WALTER BAADE, OBSERVATIONAL ASTROPHYSICIST, (3): PALOMAR AND GÖTTINGEN 1948–1960 (PART A)

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1. Introduction

Walter Baade’s birth, education, training and early research experience in Germany, followed by his years working with the 100-inch Mount Wilson reflector, led him to the discovery of the two stellar populations during the Second World War.1 His two 1944 papers, and the invited lecture he gave at the end of 1947 at the American Astronomical Society meeting in Ohio, made him a well known, highly respected figure among research astronomers. Baade was then waiting impatiently for the completion of the Palomar 200-inch reflector, so that he could exploit the ideas and concepts he was rapidly developing on the basis of his discovery.2 The rest of his life was to be a series of triumphs as, with this largest telescope in the world, he led the way in pushing our knowledge of the universe out in space and back in time. Astronomers had finally begun to learn how stars change as they age, or “evolve”. That is the reason why Baade’s scientific career is so important to us today.

From the end of the Second World War until he died, Baade’s most important scientific colleague in the world was Jan H. Oort. This reserved, intense, ascetic Dutch astronomer, some seven years younger than Baade, was in many ways a striking contrast to the older, outgoing, colourful German, who did not mind a few drinks and a good cigar on occasion, and who preferred long vacations whenever he could afford them, but they shared a consuming interest in understanding galaxies. Oort, the son of a physician, had studied astronomy at Groningen University under the outstanding Dutch astronomer of the previous generation, Jacobus C. Kapteyn. After finishing his academic courses and assisting in the Kapteyn Laboratory for one year, Oort had travelled to America and worked at Yale University with Frank Schlesinger in 1922–24, becoming an expert in astrometric research. In 1924 he was appointed a staff member at Leiden Observatory, then directed by Willem de Sitter, and spent the rest of his life there. Oort’s doctoral thesis, completed in 1926, was on the high-velocity stars, which Baade later recognized as important members of his population II. Almost all of Oort’s research was in galactic structure. Baade had first met him in Europe, and came to know him well in 1939 at the McDonald Observatory dedication and during the middle-aged Dutchman’s subsequent visit to Pasadena. After it they had corresponded, especially about supernovae, but in the autumn of 1941 the mails between America and German-occupied Holland were interrupted, and Oort’s last letter to Baade about the Crab nebula and distant Cepheid variables was returned to him.3

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That same year, Oort, one of the Dutch professors who publicly protested against the Nazis' dismissal of their Jewish colleagues, many of whom banished to concentration camps, had to leave Leiden and go into hiding in the country. There he was able to continue theoretical research, although in the harsh winter of 1944–45 he, along with most of his compatriots, came close to starving as the Allied armies approached and fought their way through the Netherlands, and the retreating Wehrmacht left a wake of destruction behind it. In the spring of 1945, Oort was rescued by Gerard P. Kuiper, the Yerkes astronomer who was returning to his native country as a naturalized American citizen and who was close behind the lines, a member of the ALSOS scientific and technical intelligence mission. Kuiper arranged for Oort to spend a few weeks in England, where food was more plentiful, before he returned to Leiden as full professor and director of the observatory.

Through Kuiper and through Bertil Lindblad in Sweden (which had sat out the war as a neutral, poised uneasily between Germany and the Soviet Union), the Dutch astronomer had obtained copies of Baade's papers on the Crab nebula and on his identification of population II in the Andromeda galaxy and its companions. At the end of August 1945, as soon as civilian mail between Europe and America went through again, Oort sent Baade a long letter, congratulating him on this work, and asking him many questions about it. He commented on the ideas Baade had expressed for further research, particularly on trying to identify the spiral arms of our Galaxy as concentrations of population I stars. Oort told of his own theoretical work, groping toward understanding the internal structure and kinematics of gas shells thrown off by novae and supernovae, as they expanded and interacted with interstellar gas and "dust" particles. His greatest joy, he said, was that at long last he was again receiving astronomical research publications from America.

Baade, greatly interested in Oort's analysis, replied with a long report on the observational results he had obtained on several nova shells, mostly still unpublished. He described the interactions in very physical terms, although completely qualitatively. Oort he knew could generalize them, and work them out mathematically. Baade also briefed Oort on his latest findings on the distant Cepheid variables in the Milky Way, and told him of the difference between the population I "classical" Cepheids, and the population II W Virginis variables, which mimicked them but had different luminosities. With the help of the spectroscopists Alfred H. Joy and Philip C. Keenan he was beginning to distinguish between them; within a very few years they would lead him to doubling the size of the universe. Baade and Oort were already deep into research discussions, supplementing one another's areas of expertise. They both referred obliquely and gingerly to the war in their first exchange, then dropped the subject; their common scientific interests overcame whatever resentment Oort may have felt toward Baade as a representative of German "culture".

Oort had been awarded the Royal Astronomical Society's George Darwin Medal, and was invited to come to London to receive it and present the Darwin lecture in...
the spring of 1946. It dealt mostly with the interactions between gas, stars, and dust about which he had written to Baade. Oort borrowed direct photographs and obtained unpublished data from the Mount Wilson astronomer to use in it. He sent Baade a written copy of his paper soon after presenting it; the latter was entranced as he read it. He congratulated Oort on it, saying it would have a tremendous influence in guiding future research, but also sent him detailed comments, suggesting additional studies and offering to cooperate with him. Baade told him all his unpublished results on the concentration of RR Lyrae variables at the centre of our Galaxy. By then they were on a “Dear Oort” and “Dear Baade” basis, as close as senior scientists of their time allowed themselves to get with their contemporaries, and for the rest of Baade’s life they remained intimate friends who inspired one another’s research.

Toward the end of 1947, Oort came to America for the first time since the war. He arranged to visit Pasadena for several long discussions with Baade. No doubt they went over Baade’s manuscript for his invited lecture at the Ohio meeting of the American Astronomical Society. Then Oort left for McDonald Observatory for a session with the 82-inch reflector, but after the meeting Baade wrote to him about the discussions at Columbus and the new information he had gleaned there. Oort, the leading figure in Dutch postwar astronomy, spent considerable effort on sending the best young prospects from his country to the United States for seasoning, making sure they would meet Baade and discuss science with him there. The Leiden director always made it clear that he wanted them back in a year or less; Baade, who favoured longer stays, in those early years was particularly impressed by the young Adriaan Blaauw and Hendrik van de Hulst and encouraged them to seek the best research opportunities they could, whether that meant staying in America as he had or not. Oort believed in strict control; Baade always favoured what he called “rein Spass” (“pure fun”, though in English he often wrote “a pure thrill”), by which he meant following his scientific ideas wherever they lay.

2. The 200-inch Hale Telescope

On 30 December 1947, Baade’s invited lecture on the two stellar populations alerted the members of the American Astronomical Society to an exciting new idea. His talk inspired many of them to follow it up in their own subfields of stellar and nebular research, but he did not rest on his laurels. He continued observing with the 100-inch telescope, exploring M 31, the Andromeda galaxy, the nearest representative of a spiral galaxy, to learn as much as he could about the distribution of population I and II objects within it. It covers a large area on the sky, which he carefully divided up into fields, each of which would fit onto a single exposure, and he systematically photographed them with various emulsion and filter combinations designed to reveal hot blue stars, diffuse emission nebulae (soon to become known as H II regions) and “dust” (recognized by its extinction of stars behind it). A highly skilled observer, he used only the nights of best seeing (which at Mount Wilson
Fig. 1. Speakers at symposium on the structure of our Galaxy, University of Michigan, 1950. Harlow Shapley is third from left in the front row, then Walter Baade, Jason J. Nassau (a Baade look-alike), and Leo Goldberg. In the rear row William W. Morgan is third from left, Nicholas U. Mayall fifth, Rudolph Minkowski sixth, and Joel Stebbins seventh (courtesy of the Mary Lea Shane Archives of the Lick Observatory, UC-Santa Cruz).
occur most often in the autumn, at which time M 31 is well placed in the sky), when
the images were smallest and the starlight most concentrated. A consummate dip-
emat, he enlisted the head of the Eastman Kodak research laboratory, W. F. Swann, in
his quest for the most sensitive, stable red-sensitive photographic plates that were
essential for his program.9

By the autumn of 1948 Baade knew he was close to finishing what he could do
with the 100-inch on M 31. As he inspected and analysed the plates, he realized
more and more clearly that the outstanding feature of a spiral galaxy, its spiral arms,
were primarily an “organization” or structure of dust, and that the supergiant stars
always occurred in close association with the dust.10 Baade summarized this con-
tect and the evidence for it in detail at the symposium at the University of Michigan
in 1950, held in connection with the dedication of its new 24-inch Schmidt tel-
escope (Figure 1).

By 1948 he had also already worked out which populations novae and superno-
vae belonged to, by going back over all the data he had on these briefly brilliant
types of stars. Novae, which occur frequently only in the central region of M 31, our
Galaxy, and other nearby spirals, and only infrequently in the Magellanic Clouds,
which Baade thought “consist of an almost pure population I”, were clearly popula-
tion II objects. Rudolph Minkowski had earlier divided supernovae into two types
on the basis of their spectra, and Baade had found that their light curves could also
be classified to these same two types; now he found that supernovae of type I occur
in all types of galaxies from elliptical to late-type spirals, and therefore belong to
population II, while supernovae of type II occur only in spirals, and hence are popu-
lation I. He was using the population concept to bring order to apparently unrelated
observational results, but he still did not grasp the physical reason behind it.11

As he worked with the 100-inch, Baade was simultaneously planning for the
200-inch, which he knew would soon double his effective observing range by quad-
rupling his light-gathering power. He would devote himself to direct photographic
work on galaxies, but he knew he would need help with spectroscopy and the quickly
advancing photoelectric photometry. Milton L. Humason, the outstanding spectroscopic
observer of faint nebulae and galaxies, would devote himself to measuring redshifts for Hubble’s cosmological program, while Minkowski preferred his
own more arcane nebular investigations to observing at Baade’s behest. Hence Baade
encouraged and stimulated Nicholas U. Mayall, at Lick Observatory, to continue
and expand his spectroscopic measurements of the radial velocities of globular clus-
ters (pure population II objects) in our Galaxy, and of H II regions (pure population
I) in M 31. Mayall had only the ancient 36-inch Crossley reflector on Mount Ham-
ilton at his disposal, but his very fast spectograph, optimized for his work, his
exceptionally keen eyesight (making it possible for him to find and observe these
faint objects) and his unbounded enthusiasm made him a valuable ally. Besides,
Lick Observatory had the go-ahead to build a 120-inch reflector, which would be
second only to the 200-inch when completed. This was then expected to occur only

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a few years in the future, although in the event it did not come on line until the year
before Baade's death. Baade kept Mayall fully informed on all his latest results,
diplomatically suggested program after program he could do, and helped him over
whatever rough spots came up in them.\textsuperscript{12}

The Baades were childless, but the astronomer, in his visits to Germany, had
loved to play the role of the friendly uncle to his brother's and sister's children.
Now he reenacted the part, becoming a sort of surrogate older brother to Mayall
and his wife, and uncle to their children, especially when they took vacations together
at Oceanside, on the Southern California coast not far from Palomar and San Diego.
There Baade also formed an unlikely friendship with a Catholic priest, Lawrence R.
Schmieder, who was a chaplain at the nearby Camp Pendleton Marine Base. "The
Padre" was fluent in German as well as English, and a confirmed astronomy buff;
he loved to sip martinis and grill steaks for Baade, Mayall and their wives on the
beach, as he pumped them for their latest insights on the universe, to supplement
his own wide reading in popular scientific and news magazines. Baade, a "hard
boiled protestant" in his own words, liked to claim he had a Lutheran theological
background and evidently enjoyed friendly disputations with the worldly priest.
Baade's dog Li ("Lee" to Americans), a powerful, threatening looking chow which
responded only to German, was an important part of these vacations, always firmly
under control when the children were present.\textsuperscript{13}

Joel Stebbins, the great pioneer of photoelectric photometry, who had done so
much of his observing at Mount Wilson Observatory, was close to retirement. Baade
wanted to bring in one of the next generation of his students and protégés, Albert E.
Whitford, Gerald E. Kron, Olin J. Eggen, or Harold L. Johnson, to make the accu-
rate measurements of the magnitudes of faint stars that the 200-inch would make
possible. Ira S. Bowen, the new director of Mount Wilson and Palomar Observato-
ries, was less enthusiastic for them. Baade respected Bowen as an expert in applied
optics, the perfect director to get the big telescope into operation, but felt that his
one shortcoming was that he would always think a Caltech Ph.D. the best man for
any job, even better than outsiders who had already proven themselves.\textsuperscript{14} Baade
helped all these former University of Wisconsin students and young faculty mem-
bers gain access to the 100-inch, schooled them in his type of galactic structure
problems, and promised to help them get observing time with the 200-inch when it
was ready. He knew that with it they could measure the faint magnitudes that would
be necessary to solve these problems.\textsuperscript{15}

The equally new director at Lick, where Kron and Eggen became staff members
along with Mayall, and where Johnson had been a graduate student, was C. Donald
Shane. A long-time teacher on the Berkeley campus, at Lick he took over the new,
20-inch wide-field "astrograph" or Ross camera and its research program. It was
designed to measure the proper motions of faint stars with respect to the reference
system fixed in the universe provided by the many faint, distant galaxies, a program
that would take years to pay off. Baade encouraged Shane to count galaxies on the
plates taken for it, and thus gain some quick results on the patchy distribution of interstellar dust in our Galaxy, and on more distant clusters of galaxies in the universe. Bäade flattered Shane with his attention, promoted informal meetings of the astronomers working on nebular and galactic research at the two observatories, gave him excellent research advice, and gained the Lick director's cooperation and support.16

By November 1947 Bowen and the members of the Observatory Council judged the 200-inch mirror close enough to its correct figure to be sent from the optical shop on the Caltech campus to Palomar. There Don O. Hendrix, the skilled young Mount Wilson optician who had taken over the work on the mirror, touched up its surface and aluminized it. Then it was put in the telescope and on 21 December Hendrix and Bowen made the first visual tests with it on stars, the "first light" for the 200-inch. The following night they began photographic tests with a full-size Hartmann screen, a procedure involving detailed measurements to determine the exact shape of the reflecting surface, which Hendrix could then further improve. Baade was anxious to use the telescope, but predicted it would not be ready until the following autumn, when the best seeing would be available for the final critical tests.17 The Carnegie Institution of Washington and Caltech bigwigs decided the telescope, which was to be named for George Ellery Hale, would be dedicated in the summer of 1948. The AAS and the Astronomical Society of the Pacific were to hold a joint meeting in Pasadena in June, and then move on to Palomar for the one-day dedication ceremony. Astronomers all over America were anxious to see the telescope, and the meeting would be a big one.

Otto Struve, president of the AAS, and Harlow Shapley, its recent past president, first planned to schedule a symposium of three invited lectures on galactic structure and dynamics for the Pasadena meeting. Hubble was to speak on the structure of galaxies from the observational point of view, Mayall on their internal kinematics from his spectroscopic radial-velocity measurements, especially of H II regions in M 31 and M 33, and Subrahmanyan Chandrasekhar, of Yerkes Observatory, on their dynamics, from the theoretical point of view. However, the head of the local organizing committee for the ASP, Seth B. Nicholson, a Mount Wilson solar astronomer and planetary-orbit expert, feared that Hubble, a master at attracting personal publicity, would steal the show. Probably aided by covert pressure from Walter S. Adams, the former Mount Wilson director, who by then no longer respected Hubble as a scientist and warned Bowen against him, and from the new director himself, who was president of the ASP and wanted the men who had built the telescope to be recognized, that symposium disappeared from the program.18 Instead, John A. Anderson, the head of the 200-inch project from the beginning, spoke on the optics of the 200-inch, Bruce Rule, the Caltech engineer, described the engineering aspects of the telescope, and Baade spoke on "a program of extragalactic research" for it.

They gave their talks in the 200-inch telescope dome on 1 July 1948, before
some 350 members of the two astronomical societies and their guests, seated on folding chairs. Bowen opened the session by welcoming the visiting astronomers, and then demonstrated the motions of the giant telescope, which he controlled with push buttons from the night assistant’s console. Then he presided as Anderson, Rule and Baade gave their talks. Baade began by emphasizing Hubble’s crucial part in starting “the great attack on the structure of the universe” twenty-five years earlier, but then got down to brass tacks. He stressed the important work that would now have to be done to make real progress, setting up accurate magnitude scales down to the faintest limit to which the Hale telescope could be pushed, measuring Cepheid variables to this limit, using them to calibrate the absolute magnitudes of the brightest stars which could then be used to extend distance measurements to more distant galaxies, calibrating RR Lyrae variables and population II Cepheid variables, comparing them with population I Cepheids and thus “check[ing] ... if our basic assumptions are correct”. It was very forward-looking and made a deep impression on the many astronomers who had travelled across the country to see the 200-inch and hear about the problems it would solve.\(^{19}\)

Sadly, Hubble had never caught up on research after his return from Aberdeen Proving Ground at the end of the Second World War, and after his disappointment in not being named director to succeed Adams.\(^{20}\) He was so out of touch, or possibly so self-centred, that in a popular talk he gave in 1946 in Los Angeles before the Sunset Club, an establishment dining and discussion club, he began by saying, “There was little or no real progress in science during the war (except perhaps in the fields of medicine and surgery). The research men are returning to find the same old problems just about as they were left.” He evidently had not read Baade’s two papers, published during the war, on the discovery of the two populations, or if he had read them, he had not understood them. Every astronomer even slightly interested in galaxies or galactic structure, and every astronomer at Lick, Mount Wilson, and Yerkes Observatories (except Hubble), was well aware by then that Baade’s two papers had fundamentally changed their science.\(^{21}\)

Getting the 200-inch into full operation took even longer than Baade had predicted. The tests confirmed Bowen’s and Rule’s fears that the support system, designed in 1935 to keep the figure of the huge mirror from ‘flexing’, or distorting slightly under its own weight, suffered from too much friction to fulfil this task completely. They updated the design, tested a pilot model, and put the new system into operation in the autumn of 1948. It greatly improved the performance of the mirror, but the tests were then sensitive enough to confirm that its shape deviated from a perfect paraboloid, having a “turned-up edge”. Also they found that the mirror figure suffered from thermal distortions when the night-time temperature changed. Hendrix touched up the figure, working in the dome and testing each improvement on stars, a tedious, but necessary, process that ate up time. Bowen and Rule added thermal insulation at the sides and back of the mirror, plus electrical fans to help equalize the temperature within it, and by January 1949 the figure was
reported to be “remarkably fine” — but still needing improvement! Tests continued all through the summer, as the atmospheric conditions and seeing gradually improved, and the measurements thus more precise. Finally in September 1949 Bowen judged the mirror as nearly perfect as it was possible to make it, Hendrix aluminized it in October, and on 12 November 1949 regularly scheduled observations began and continued in each dark of the moon. The big coudé spectrograph that could be used in bright moonlight was not yet ready, so the period near full moon was devoted to further testing. The winter weather revealed that thermal distortion still occurred, though at a lower level, but more insulation solved that problem too by March 1950.22

Bowen, a quiet, diplomatic leader, had involved Hubble strongly in the testing process. As the senior astronomer, he went to Palomar to test the telescope in actual use, obtaining direct photographs of many of the most famous, spectacular nebulae, star clusters, and galaxies, as well as critical test plates of stars, beginning in January 1949. The newspapers were clamouring for information, and he enjoyed the attention. In the spring he prepared a short paper in which he published the “first photographs with the 200-inch Hale telescope”. He gave it his seal of approval, writing that these pictures “confirm[ed] the most optimistic predictions of its designers” and that they had demonstrated that it could reach stars 1.5 magnitudes fainter than the limit of the 100-inch. Further improvements in definition could be expected, he stated, but the telescope had already proven itself a great success. The spectacular reproductions of six of his photographs must surely have convinced any sceptical readers.23

Unfortunately the great observational cosmologist was never to get the chance to do real research with the 200-inch. In July of that same year, while on a fishing vacation in Colorado, Hubble suffered a heart attack and nearly died. He pulled through, but when he was finally able to return to his office in Pasadena, Baade reported privately that he looked “awful”, tired, drawn and easily depressed. A year later Hubble was able to get to Palomar and observe with the 200-inch again, but his creative juices were gone, and he had too little time left to regain them. On 27 September 1953, nearly home in his wife’s car from a morning at the office, he collapsed and died of a massive stroke. In an emotional letter to Hubble’s widow, Mayall wrote that her husband, more than anyone else, had stimulated and counselled him in the field of research he had made so much his own. In truth, however, after about 1939 Baade, not Hubble, had played that role to him, as he had to American astronomy in general.24 After Hubble’s death, Baade was named to take his place on the Observatory Council, the small group of senior Mount Wilson and Caltech scientists which Bowen chaired and which was charged with advising him.

Baade had been taking an active part in the 200-inch tests from the beginning. He had provided the only quantitative datum in Hubble’s 1949 paper: the actual magnitude limit his plates had reached. Baade had drawn up a simple plan for Hubble to take a series of exposures of Selected Area 57 with the 200-inch mirror diaphragmed
to various apertures, and then had used the photoelectric magnitudes he had arranged for Stebbins and Whitford to measure with the 100-inch to calibrate Hubble's plate. Baade's reduction, analysis and extrapolation gave the magnitude limit of 22.6 or 22.7 that Hubble had implicitly stated.25 After Hubble's first heart attack, Baade took all the most important test exposures with the 200-inch. These gradually tapered off, but months after the telescope went into full research operation in January 1950, he still had occasional spurts of obtaining test plates for Bowen to analyse. By January 1951 they were both convinced it was in tip-top shape. Baade found it "wonderful to work with", and was vigorously pushing his program on local-group galaxies to the new faint-magnitude limit that they had all worked so hard to achieve.26

With more data coming in every month, Baade clearly needed help to reduce them, particularly to measure the magnitudes of the stars in these galaxies. Mount Wilson Observatory had a long tradition of being badly understaffed with assistants. Young visiting astronomers, many of them from abroad, had done some of the reduction work, usually only for a year or two until they had gained enough experience to get a better job or return to a higher-level post in their native countries. A few women assistants were on the permanent staff, dividing their time between helping various astronomers. Bowen recognized Baade as the most important research worker at the observatory, and particularly after Hubble's activity slackened, tried to give him all the support he needed. In 1951 Baade briefly had his own first full-time assistant, Sister Mary Therese, a nun who taught astronomy at Mundelein College in Chicago. Supported by her order, she came as a volunteer, but as she had little previous experience and stayed only a few months, her help was largely symbolic. However, the work she did for him apparently awakened Baade to how useful another pair of hands and eyes could be. By January of the following year he had found Henrietta H. Swope in New York City, a much more skilled, experienced worker, and she was driving across the country with her sister-in-law, the Hollywood actress Dorothy McGuire, to begin work down the hall from Baade. She was to be his trusted assistant, friend and confidante until the day he died, and continued his work after his death.27

"Miss Swope", as she was universally known in those sexist days, had done her undergraduate work in astronomy at Columbia, graduating in 1925 and going on to earn a master's degree at Radcliffe in 1928. Then she began work as an assistant at Harvard College Observatory. It was commonly reputed to have an even lower salary scale for assistants than Mount Wilson, but whatever she was paid could hardly have mattered to her, for her father Gerald Swope, the president of General Electric, was one of the richest men in America. In January 1931 he and his wife took Henrietta on a trip to Los Angeles, and arranged for a weekend visit to Mount Wilson. Henrietta was quiet and somewhat shy; her father was not. He ordered his Los Angeles manager, who organized the visit, to emphasize that he would prefer to go up the mountain with Albert Einstein (then visiting Pasadena), who, he said, "had entertained
him on one of his recent trips to Berlin'. He had to be content with Adams, then director of the observatory, instead.

At the HCO, Henrietta Swope worked chiefly on measuring photographic magnitudes for Shapley just as she would in Pasadena for Baade years later. Shapley encouraged her and made her feel a member of the observatory family. He did the same for all the many women assistants, though he probably devoted extra attention to Swope, for her father was contributing several thousand dollars a year to Harvard research. She lived unostentatiously and did not flaunt her wealth, but her relatives and her ability to finance whatever trips she wanted to make, such as to the 1942 dedication of Tonantzintla Observatory in Mexico, could not escape notice.

During the Second World War Swope left her Harvard job for war work, first at the Radiation Laboratory, the radar development centre on the MIT campus, and then for four years at the Navy Hydrographic Office in Washington, working on LORAN, a radio navigation system. At the end of the war she longed to get back to Harvard but Shapley, preoccupied with national and international humanitarian projects, was not particularly attentive, nor positive about the future of the variable-star research Swope loved. In 1947 she accepted a teaching and research job in astronomy at Barnard College, the women's sister-institution of Columbia University. She tried to do various variable-star projects on her own there, but she missed the family atmosphere of the HCO and her many friends on its staff.28

Baade had certainly read Swope's variable-star papers, especially as many of them dealt with the galactic-centre region. No doubt he had met her on visits to Harvard and at astronomical meetings. Somehow, one or the other of them got the idea that she might work with him, and by November 1951 she had definitely decided to leave Barnard for the assistantship with Baade. Shapley was surprised and hurt; he liked to think the HCO was the ideal place for anyone. Before long he realized that Swope would not come back; then he began trying to ferret out from her what Baade was doing. But she did not answer his queries; she was now a loyal Mount Wilson and Palomar Observatories staff member, and had all the variable-star work she could handle, at the very forefront of galactic-structure research. With her assistance, Baade was plunging on with the largest telescope in the world.29

3. The 48-inch Schmidt Telescope

The "Big Schmidt" for Palomar, though like the 200-inch it was twice as large as the largest telescope of the same type previously built, was a much more straightforward project. Its primary mirror, 72 inches in diameter, was small enough so that telescope designers and engineers had encountered any problems it might have provided years earlier, and had solved them. The young Caltech engineer Rule had drawn up the final design for the 48-inch Schmidt, taking full advantage of all the previous studies made for it and the 200-inch. The mounting was completed and assembled in its dome on Palomar in the first half of 1948, while Hendrix, the optician, was working on the 48-inch corrector plate in Pasadena. He had made
many smaller correctors in the Mount Wilson Observatory optical shop during the Second World War, for long-range aerial and ground-based military surveillance cameras. He completed the 48-inch corrector, installed it in the Palomar Schmidt, and took the first test plates with it himself in September 1948. Baade was very favourably impressed by the small star images, which had excellent definition over the whole field, better than he had expected. He praised Rule and Hendrix fulsomely, and began using the 48-inch Schmidt himself in January 1949. 30 Years earlier, shortly after he declined the proferred Hamburg directorship, Baade had requested a photograph of the optician Bernhard Schmidt (who had died in 1935) from Richard Schorr, to be hung in an honoured place at Palomar. But much had changed since those prewar days; Baade knew that the Carnegie Institution and most Americans did not like to be reminded that he himself was a German, and even though Schmidt had in reality been Estonian, his picture never went up on the wall in the 48-inch Schmidt dome. 31

Baade was soon repeating his earlier search for the nucleus of our Galaxy, this time with the 48-inch Schmidt and red sensitive plates. Though they showed much fainter stars than his earlier 18-inch Schmidt films of the same region, these new exposures still did not penetrate the dense, optically thick layer of dust between us and the centre. That would have to await longer-wavelength infrared detectors of the future. 32
In the course of this survey, Baade noticed the long trail of an unusual asteroid on one of his wide-field Schmidt plates. Always alert to minor planets, which he liked to joke about but never ignored, he could see it had moved rapidly in the sky during his hour-long exposure. His interest awakened, he took a second plate two nights later, and a third two nights after that, capturing the quickly moving asteroid on each one of them. Baade turned the plates over to Nicholson, the Mount Wilson Observatory planetary expert, for measurement. He, with Robert S. Richardson, computed its orbit from these measurements; the asteroid had a highly eccentric orbit which took it from closer to the Sun than Mercury to farther than Mars. Soon named Icarus, it gave Baade the distinction of being the discoverer of both the most distant asteroid then known, Hidalgo, and this, the nearest.33

His find brought forth a revealing exchange between Bowen and Shapley, director of HCO, who by now was busily engaged in world betterment activities and had little time or energy left for science. He wrote to congratulate Bowen on “the first big discovery from Palomar Mountain”, made some inane remarks about the new object’s orbit, and urged the Palomar director to get out “a proper and full news release” on it. Shapley went on with some wordplay about “Eros and other Erotic minor planets” and “amorous godlets”, referring to the names used for asteroids with small perihelion distances and large eccentricities. He suggested an “angle” for the news release, that the new object was smaller than Palomar Mountain, an estimate based on its brightness. Bowen replied in a letter fifty-seven words long, thanking Shapley, saying that so far as he knew Baade had not yet “given any thought to the name”, and ending that he suspected “that we better wait and see if we can hold on to it before we worry too much about a name for it”. No better contrast could be made between the long-winded, publicity-seeking Shapley and the brief, factual, publicity-shy Bowen.34

That same summer Minkowski published some of the first direct photographs taken with the 48-inch Schmidt. They were exposures, taken in red and blue light, of a large diffuse nebula in Monoceros, surrounding the galactic cluster NGC 2244. The “Big Schmidt” revealed, as no telescope had before, intricate structure in the ionized gas and in the dust clouds and condensations imbedded in it. They were spectacular photographs, just as the 200-inch photographs published two months earlier had been.35

Under an agreement with the National Geographic Society, nearly all the observing time with the 48-inch Schmidt telescope was set aside for a survey of the entire sky north of −27° declination. The results, in the form of photographic prints, were to be made available to observatories everywhere. Once the survey got fully underway in November 1949, following a delay caused by damage in transit to a shipment of the large glass photographic plates used in the telescope, Baade and the other regular observers could only use it very occasionally, but Baade had had a first look at most of his important fields by then.

Oort, just as serious a director and astronomical statesman as Bowen, was
organizing his first postwar international conference, to be held in 1949, and wanted Baade to take part in it. The subject was to be the motions and interactions of interstellar gas clouds, nova and supernova shells, and other “gaseous masses of cosmical dimensions”, the topics of Oort’s 1946 Darwin lecture. He tried again and again to persuade Baade to come, and to bring some of the new, tantalizing 48-inch Schmidt and 200-inch photographs of gaseous nebulae and dust clouds he had heard so much about. It was to be a joint meeting of astronomers, most of whom would bring their data, and theoretical hydrodynamicists, who would try to understand and interpret them. At first Oort planned to hold it in Oslo, but that idea fell through and he changed the venue to Paris. Initially Baade expressed interest, but then said he could not come, blaming his switch on lack of support, presumably from Bowen. More probably one of the real reasons was that after Hubble’s heart attack (which was kept confidential for several weeks), the director wanted Baade to stay to make the crucial observational tests on the 200-inch (as he did). This was compounded by Baade’s scepticism that hydrodynamicists could provide real insights into complicated astrophysical situations, and his now much deeper interest in stellar populations than in nova shells.36 However, Mayall went instead and presented a joint paper, mostly on his own radial-velocity measurements of gaseous nebulae in M 31, but including some of Baade’s direct photographs too. Mayall showed several of Baade’s pairs of matched 100-inch photographs of regions in M 31, one taken in the light of Hα in which the diffuse nebulae stood out clearly in the spiral arms, the other in a comparison band which suppressed the nebular light, to help eliminate stars and star clusters. These reproductions, published in their paper, clearly demonstrated that the spiral arms were outlined by interstellar gas, not by stars in general, as a whole generation of previous observers and theoreticians had imagined.37

4. Stellar Evolution

Baade’s discovery of the two stellar populations in 1944 was a completely empirical result. He had based his work on his previous observational knowledge of the properties of stars in globular clusters, star clouds, the Milky Way, and other galaxies. He was not following up on a theory, nor did he have any physical explanation for what he had found, although he was actively seeking one.

Actually, however, five years earlier, a young theorist, Lyman Spitzer, Jr, then a postdoctoral fellow at HCO, had predicted the very same concept on theoretical grounds, though Baade was not aware of it. Very few astronomers were, for Spitzer had not published his prediction. In 1939 Hans Bethe had convincingly proved that energy radiated by stars at their surfaces is “produced” (or liberated) by nuclear reactions deep in their interiors. Earlier theorists, going back to Arthur S. Eddington, had argued that nuclear energy must be released somehow in a conversion of mass (rest energy) to heat, and in 1919 Henry Norris Russell, without knowing the nuclear physics at all, had worked out the fact that the “unknown process” depended strongly on temperature, two decades before Bethe. But the refugee from Hitler’s
Germany had shown quantitatively that this process was the carbon cycle, a chain of nuclear reactions converting hydrogen to helium through interactions with carbon and nitrogen isotopes. Bethe himself, Spitzer, Martin Schwarzschild, who was then at Harvard with Spitzer, and a handful of other theoretical astrophysicists immediately realized that this new knowledge set an upper limit to the lifetime of any star; it could not radiate longer than it would take to convert all its hydrogen to helium. From the roughly known masses of a few representative stars, the conclusion was inescapable that the most luminous supergiants and hot O stars, even if they were originally composed of pure hydrogen, could not have been shining for as long as the age of the Galaxy, which had to be at least several billion years old (the accepted age at the time). Thus these luminous stars must be young, recently formed objects. The only thing they could have formed from was matter that was not stars, that is, interstellar matter. Spitzer recognized that supergiant stars are found only in spiral galaxies, rich in interstellar matter, and now he understood why. High luminosity stars were always associated with interstellar matter because they had formed recently from it. In a rotating system, namely a galaxy, interstellar matter would tend to sink to a plane, and it was there that gas and high-luminosity young stars would be found.

Such high-luminosity stars, interstellar gas, and dust are most prevalent in Sc spiral galaxies, which Spitzer therefore regarded as one extreme type of stellar system. The other extreme was systems without interstellar matter, namely elliptical galaxies, which Spitzer knew do not contain high-luminosity stars. Baade’s great discovery, five years later, was that the upper limit to the luminosity in such systems is about $M_v = -3$; Spitzer, with only a rough estimate of the luminosities of the brightest stars in elliptical galaxies (based on the fact that they had not been observed individually), realized that they could be as old as the Earth. His two extreme types of stars systems, “Sc systems” and “Globular systems”, rich in interstellar gas and dust or free of it, containing young stars or not containing such stars, exactly paralleled Baade’s later discovered populations I and II.

Spitzer included these ideas in the draft of his first paper on the dynamics of the interstellar medium, which he prepared in 1940, after he had become a young faculty member at Yale. He intended them as the introduction, to explain why his dynamical study was important. But two senior colleagues to whom he sent the draft paper both advised him that these ideas were too speculative, too “philosophical”, and that he should leave them out and stick to the quantitative, mathematical hard science that made up the rest of his paper. He did so, and left the discovery of the two stellar populations to Baade. Russell, keenly interested in every facet of stellar astronomy, undoubtedly discussed this work with Spitzer, his former student, protégé and confidant. The older theoretical astrophysicist pushed these ideas further himself, particularly for the specific example of Y Cygni, a spectroscopic, eclipsing binary supergiant, with known mass and luminosity. In a paper he presented at the dedication of Tonantzintla
Observatory in Mexico in 1942, Russell concluded that this star must have formed recently from interstellar matter. In his paper he referred to Spitzer’s work, and to related investigations by Fred L. Whipple, who had been at Harvard and had also heard Spitzer describe his ideas.\textsuperscript{40}

In 1944, when Baade wrote his two papers on the stellar populations, he sent his final drafts to Russell for his advice and comments, before submitting them for publication. The Princeton astrophysicist, a Carnegie Institution Research Fellow who frequently visited Pasadena and dispensed theoretical advice, approved highly of Baade’s papers. In his letter to the Mount Wilson astronomer, he called “the two types of relation between color and magnitude” (the two populations) “a major discovery ... which ... will probably have important consequences in the future”. Russell wrote to Baade that his draft papers tied up with the ideas the older man had expressed at the Tonantzintla dedication “to the effect that super-giant stars are relatively short-lived, and are still forming but only in specialized regions” (interstellar matter). Thus Russell had immediately grasped that the two populations were young stars and old, but Baade evidently did not understand what he meant nor follow up on it.\textsuperscript{41}

George Gamow, the Russian-born theorist of nuclear reactions, also realized, even before Baade’s papers appeared, that nuclear burning would have important consequences on the ageing, or “evolution”, of high-luminosity stars. He was hampered by his lack of knowledge of the empirical data on the properties of stars, but in 1944 wrote to Adams, the Mount Wilson director and spectroscopist, to express some of his ideas on the Hertzsprung-Russell diagram and its relationship to stellar evolution. Adams took Gamow’s theoretical ideas seriously, but could not make much out of them, expressed as they were in the émigré physicists’s ungrammatical, badly mis-spelled version of English. The Mount Wilson director did not realize that they were very closely related to Baade’s two stellar populations, and evidently never passed the letter on to him.\textsuperscript{42}

Thus by 1947, after the Second World War had ended and the physicists and astrophysicists had come home from their wartime laboratories, several theorists realized that Baade’s populations I and II were almost certainly composed of young and old stars respectively. But Baade himself initially resisted this interpretation. Whipple and Cecilia Payne-Gaposchkin both suggested it to him in 1947, but he rejected it. He scoffed at the idea that stellar evolution was the reason for the difference between the two populations, writing that “all the astronomical Darwinism of the last decades had led only into blind alleys”. He saw on his direct photographs of M 31 that the high-luminosity stars were always associated with interstellar gas and dust, but he could not grasp that this was because they had recently been born from that same gas and dust.\textsuperscript{43} In 1948 Russell published a short note explicitly stating that the high-luminosity stars of population I could not survive for the age of the Galaxy and must be young objects, which had formed recently from the interstellar matter in which they were always involved. Still Baade did not accept it.\textsuperscript{44} He thought
the Russell-Vogt theorem (an existence theorem) implied that a star’s surface temperature (essentially its spectral type or colour index) and luminosity are completely determined by its mass and composition, but this was a misconception; actually Russell had carefully pointed out years before that the run of the abundances of all the elements through the star could also play an important role. But nearly all astronomers were so used to thinking of the abundances as fixed quantities that never changed in time or place that this had seemed an unimportant quibble, and was soon forgotten. They had not realized that the very energy-producing reactions that make a star shine, also change the relative abundances of hydrogen and helium within it.45

Baade was converted to recognizing age as the main difference between the two populations in the spring of 1950, when he spent a month at Princeton University, giving a series of ten lectures to the small group of faculty members and graduate students in its astronomy department. This was the first of the many similar lecture series Baade was to give in the ensuing decade, and which were far more influential in spreading his concepts and ideas than his relatively few published papers. By 1950 Spitzer and Schwarzschild were the key Princeton astronomy professors, and Russell, though retired, attended all the lectures and participated vigorously in the discussions. Afterward Baade remembered the weeks in Princeton as one of the happiest periods of his life, with “an atmosphere as stimulating as Göttingen during my student days”. Spitzer and Schwarzschild were “a wonderful combination”; they explained the theoretical concepts of nuclear reactions and how they must affect stellar evolution to him in terms he could understand, and discussed his data with him from fresh, new angles.46

Baade especially related to Schwarzschild, whose father Karl had been the leading German theoretical astrophysicist of the previous generation, until his untimely death during the First World War while an officer in the German army. According to Hitler’s racial edicts, Martin was a “Non-Aryan”; he had emigrated to the United States, earned his Ph.D., served in the American army during the Second World War, and longed to get into action but ended up interrogating German prisoners and seeking technical intelligence just behind the lines. A small, intense, talkative, sensitive, hard-working and fantastically considerate individual, he had already resolved to make studying stellar evolution his life work. Schwarzschild and Baade hit it off very well together, talking science constantly (always in English), and by the end of March 1950 Baade was convinced. No doubt a paper by the “good German” theoretical physicist, Carl F. von Weizsäcker, who had worked on Werner Heisenberg’s atomic-bomb project, but then visited America in 1949–50, also influenced Baade’s thinking. He respected the theorist and discussed with him his ideas on stellar evolution, which were similar to Bethe’s, Gamow’s, Spitzer’s and Schwarzschild’s.47

One result of Baade’s visit to Princeton was a joint paper in which he and Spitzer suggested that 50 galaxies in rich clusters such as Coma had lost their gas and dust in high-velocity “collisions” between pairs of galaxies, in which the stars passed
right through but the interstellar clouds did collide and were left behind. In this paper they explicitly stated that the most luminous population I stars were young, while the population II stars could be as old as the galaxies themselves. From then on Baade was to use and exploit this concept frequently.\textsuperscript{48}

Caltech had begun its new graduate program in astronomy and astrophysics under the leadership of Jesse L. Greenstein in 1948. Two of its very first graduate students were Allan Sandage, who was assigned to work as Hubble’s research assistant, and Halton C. Arp, who did the same for Baade. Both of them began theses under Baade’s supervision, measuring colour–magnitude diagrams of globular clusters, to provide the quantitative data on population II that Baade needed. His primary aim was to fit their measurements of the main-sequence stars in these clusters (which could be reached on direct plates taken with the new 200-inch telescope) to the main sequence of nearby stars with known distances, thus determining the absolute magnitudes of all the population II stars in the globular clusters. Baade was particularly anxious to make an independent determination in this way of the absolute magnitudes of RR Lyrae variables, the most abundant type of population II variable stars. Arp and Sandage used the methods of photographic photometry Baade had taught them; William A. Baum, a recent Caltech Ph.D. in physics whom Bowen added to the Mount Wilson staff as its photoelectric expert, set up the standard sequences around each cluster that they used to calibrate their measurements.

The first results of this program, presented at a symposium at the end of 1951, showed that the cluster main sequences could be made to match the nearby stars up to absolute magnitude $M_v = 3.5$, but then deviated into the subgiant region, just as Baade had sketched it schematically in his famous diagram in his 1944 paper. The most straightforward interpretation was that all the stars in a globular cluster had formed at essentially the same time, initially populating the main sequence up to high luminosity, but that the more luminous stars exhausted some or all of their fuel and moved away from it. The colour–magnitude (or Hertzsprung-Russell) diagram itself demonstrated stellar evolution.\textsuperscript{49}

By that time theorists including Gamow, Chandrasekhar, Mario Schönberg, and Schwarzschild had begun exploring stellar evolution, and realized that main-sequence stars would evolve slowly at first, remaining close to their initial luminosities until they had exhausted all the hydrogen in their hot central convective cores, where the nuclear reactions were fastest. But when this central fuel supply was used up, the stars would expand and thus evolve rapidly to the subgiant and giant stages. Schwarzschild had the right combination of physical insight, mathematical skills, astronomical knowledge, analytic reasoning powers, and determination to reach the solution first. He, Baade and Spitzer arranged for Sandage, still a Caltech graduate student, to come east to Princeton in the spring of 1952, and work with Schwarzschild. There Sandage helped numerically integrate the stellar-interiors equations to model the evolution of stars just as they burned the last fuel in their convective cores, which then contracted until the remaining hydrogen just outside the core became
hot enough to ignite. These models reproduced remarkably well the observed “turn-off” from the main sequence in the globular cluster, thus confirming the entire picture. Finally, they made it possible to determine the initial parts of the evolutionary tracks of stars in the colour–magnitude diagrams, and to calculate the time the stars took to reach any point on these tracks. It was a most important result, achieved by two of Baade’s favourite protégés, and from then on he never doubted the physical meaning of the two populations.50

5. Baade’s Disciples

Throughout his career, and particularly after he announced his concept of the two stellar populations, Baade played an important role in inspiring and guiding some of the most productive astronomical research workers in America. Not only Sandage and Arp in Pasadena, and Mayall and Shane at Lick, but many other top-notch observational astronomers sought his guidance and delighted in showing or sending him their most recent results. Baade had read and seemed to remember almost every published paper on galactic structure and dynamics, and always had time for serious discussions with real scientists who visited him in his office, or for writing long letters to them. But he had no sympathy for “the weak pets” of scientific “politicians”, as he called unproductive poseurs and superannuated relics of the past. One highly productive contemporary of Baade’s, who depended heavily on him for advice, particularly after Hubble’s death, was Humason. A highly skilled observer with practically no scientific education, he, Mayall and Sandage published a most important paper in 1956 on the redshift–distance relation, full of new data and giving an updated value of the Hubble constant. This paper owed much to Baade’s behind-the-scenes influence on all three of them, particularly his continued insistence on the importance of accurately measured apparent magnitudes on a well-defined photometric system.51

With Stebbins retired and moved to California, Whitford, his successor as director at Wisconsin, made frequent trips to Mount Wilson to continue precision photoelectric photometry with the 60-inch and 100-inch telescopes. Baade encouraged him to measure distant galaxies, RR Lyrae variables and other stars in and around the globular cluster in “Baade’s window” near the galactic centre, and Whitford responded with data.52 At Lick, Kron and Eggen, both Stebbins’s former students, were now applying much improved versions of his photoelectric techniques to population questions, and Harold F. Weaver, who had worked with Baade at Mount Wilson for a year as a postdoctoral fellow, was applying photographic photometry to similar problems. They all sought Baade’s guidance, and he dispensed it freely, sometimes intervening tactfully between them when their competition became too intense.53 Arthur D. Code and Johnson were two other photoelectric observers whom Baade frequently advised and encouraged. He was glad to get these young observers’ data and results, for they were all engaged in population research, four of them using the best modern detectors which were much more sensitive and accurate than

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the older photographic plates.

One result they all confirmed was what Baade had seen qualitatively on the early 48-inch Schmidt plates. There was obscuring dust near the galactic plane in all directions; it had a very spotty irregular distribution; but there were no extinction-free paths, nor real "windows" to distant clusters and high-luminosity stars. All the old ideas about star counts and ignoring extinction corrections were wrong. Baade seemed to relish stating these facts, so contrary to the old Kapteyn assumptions, matter-of-factly but forcefully in a report to the International Astronomical Union in 1950.54

At Harvard, Cecilia Payne-Gaposchkin had quickly grasped Baade's population concept, and applied and extended it in her own research. She had an excellent English education in physics and astronomy, a Radcliffe Ph.D., and a deep knowledge of stellar spectra and variable stars. As a woman astronomer at Harvard in those days, she was condemned to a poorly paid assistant's position, in spite of her doctoral degree and her keen, creative research mind. Shapley had assigned her to work on photographic photometry, though she greatly preferred stellar spectroscopy, so much more meaningful in gaining physical understanding of variable stars. She was more highly valued in her native England than by the Harvard director. In 1952 the University of London invited her to give a series of lectures, which she converted into a book, Variable stars and galactic structure, published in 1954. In it Payne-Gaposchkin adopted the population idea as the key to understanding stellar evolution. Baade had explained his own thoughts on long-period variables, Cepheid variables, and RR Lyrae variables to her at length; in general he believed that there were two discrete populations, and every star belonged to one or the other. To her it was more natural to think of the age of a star as a continuous variable, with two extreme values, the oldest and youngest, but with a continuum of ages between them. Baade's resistance to this thought was probably the subconscious reason he had held out against the stellar evolution interpretation for so long. But he encouraged her to write the book, and considered her as the most knowledgeable research worker on variable stars and stellar populations he knew.55

Payne-Gaposchkin's husband, Sergei, was a different type of scientist altogether. He had practically no training in astronomy, and no creative ideas whatsoever. Nevertheless he had a long-term job at Harvard, estimating magnitudes on photographic plates others had taken, no doubt with the hope that his wife would keep him working productively. Baade, highly sympathetic to her, and badly needing an assistant to measure the light curves of the RR Lyrae stars and other variables he had found on his 100-inch plates of "Baade's window", taken in 1945, invited Sergei to collaborate with him on that project. It was a long tedious job. Gaposchkin ended with many incorrect periods for the RR Lyrae variables, an understandable error because the plates of the galactic centre region, necessarily taken at intervals of almost exactly one day, could all too easily yield an "aliased" or false period. Baade was well aware of this problem and had warned him against it; Gaposchkin insisted he had
avoided this mistake, but he had not. Baade remained suspicious and evidently did not trust Gaposchkin’s statement, for the light curves appeared in a paper signed by the Harvard astronomer alone. After that Baade gave him as few raw data as he could. Doing the best you could was fine, but saying that you had done better than you had was not.66

6. Radio Astronomy and Spiral Arms

Although Karl Jansky had detected radio-frequency continuum radiation from the Milky Way as early as 1933, and Grote Reber had confirmed it in 1940, radio astronomy really did not get underway as a scientific field until after the Second World War. As the war ended, several groups of British and Australian radar ‘boffins’ shifted their research interests to the peaceful heavens. They easily detected the Sun, and it was natural for them to begin building larger, more directional antennas, and more highly sensitive receivers, to search for other “radio stars”.57

One set of observations, however, was planned from the beginning for detecting interstellar gas in our Galaxy. Oort, quietly sitting out the war and pondering galactic structure at his hideaway in eastern Holland, realized that although Jansky’s and Reber’s continuum radiation was important, if there were just one radio-frequency spectral line, it would be even more important. Such a line would provide the possibility of measuring radial velocities of interstellar gas throughout the Galaxy. Oort had confirmed the main ideas of galactic rotation in the later 1920s, and he knew that these velocities could then be converted to approximate distances in a straightforward way. Sometime during the war he asked Van de Hulst to investigate theoretically whether there was a suitable line. The brilliant young Utrecht student reported in 1944 that the hyperfine-structure transition of atomic hydrogen, the $\text{H I}$ 21-cm line, should be measurable. Oort filed this problem away for immediate post-war attention.58

Once he began corresponding with Baade, after the liberation of the Netherlands, Oort soon realized that mapping the interstellar gas in the galactic plane was even more important than he had earlier realized. Baade’s letters and papers made clear that the spiral arms in the Andromeda galaxy and other nearby galaxies were primarily concentrations of gas, dust and O stars, not of average stars as all previous theorists had assumed. Locating concentrations of 21-cm emitting interstellar atomic hydrogen in our Galaxy would mean discovering its spiral arms. In 1947, before Oort’s visit to Pasadena at the end of that year, Mayall and Baade had both written to him about their joint work on M 31, which included determining its rotation curve from radial velocities of emission nebulae. Undoubtedly Oort saw all Baade’s direct photographs and other evidence during his stay.59

Back in Holland the Leiden director, organizing support for a Dutch radio-astronomy observatory, planned to find and measure $\text{H I}$ 21-cm line radiation. This program was much better suited to the low, flat, cloudy Netherlands than optical astronomy. Occupied by the Germans through the war, Holland had no experienced
radar scientists nor equipment of its own, but it did have a strong technological base at the Phillips Eindhoven Laboratory, and more than one liberated German “Würzburg dish” to use as a radio telescope. Oort used all his prestige as the leading Dutch astronomer and friend of Baade to make 21-cm astronomy the key observational project in the country, and personally pushed it through. He called on Baade for help in persuading board members of the Netherlands Foundation for Radio Astronomy who visited California, as to how important this research was. In June 1951 Oort and C. A. Muller, his chief radio engineer, were able to announce actual detection of 21-cm line radiation from interstellar hydrogen, only seven weeks after Edward M. Purcell and H. I. Ewen, both veterans of the highly successful American wartime radar crash program, had detected it at Harvard.60

Oort converted his accomplishment into more government support for a bigger and better 21-cm observatory. Many of the very best young Dutch astronomers of this period worked on reducing, analysing and interpreting HI interstellar line radiation. Oort himself described the aims and methods of the program, and showed some of the earliest data from it in his Henry Norris Russell lecture at the AAS meeting in Cleveland at the end of December 1951. In it he mentioned that if our Galaxy were a spiral (which he must have known it was), the arms would be recognizable in the 21-cm line contours.61

However, Oort and his group were not to be the first to discover the spiral arms. At Yerkes Observatory William W. Morgan, who had read Baade’s 1944 papers soon after they were submitted for publication, who had supervised the assistants who prepared the photographic prints that were inserted into every issue as illustrations for one of the papers, and who had heard Baade’s invited lectures at Perkins Observatory in 1947 and at Palomar in 1948, had been inspired by them. His research on spectral classification enabled him to jump right into the problem of finding the spiral arms himself. He recognized that the emission nebulae (or H II regions) in our Galaxy must lie in (or define) its spiral arms. Morgan and his assistants identified as many large, bright H II regions as they could; then he determined the spectral types and luminosity classes of the hot O and B stars in them. These led directly to their absolute magnitudes; photometric colour indices (mostly previously measured by Stebbins, Whitford, and C. Morse Huffer), combined with the “normal” or intrinsic colour index Morgan had determined for each type, gave the distance to the star. Most of the H II regions contained several hot, high-luminosity stars, each yielding an independent distance determination; the average of all the stars in one H II region thus gave a good value of its distance. Plotting these H II regions revealed portions of two spiral arms, one through the Sun, the other approximately one kiloparsec further out from the galactic centre. Morgan presented this result in a symposium at the same Cleveland meeting, in a session at which Oort himself presided. He congratulated Morgan, and the audience gave him an unprecedented standing ovation. Baade was not present, but enthusiastically endorsed the published map of the fragments of spiral arms.62
Though Morgan, Whitford, and Code soon extended this work and found a part of the next spiral arm further in toward the centre, this optical method is fundamentally limited to a range of a few kiloparsecs by strong interstellar dust extinction close to the galactic plane. The HI measurements were better, because the long-wavelength radio-frequency radiation was completely unattenuated; however, the assumption that all gas moved in exactly circular orbits, with a rotational velocity that depended only on distance, was an oversimplification that introduced unknown, presumably small, errors into the resulting maps. Oort sent Baade some of the first results in July 1952, and he now became even more enthusiastic over them, because they showed so much more of the spiral pattern than "the pedestrian [optical] methods".63

Van de Hulst, Muller and Oort published a long paper in 1954, in which they derived the positions of more spiral arms at larger distances from the Sun and the galactic centre. Another of Oort's best graduate students at Leiden, Maarten Schmidt, used the rotational velocities derived in this survey for the inner parts of the Galaxy, together with stellar-velocity data closer to the Sun's distance, to derive an improved model for the overall distribution of mass in the Galaxy. He had been working on this project for two years, after earlier publishing first-class shorter research papers on comets (collecting the photometric data on the differences between 'new' and 'old' comets), which helped define the concept of the Oort Cloud, and on the solar corona.64 Oort wanted to send him to Pasadena for further seasoning, and in the autumn of 1954, fully two years before Schmidt was to come, Baade "reserved" one of the Carnegie Postdoctoral Fellowships that Bowen controlled for him. "His later application will be a mere formality", Baade assured Oort, but Schmidt should be sure to submit the necessary form in early 1955. It was up to Oort whether he should send Schmidt for one or two years, Baade wrote to the Dutch director; two years would give him a better chance to tackle a major observing program, but if he could not be spared that long, one year would be enough. Furthermore, Baade wrote, Bowen had promised "to respect [Oort's] plan" and not offer Schmidt a job to stay at Mount Wilson and Palomar Observatories as a staff member. But Oort had better speak to Greenstein "since it is his dream to have a group of bright young men around him at Caltech". Oort agreed he would favour Schmidt staying in Pasadena for two years on general principles, but could not yet estimate "whether we could really spare him during so long a time". In those bad old days, directors were accustomed to thinking they could move eager young astronomers around like blocks of wood, but in the end Baade always sided with the youngsters and advised them to go their own way! Schmidt did send in an application, proposing a number of excellent research problems he could carry out with the big Mount Wilson optical telescopes, and he got the fellowship.65

In California he made a good start on several of them, but spent part of his time working out a mass model for M 31, based on new 21-cm data on it, which the Dutch radio astronomers had obtained with their big new dish at Dwingeloo. Baade
loved this work, paralleling so closely his own optical program of comparing our Galaxy and the Andromeda galaxy, using insights gained from each to help understand the other. No doubt at Oort’s insistence, Schmidt’s institutional affiliation was stated in this paper as “Temporary Carnegie Fellow at Mount Wilson and Palomar Observatories”. Before he left, Schmidt began a new, semi-theoretical study of the rate of star formation in the Galaxy; the unknown or unrecognized concept of just a decade ago had become his hot new research topic.

Bowen did abide by his promise and Schmidt returned to Leiden in 1958, after two years in Pasadena. But Greenstein did lure him back to Caltech in 1959, where he quickly became an outstanding observer with the 200-inch Hale telescope, and an authority on galaxies, radio galaxies and quasars, in many ways a latter-day amalgam of Oort, Baade and Minkowski.66

7. Radio Sources and Supernova Remnants

While early 21-cm line research was dominated by traditional astronomers, especially Oort and his students in the immediate post-war years, radio-continuum research was led by “radiophysicists”, experts on receivers, antennas, and electronic techniques, who were quite unfamiliar with the astronomical literature. Baade, with his lively, friendly personality, his relatively open mind, and his diplomatic way of imparting information, got along well with both groups. Early postwar radio astronomy, carried out at wavelengths long by today’s standards, had very poor angular resolution. Some measured positions were in error by several degrees, as later, better-calibrated programs revealed. Nevertheless, it soon became evident that radio sources were not bright stars, nor any type of stars. The brightest radio source in the sky, Cygnus A, long remained unidentified. Taurus A was one of the very first of such sources to be identified, by John C. Bolton, Gordon J. Stanley, and O. B. Slee at the CSIRO Radiophysics Laboratory in Australia. It was NGC 1952, the Crab nebula, as they reported in a 1949 paper. In tracking down this identification, Bolton had written to Oort and Minkowski, among others. He had chosen between Baade and Minkowski by flipping a coin. Oort replied with a five-page disquisition on the Crab nebula; Baade answered instead of Minkowski. Bolton addressed his next letter to Minkowski; Baade replied to it. They were both highly interested in identifying radio sources, especially as one of them had turned out to be their favourite supernova remnant. The other two radio sources that Bolton, Stanley, and Slee identified, Virgo A and Centaurus A, were the bright galaxies M 87 and NGC 5128, which the two astronomers also knew well. Baade and Minkowski plunged into the game and quickly became the acknowledged world leaders in the search for further identifications. With the largest telescope in the world at their disposal, large amounts of observing time with it, and their unrivalled knowledge of supernova remnants, nebulae and galaxies, they were an ideal team. Baade took most of the direct exposures; Minkowski concentrated on spectroscopy but also obtained a few direct photographs. Radio astronomers throughout the world sent them positions of
new sources they discovered. The two Palomar astronomers emphasized over and over again the necessity of improving their accuracy: agreement with an optical position to a few seconds of arc was convincing evidence; agreement to a degree left open the possibility of identifications with many other objects. Angular-size measurements also helped.67

In 1951 Francis Graham Smith, using a large radio interferometer at Cambridge, measured much better positions of Taurus A, Virgo A, Cygnus A and Cassiopeia A. He published them and sent them to Baade. By then the Palomar astronomer regarded the identifications of Taurus A and Virgo A as certain, not only because of positional agreement with the optical objects, but because the latter were such unusual objects, a supernova remnant and a great elliptical galaxy with a jet extending out of its nucleus. Baade did not know what the jet was, but he knew it was unique.
With Smith’s new positions, Baade and Minkowski were soon able to identify Cygnus A with an unusual galaxy, the brightest in a cluster of galaxies, and Cassiopeia A with a faint, heavily reddened, intricate, filamentary nebula highly reminiscent of the Crab nebula. Minkowski obtained spectra of the brightest filaments, which showed unusually strong forbidden [N II] and [O III] emission lines. Similarly, Baade’s direct photographs of Cygnus A showed a most unusual structure, which he interpreted as two galaxies in collision. Its spectrum, taken by Minkowski, showed rare high-ionization lines, especially [Ne V], which were broadened by velocities of 1500 km/sec. Both these identifications as radio sources were thus almost certain, from positions and from near-uniqueness, but with similarities to the few previously identified radio sources. Baade and Minkowski, with the 200-inch, had broken the radio-source puzzle.68

They began writing two long papers, discussing critically all the optical identifications, and adding as many more as they could. Meanwhile Baade informed Oort of the new results, gloating that he had won a bottle of scotch whiskey from Minkowski, who at first had not believed that Cygnus A was a pair of colliding galaxies, but now had to admit this was the case from the evidence in his own spectrograms. Oort congratulated Baade but expressed scepticism about the collision, and even more scepticism that there could be enough such interactions to account for many radio sources. Baade acknowledged that he was afraid it might be a fantastic coincidence, but he was trying to get more evidence at the telescope, one way or the other. He pointed out that Minkowski and Humason had found one other object they thought was a similar collision in progress, NGC 1275 in the Perseus cluster of galaxies, and added that tidal distortions in close passages should be much more common than head-on collisions. All of these ideas became key issues in subsequent studies of Seyfert and radio galaxies in the ensuing half-century.69

The news of Baade and Minkowski’s spectacular new results on radio-source identifications began to leak out as soon as they obtained them. In the autumn of 1951 Shapley had heard rumours of their preliminary conclusions on Cygnus A and Cassiopeia A, and wrote to ask Baade for more information, because he wanted to mention it “as one of the ten [astronomical] highlights of the year” in the annual list he was accustomed to releasing “to local astronomers” (and actually simultaneously to newspapers, magazines, and the Associated Press!). Baade by this time regarded Shapley as more of a publicity hound than a serious scientist, and replied that it was far too early for public statements. He had only obtained the data at the telescope the previous month. Baade gave Shapley just enough of the facts to establish that he and Minkowski had made the identifications, but none of the details, and included a firm injunction that nothing should be published. On the other hand when Donald H. Menzel, also at Harvard, asked for the same information, giving as his reason that he was working on a new theory of “radio stars”, which he hoped the new observational data would confirm, Baade promptly sent him their results. He was much more favourable to Menzel, a jaunty, younger admirer. Baade encouraged
him to go on with his theoretical work on radio sources, and to present the results soon at a colloquium in Pasadena.\textsuperscript{70}

Baade and Minkowski published their two long papers on the optical identifications of radio sources in 1953. Although they could find no optical remnant of B Cassiopeia (Tycho Brahe’s supernova of 1572, which Baade had searched for with the 100-inch in 1941 before radio sources had been discovered), there was a radio source at its position, which he tentatively identified with it. He had hoped then that the 200-inch would reveal it, but the heavy dust extinction still proved impenetrable. They also identified Puppis A with a filamentary nebula similar to Cassiopeia A. The 48-inch Schmidt telescope was essential for this identification because of the source’s far southern declination. These two joint papers established that “many” peculiar galaxies, such as M 87 and Cygnus A, and supernova remnants, and filamentary nebulae (which Minkowski later established as older supernovae remnants) were radio sources. They also estimated the radio-frequency luminosity of ‘normal’ galaxies, and showed that they might account for many of the weaker sources. All subsequent work on identifications flowed from these two important papers.\textsuperscript{71}

The unknown mechanism by which the radio-frequency continuum is emitted in galaxies and supernova remnants turned out to be synchrotron radiation by relativistic electrons in a magnetic field. It had been suggested by Hannes Alfvén, N. Herlofson, and Karl-Otto Kiepenheuer in 1950, and worked out quantitatively in great detail by Vitaly L. Ginzburg and other Soviet theorists within the next few years. This theory predicted that continuum radiation should be polarized perpendicular to the magnetic field. The Soviet astrophysicists Josef Shklovsky and Solomon Pikelner had suggested that this synchrotron continuum was the source of the continuous spectrum of the Crab nebula in both the radio and optical spectral regions. Viktor A. Dombrovsky at Abastumani Observatory had reported measuring strong polarization in the optical region, but because of the Cold War, communication between the two blocs was difficult; few Western astronomers were aware of his result, and those who did know about his paper were sceptical.

In 1955 Oort asked his photoelectric expert, Thedor Walraven, to try to measure the optical surface brightness of the Crab nebula in the continuum and to check to see if it were polarized. In spite of the cloudy Dutch skies Walraven was able to get a few nights’ measurements, and discovered to his surprise that the polarization was quite high, 18\% averaged over the “amorphous mass” that emitted the continuum. Measurements with a smaller diaphragm showed the amount and direction of the polarization varied systematically within it. Oort quickly notified Baade and suggested that he try to take direct exposures through a polaroid filter with the 200-inch, to obtain higher angular-resolution measurements of the polarization at each point in the nebula. He also reminded Baade of the moving “light ripples” that he had reported moving through the amorphous mass from a series of direct photographs he had taken in 1944–45. Oort asked if they might possibly have resulted

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from bursts of high-energy electrons, released from the central star, exciting disturbances in the magnetic field which propagated outward.\(^2\) Baade was greatly excited by the letter; he had Alexander Pogo, the librarian at the Mount Wilson Observatory office, translate Shklovsky’s paper, and realized from it that the synchrotron mechanism would explain the observed features of the optical continuum of the Crab nebula, unlike all previous attempted interpretations in terms of ‘normal’ thermal emission at any assumed temperature. He informed Oort of a paper by Carl O. Lampland, at Lowell Observatory, who had also noticed the light ripples years earlier.\(^3\) Meanwhile Oort, who was to see Baade at meetings in Hamburg and Dublin that summer, arranged to give brief reports of the exciting new result at a radio-astronomy meeting in Manchester in August, and at a cosmic-ray conference in Mexico in September.\(^4\)

After the cosmic-ray meeting, Oort stopped in Pasadena, where Baade had already borrowed Lampland’s plates for him to inspect, as well as others from Lick. They went to Palomar together, and Baade took a set of direct photographs of the Crab nebula with the 200-inch telescope, using a polaroid filter with another filter which isolated a band of wavelengths in the continuum, to measure the polarization. Oort took these plates back to Leiden with him; he had already chosen Lo Woltjer as the graduate student who would measure them, and soon began badgering Baade to take more of these polaroid-filter photographs of the Crab nebula as additional data. Meanwhile Walraven has taken his photoelectric photometer to the larger telescope and clearer skies of Haute Provence Observatory in France, and was repeating his polarization measurements there. Oort was already thinking about mapping the magnetic lines of force in the nebula from the polarization, and discussing the concepts with Woltjer. Theorists everywhere had heard about the new results, and were bombarding Baade and Oort with requests for photographs and data. Finally, in December 1955, Baade got another excellent series of polaroid photographs for Woltjer to measure. Oort was anxious to publish a paper quickly, because everyone in astronomy knew something about their results by hearsay. However, he and Walraven held up their paper, based on the Leiden photometry, so they could include Baade’s qualitative description of the 200-inch photographs, illustrated by the stunning pictures themselves, in the same issue of the journal. It was delayed, because Baade was giving his main attention to his research on galaxies, and was also teaching a graduate course at Caltech.\(^5\)

He sent prints for the illustrations, as well as the exciting news that he had detected polarization in the jet in M 87 (Virgo A) with the same polaroid filters, proving that the synchrotron process was operative in more than one radio source. But he did not send his note on the Crab nebula until Oort threatened to write it for him and publish it.\(^6\) Thus Fritz Zwicky, who by now was on very bad terms with nearly all the other Pasadena astronomers, very nearly published his 200-inch polaroid photographs, also taken with the Hale telescope, before Oort, Baade and Walraven got into print. Zwicky may have taken them soon after Oort had given his colloquium on the first results on the polarization in the Crab nebula in September 1955, when he
stopped in California between Mexico and Holland, although Baade, who was in a position to know from the night assistants at Palomar, believed it had been as recently as March 1956. Zwicky had not said a word to Oort about this work before they learned it was in print, and it is hard to imagine any motive for his publishing his brief note except to try to 'beat' his hated rival Baade. Certainly Zwicky's paper had no scientific content or quantitative measurements to report, and he did not state the date of his exposures. He had taken the plates with the axis of the polaroid in only two, perpendicular directions, so that it would have been impossible to determine the degree or axis of polarization if he had measured them. Baade, now anxious to establish his priority for discovering the polarization in the jet of M 87, quickly submitted a brief announcement, published as a fast-track "note" in the Astrophysical Journal.77

Oort and Walraven's paper, with Baade's note, finally appeared in the Bulletin of the Astronomical Institutes of the Netherlands just a week or two before Zwicky's publication. Their long paper awakened great interest, especially in several Caltech theoretical physicists, who saw it as opening a fresh new field for them. The Dutch paper was packed with Walraven's fully reduced data, and even more importantly, Oort's quantitative discussion and analysis of their physical meaning. He clearly stated the idea that the light ripples were magnetohydrodynamic disturbances, excited by outbursts of particles from the stellar remnant of the supernova, propagating through the inner part of the nebular remnant. Baade's illustrations were magnificent; and his very brief discussion of the magnetic field outlined in simple words the main idea of Oort's analysis. In his note Baade stated that he had turned over his 200-inch plates to Woltjer, who would analyse them in detail.78

Woltjer continued his reductions, and Oort "enticed" Mayall to obtain a new set of spectrograms of the filaments in the Crab nebula, designed to reveal their spatial distribution. Walraven reduced all his improved polarization measurements from Haute Provence, and Woltjer used this material in a very complete, far-reaching study of the Crab nebula. He combined all the data, analysed the ionization, radial velocities, the central amorphous mass, the filamentary system, the nature of the magnetic field, and even the central star. After completing this thesis near the end of 1957, Woltjer departed for America with a one-year travel grant and a research position at Yerkes Observatory, both arranged by Oort. There the young Dutch postdoc spent several months with Chandrasekhar, working on the theory of 'force-free' magnetic fields, applicable to the Crab nebula. Then Woltjer went on to Pasadena for two months, where Baade, just after he retired at the end of June 1958, inspired him to begin studying Seyfert galaxies and the physical processes operative in them, which result in emission-line spectra similar in many ways to that of the Crab nebula. Baade had long been interested in these rare, then mysterious galaxies, and Woltjer's resulting paper, written back at Yerkes where he worked several more months with Chandrasekhar, was a landmark in beginning to understand their nature.79

(To be concluded in a forthcoming issue of JHA.)
REFERENCES

ABBREVIATIONS USED

(a) Archival Sources

AIP  American Institute of Physics Microfilm Sources for the History of Modern Astrophysics
     Correspondence — Jan Hendrik Oort

BL  Bancroft Library, University of California, Berkeley
     Otto Struve Papers

HCO  Harvard College Observatory Records, Harvard University Archives, Pusey Library, Cambridge, Massachusetts
     Harlow Shapley Director's Papers
     Donald H. Menzel Director's Papers
     Bart J. Bok Administrative Files

HHL  Mount Wilson Observatory Collection, Henry E. Huntington Library, San Marino, California
     Walter S. Adams Papers
     Walter Baade Papers
     Ira S. Bowen Papers
     Edwin Hubble Papers
     Alfred H. Joy Papers

HO  Hamburg Observatory, Hamburg, Germany

OGC  Owen Gingerich Collection, Cambridge, Massachusetts

SLO  Mary Lea Shane Archives of the Lick Observatory, McHenry Library, University of California, Santa Cruz
     Directors' Papers
     Nicholas U. Mayall Papers

UAL  Special Collections Department, University of Arizona Library, Tucson
     Gerard P. Kuiper Papers

ULL  University of Leiden Library, The Netherlands
     Letters and Papers of Jan H. Oort

UWA  University of Wisconsin Archives, Madison
     Department of Astronomy Papers

Note: AIP and ULL strongly overlap in their coverage, but I have listed the source in which I actually read each letter myself.

(b) Individuals

WSA  Walter S. Adams
HCA  Halton C. Arp
WB  Walter Baade
BJB  Bart J. Bok
ISB  Ira S. Bowen
GG  George Gamow
CPG  Cecilia Payne-Gaposchkin
SG  Sergei Gaposchkin
EH  Edwin Hubble
MLH  Milton L. Humason
Walter Baade

(c) Publications

AJ Astronomical Journal
AN Astronomische Nachrichten
BMNAS Biographical Memoirs of the National Academy of Sciences
BAN Bulletin of the Astronomical Institutes of the Netherlands
CIW Carnegie Institution of Washington year book
JHA Journal for the History of Astronomy
MNRAS Monthly Notices of the Royal Astronomical Society
PA Popular Astronomy
PASP Publications of the Astronomical Society of the Pacific
PNAS Proceedings of the National Academy of Sciences
QJRAS Quarterly Journal of the Royal Astronomical Society
S&T Sky and Telescope

3. See ref. 2.
5. [JHO] to WB, [~28 Aug. 1945], WB to [JHO], 29 Oct. 1945, AIP.
7. JHO to WB, 7 Sep. 1946, WB to [JHO], 23 Sep. 1946, AIP.
9. WB to W. F. Swann, 23 July, 10 Aug. 1948, 6 Jan 1949, HHL.
10. WB to [NU]M, 1 Sep. 1948, SLO; WB to [JH]O, 7 Mar. 1949, AIP. See also ref. 2.
11. WB to D. ter Haar, 22 Oct. 1948, HHL.
14. WB to NUM, 7 Nov., 14 Nov. 1945, SLO.
18. WSA to [SB], [–5 Dec. 1947], Ira S. Bowen Papers, California Institute of Technology Archives; WB to [NU]M, 20 Jan. 1948, MLH to NUM, 19 Apr. 1948, SLO.
21. EH, [“Scientists and the Ballistic Research Laboratory, Aberdeen Proving Ground, Md. during World War II”], “Address delivered to the Sunset Club, 1946” [undated handwritten ms by EH, and typed reading copy of same], HHL.
22. ISB, “Annual report of the director”, C/IW, xlviii (1949), 3–27; xlix (1950), 3–50. These, and the other similar annual reports up through lx (1961), 59–99 are used as sources for facts later in this paper without being cited again.
25. WB, “Provisional determination of the limiting magnitude of the 200-inch” [3-page typed report], 5 Feb 1949, HHL; AEW to WB, 7 Mar., 18 May 1949, UWA.
27. WB to Sister M. Therese, 27 Feb. 1951, WB to HHS, 7 Jan. 1952, HHL.
29. HS to HHS, 23 Nov. 1951, 27 June 1952, HCO.
31. WB to [RS], 10 Nov. 1937, 4 Aug. 1939, HO.
34. HS to ISB, 17 July 1949, ISB to HS, 22 July 1949, HCO.
41. WB to [HNJR], 9 June, 24 June 1944, HNR to WB, 14 June 1944, HNR Papers, Special Collections Department, Firestone Library, Princeton University.
42. GG to [WSA], 8 Mar. 1944, WSA to GG, 6 Apr. 1944, HHL.
43. WB to [CPG], 3 Feb. 1947, OGC; WB to BJB, 7 Apr. 1947, UAL; WB to F. L. Whipple, 2 July 1947, HHL.
54. WB to [P. J] van Rhijn, 13 Dec. 1950, AIP.
57. W. T. Sullivan, III (ed.), \textit{Classics in radio astronomy} (Dordrecht, 1982) and \textit{idem} (ed.), \textit{The early years of radio astronomy: Reflections fifty years after Jansky’s discovery} (Cambridge, 1984) contain many original papers and retrospective accounts of this work up to about 1960. They, and some of the references listed in them, are the primary sources for this section and parts of the next.
58. See ref. 2.
59. NUM to JHO, 4 Feb. 1947, SLO; see also ref. 8.
60. [JHO] to WB, 4 Sep. 1950, 23 May 1951, AIP.
62. W. W. Morgan, S. Sharpless, and DEO, “Some features of galactic structure in the neighborhood...

63. [JHO] to WB, 14 July 1952, WB to [JHO], 7 Aug. 1952, AIP; W[B] to N[UM], 15 Sep. 1952, SLO.

64. H. C. van de Hulst, C. A. Muller, and JHO, “The spiral structure of the outer part of the galactic system determined from the hydrogen emission at 21-cm wavelength”, BAN, xii (1954), 117–49; M. Schmidt, “A model of the distribution of mass in the galactic system”, BAN, xii (1956), 15–41; [JHO] to WB, 15 May 1950, ULL.


68. F. G. Smith and B. Lovell, “On the discovery of extragalactic radio sources”, JHA, xiv (1983), 155–65. This paper includes long quotations from several letters between Baade, Smith and Lovell. Most of the original incoming letters to WB are in his papers in the HHL, but no copies of his own outgoing letters (which he only rarely kept).

69. WB to [JHO], 20 May, 2 June 1952, [JHO] to [WB], 24 May 1952, AIP.

70. HS to WB, 27 Sep. 1951, WB to [HJS], 28 Sep. 1951, DHM to WB, 24 Jan. 1952, WB to D[HM], 29 Jan. 1952, HCO.


73. [JHO] to WB, 26 Apr. 1955, WB to [JHO], 30 Apr. 1955, AIP; WB to [JHO], 29 Apr. 1955, HHL. The last is an earlier, longer, unsent draft of the letter of 30 Apr., containing calculations Baade made as he wrote it.

74. WB to [JHO], 2 May, 5 June 1955, [JHO] to WB 26 May, 31 May, 27 June 1955, AIP.


78. JHO and Th. Walraven, “Polarization and continuum of the Crab nebula”, BAN, xii (1956), 285–308; WB, “The polarization of the Crab nebula on plates taken with the 200-inch telescope”, BAN, xii (1956), 312; [JHO] to [WB], 19 May, 6 June 1956, WB to [JHO], 23 May 1956, AIP.