught, trained, inspired, and guided. They were the primary leaders in the next two decades of research on stellar and galactic evolution. Although his friend and collaborator Oort was the moving spirit in creating ESO, Baade’s support, realistic advice and will to make it succeed were crucial, and made it part of his legacy too. ESO’s first three directors, Heckmann, Blaauw, and Woltjer, were all very close to Baade. At Mount Wilson and Palomar Observatories, the stellar spectroscopist Babcock succeeded Bowen as director but after him Maarten Schmidt, another Oort and Baade protégé, became the last director of the joint observatories.

Baade was an outstanding scientist. He combined an excellent mind with excellent training and experience in Germany and the United States. He had tremendous drive, and lived for astronomical research. Dedicated to doing science himself, he avoided all administrative posts, and hardly ever agreed to serve on any committees, except those directly concerned with operating his own observatory. Yet he spent hours, days, and weeks in scientific discussions with individuals and small groups of research workers, and in lecturing to advanced students and working scientists. Baade had tremendous personal magnetism, and by the end of his life he had had a direct, stimulating effect on just about every astronomer in America, Germany, and Australia, as well as on many in Holland and England. Practically every one of them loved him, or at least admired him at the same time they were drawn to him, and he used that personal attraction to communicate his scientific ideas effectively to other scientists. Stellar evolution and population research dominated astronomy for two decades or more; today galaxy evolution, based on those fields, including the ages of globular clusters and other systems, is one of the most important research topics. Although Baade’s name is not particularly well known to astronomers today, his ideas still have a very important place in the research they are doing.

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Bernard Schermutzer (University of Wisconsin Archives), Helen Knudsen (Caltech Astrophysics Library), Owen Gingerich (Center for Astrophysics), Albert E. Whitford (Madison), George V. Coyne, S.J. and Sabino Maffeo, S.J. (Vatican City), David DeVorkin (National Air and Space Museum), William G. Tifft (Arizona), Ronald D. Ekers (Australia Telescope), Jeremy R. Mould (Mount Stromlo and Siding Springs Observatories), Butler Burton (Leiden), Dieter Reimers (Hamburg), Roswitha Rahmy (CERN), I. Khan (Göttingen), and Ray Bowers and Maxine Singer (CIW), as well as to the late Martin Schwarzschild and Lyman Spitzer, Jr (Princeton).
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REFERENCES

ABBREVIATIONS USED
The following are additional to those listed on pp. 312–13 of vol. xxviii.

(a) Archival Sources

ANU  Australian National University, Canberra, A.C.T.
ATNF  Australia Telescope National Facility, Epping, N. S. W.
CIW  Carnegie Institution of Washington, Washington, D.C.
      Institution Files
GO  Göttingen Observatory, Germany
HHL  Mount Wilson Observatory Collection, Henry E. Huntington Library, San Marino, California
      Henrietta H. Swope Papers

(b) Individuals

HB  Johanna ("Hanni" or "Muschi") Baade
EBB  E. B. Biesecker
JWB  John W. Boise
MGB  McGeorge Bundy
LG  Leo Goldberg
JLP  Joseph L. Pawsey

80.  WB to CPG, 3 Feb., 27 Mar. 1947, OGC.
81.  WB to [HH]S, 29 Feb. 1948, HHL.
82.  E. Gaviola to WSA, 9 June, 10 Oct. 1942, WSA to Gaviola, 22 June, 21 Oct. 1942, HHL; WSA,
     "Algunas posibles investigaciones para un observatorio austral", Revista astronomica, xiv
     (1942), 243–6.
83.  E. Gaviola to ISB, 26 Nov. 1946, EH to Gaviola, 3 Jan. 1947, [WB] to Gaviola, [–15 Feb. 1947],
     ISB to Gaviola, 11 Jan., 5 Apr. 1947, Gaviola to ISB, 25 Mar. 1947, HHL.
84.  HS to WB, 17 Mar. 1948, 10 Feb. 1949, 21 Nov. 1950, WB to [H]S, 9 Feb. 1949, HCO; WB to
85.  HS to AHJ, 28 Oct., 25 Nov. 1949, AHJ to HS, 18 Nov. 1949, HS to WB, 11 July 1950, HCO.
86.  WB to [JH]O, 2 June 1952, AIP.
87.  F. Hoyle, "Report of meeting" [of IAU Commission 28], Transactions of the IAU, viii (1954),
     397–9; WB, "Basic facts of stellar evolution", ibid., 682–9.
88. S. Chandrasekhar to NUM, 10 Nov. 1952, NUM to Chandrasekhar, 12 Nov. 1952, SLO.
89. Science Service, “Yardstick of the universe needs to be shortened”, Future release, 16 Dec. 1952, HCO; G. W. Gray to ISB, 5 Jan. 1953, HHL; C. A. Federer to OS, 17 Jan. 1953 (two letters of same date), BL. See also F. Hoyle, Home is where the wind blows: Chapters from a cosmologist’s life (Mill Valley, 1994), 263, who characterizes Shapley’s action as plagiarism without naming him. Hoyle was the acting secretary who took minutes of what Baade, Shapley and other speakers said at the IAU Commission 28 meeting.
90. WB to S[G], 13 Jan. 1953, HCO (there are three earlier, more extreme drafts of this letter, two dated 9 Jan. and one 10 Jan., HHL); WB to DHM, 29 Jan. 1953, GO; WB to [GPJK], 30 Jan. 1953, GPK to WB, 11 Feb. 1953, UAL; WB to [GW[G], 2 May 1953, HHL; WB to [GEJK], 14 June 1954, SLO.
93. ISB to GWG, 9, 27 Jan., 10 Feb., 16 Mar., 18 May, 1 June 1953, GWG to ISB, 5 Feb., 6 Mar., 21 Apr., 11 May 1953, WB to [GWG], 2 May 1953, GWG to WB, 8 May 1953, HHL.
94. A. Behr, “Zur Entfernungskala der extragalaktischen Nebel”, AN, cxvii (1951), 97–104; GG to [WB], 12 May [1951], GO.
98. WB to [HJS], 29 May 1940, WB to [LG], 23 Apr. 1943, HCO; WB to [JH]O, 5 Dec. 1952, AIP.
99. WB to [JH]O, 7 Jan. 1954, AIP; LG (ed.), Symposium on astrophysics (Ann Arbor, 1953) [mimeographed notes of the lectures at the Michigan symposium, prepared by the participants]; LG, “The Observatory, Ann Arbor” [Annual report for 1953–54], AJ, lix (1954), 436–50; O. Gingerich, “The summer of 1953: A watershed for astrophysics”, Physics Today, xlvi (1994), Dec. issue, 34–40. The last of these is an excellent article by a participant in the Michigan symposium, which gives the full flavour of the experience. I was also a participant and was inspired greatly by Baade’s lectures, as I had been earlier by his invited paper at the dedication of the Michigan Schmidt telescope in 1950 (ref. 32).
100. WB to [OJS], 7 Feb., 1 Apr., 26 Apr., 1 June, 1954, OS to WB, 26 Feb. 1954, BL; [WB], “Hitchcock Lectures” [spiral notebook of his handwritten notes for the lectures], 4–24 May 1954, WB to ISB, 9 May 1954, HHL.
101. W. G. Tifft, “Baade — Extragalactic nebulae — Ay 212”, 61 pp. handwritten notes (rewritten soon after the lectures from rough notes taken in class), 23 Jan. —7 Mar. 1956. I am very grateful to Dr Tifft for lending me a copy of these notes, which are in his possession. I sat in this course for all the lectures I could, and the remarks on Baade’s teaching skills are based on
my own observation. They were corroborated by the former students in the course with whom I have been able to discuss them in 1996–97, Drs Tiff, Walter K. Bonsack, and Ray L. Newburn.

102. WB to [JHO], 20 Jan. 1956, AIP.


116. A. Blaauw, ESO’s early history: The European Southern Observatory from concept to reality (Garching bei München, 1991). This excellent book is the source for much of the section on ESO; however, Dr Blaauw was naturally not aware of Oort and Baade’s earlier discussion of this concept.

117. H. W. Babcock to J. M. Beckers, 22 Jan. 1993. I am greatly indebted to Dr Babcock for telling me of his recollection, and sending me a copy of this letter. He thought their conversation was “in 1953 (or possibly in the preceding year)”; since Oort was not in Pasadena in 1953 but was for nearly three months in early 1952 it must have been then. At least partial corroboration is in WB to [JHO], 14 Oct. 1957, ULL, a retrospective letter Baade wrote after learning from Oort that the ESO project would go ahead. In it Baade said he was enthusiastic as he was when they discussed it on the automobile trip from Amsterdam (when Baade arrived by air in May 1953)
through the tulip fields to Leiden. Its seems unlikely that this topic would have come up spontaneously under those circumstances, but quite natural if Oort were reminding Baade of their early discussion, and telling him that now was the time to go ahead with it.

118. WB to the Higher Educational Authorities in Hamburg, [–1 July 1927], HHL.


122. La structure et l’évolution de l’univers: Onzième Conseil de Physique tenu à l’Université de Bruxelles du 9 au 13 juin 1958 (Brussels, 1957); J. Mehra, The Solvay Conferences in Physics: Aspects of the development of physics since 1911 (Dordrecht, 1975); [WB], “Vortrag in Brussel: The basic observational data” (handwritten notes, in English, for his report at the Solvay conference), [=1 June 1958, HHL.

123. “Program of the One Hundredth Meeting of the AAS, ..., Madison, Wisconsin, June 29–July 2, 1958”; [Madison] Wisconsin State Journal, 30 June, 1 July 1958. I was at the AAS meeting in Madison and remember Baade’s Russell Lecture well. At midnight on 30 June he, Albert E. Whitford, who at that same instant was officially transferring from the University of Wisconsin faculty to the University of California (as director of Lick Observatory), and I, moving from Caltech to Wisconsin (as an assistant professor), were talking over a few quiet glasses of beer in Baade’s motel near the campus.

124. JW B to WB, 15 Apr., 31 July 1958, WB to CIW, 5 June 1958, CIW.


127. WB to [HH]S, 6 Dec. 1958, WB, “Harvard Lectures (Fall Term 1958–59)” [a spiral notebook containing his detailed notes for the first 19 lectures, plus outlines on sheets of paper for the rest], HHL; WB, Evolution in stars and galaxies: Lectures delivered at the Harvard College Observatory, transcribed from tape recordings by Richard S. Rodman (Cambridge, 1959) (typed, mimeographed notes, 432 pp., later published as a facsimile (Ann Arbor, 1980)).


129. BBJ to WB, 28 May 1958, HHL; WB to Registrar, ANU, 4 June 1958, ANU; JLP to WB, 11 Nov. 1958, WB to JLP, 13 Dec. 1958, ATNF.


132. WB to [JH]O, 29 June 1959, OLL; WB to H[HS], 17 July 1959, OLL, WB, “7 Lectures at the Australian National University July 2–Aug. 13, 1959” (his notes), HHL. The attitudes of the young astronomers and graduate students are based on my interviews with Roger Bell and John Whiteoak (students) in 1994 and on a memo from Alex Rodgers (research associate), written in May 1995.

133. “Visit to Western Australia by Mount Stromlo astronomers and visitors from the United States” (press release), 24 July 1959, W. G. Tiff Collection; WB to [EB]B, 26 May 1959, CIW; J. B. Whiteoak, “Student memories of Bart Bok, an astronomical godfather”, Proceedings of the
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136. J. J. Nassau to WB, 16 Sep. 1958, GO; “Astronomie und Astrophysik, Vorlesungssankungung für das Winter-Semester 1959/60” [mimeographed one-page course announcement including WB, “Entwicklung von Sternen und Sternsystem”], “Göttingen, Klassifikation der Sternsystem nach Typus” [Baade’s notes in German for only one lecture, closely following his notes for his second lecture at Harvard in the autumn of 1958], HHL.


138. [H]B to [J]HO, 5 Feb. 1960, ULL; O. Heckmann to LRS, 18 July 1960, P. ten Bruggencate to [LRS], 28 July 1960, SLO. The last two letters were originally written in German to Baade’s old friend the Marine chaplain, who understood the language well but was not familiar with medical terminology. Schneider sent his own translations into English to Mayall (and Bowen), and these translations are all that I have seen. They are the main sources for most of the details of Baade’s illness and death.


140. DHM to MGB, 24 Nov. 1959, MGB to DHM, 1 Dec. 1959, DHM to WB, 7 Dec. 1959, HCO.

141. JHO to WB, 15 Feb., 14 June 1960, ULL; W. A. Fowler to WB, 6 May 1960, GO.


143. [JHO] to HB, 12 July 1960, ULL; HHS to W[B], 6 Mar., 2 Apr., 2 June, 1960, H[HS] to [HB], 5 May, 6 July 1960, HHL.


