WALTER BAADE, OBSERVATIONAL ASTROPHYSICIST, (2): MOUNT WILSON 1931–1947

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1. Introduction

Walter Baade’s discovery of the two stellar populations opened the whole fields of study of stellar and galactic evolution, which have been so important ever since 1944, the date of his two papers announcing his new concept and the observational evidence for it. Baade’s whole life up to that moment was a preparation for his discovery; his scientific career after it was largely devoted to working out the consequences of it, and to stimulating other research workers to extend them further. Born in Germany, he was educated, trained and did his early research at Hamburg Observatory, except for one year as a Rockefeller fellow in the United States, as recounted in the first paper of this series. Then in 1931 he accepted a position on the Mount Wilson Observatory staff in Pasadena, where he could observe with larger telescopes on clearer skies. The present paper continues with the story of his research career there, up to the time of his great discovery, and the few years immediately after it, in which he became famous in the world of American astronomy. This period culminated in his invited address on “The survey of the problem of the two stellar populations” at the American Astronomical Society meeting in Ohio at the end of December 1947. In the following year both the 200-inch Hale telescope and the 48-inch “Big Schmidt”, with which Baade and his co-workers were to do so much more in working out the populations concept, went into operation on Palomar Mountain. His work there to the end of his life in 1960, and his many scientific travels, lectures and courses which inspired a generation of astronomers, will be described in the third and final paper of this series.

When Baade and his wife Johanna (more frequently called Hanni or “Muschi”) arrived in Pasadena in September 1931 he was thirty-eight years old. Mount Wilson Observatory, created by George Ellery Hale in 1904, was one of the most important departments of the Carnegie Institution of Washington, a privately funded foundation, devoted entirely to pure research. Walter S. Adams, who had been Hale’s student, assistant, “first astronomer”, and assistant director, frequently acting in his place, had succeeded him as director in 1923. Now nearly fifty-five, Adams was an outstanding stellar spectroscopist and a skilful, quiet, diplomatic, but iron-willed scientific leader. Edwin Hubble, three years older than Baade and a much more colourful figure than Adams or the younger German, was already famous among astronomers for his outstanding observational research on “nebulae”. He had proven without a doubt that some
of them were really galaxies. To the general public Hubble was most famous for discovering "the expansion of the universe", even if he did not want to use those words himself. Some of the other leading staff members were Frederick H. Seares, the assistant director, a few years older than Adams, Alfred H. Joy and Paul W. Merrill, both stellar spectroscopists, and Seth B. Nicholson, an expert on solar and planetary research.

Mount Wilson Observatory was unquestionably the most important observational astronomy research centre in the world. Its 100-inch reflector was the largest telescope in existence; this and its 60-inch were superb instruments at an excellent site, well equipped with the photographic spectrographs and direct plateholders that were the prime data-collecting systems of the time. Results flowed from them, published as Mount Wilson Observatory contributions at the same time they appeared in the Astrophysical journal. Briefer papers could be published even more quickly in the Proceedings of the National Academy of Sciences. Lick Observatory, seventy miles south of San Francisco, had a good site but two older, smaller telescopes, the 36-inch refractor and the antiquated Crossley 36-inch reflector. They restricted its research to long-term radial velocity programs, or astrophysical problems the Mount Wilson astronomers did not want.
to do themselves. Yerkes Observatory, with its 40-inch refractor at a much poorer Midwestern site, was in the doldrums, awaiting the retirement of its long-time director Edwin B. Frost, locked to the ideas of the past. Harvard College Observatory, directed by Harlow Shapley, who had left the Mount Wilson staff in 1920, had an assortment of small telescopes in Cambridge and in South Africa, which could not compete with the big Mount Wilson instruments, but were highly effective in discovering new objects. Shapley was at the apex of a horde of assistants and computers, and epitomized the mass-production, assembly-line type of research which contrasted strongly with the Mount Wilson skilled-craftsman approach. The Harvard director had been the great pioneer of globular-cluster work with the big Mount Wilson reflectors, and was a leader in galaxy research with smaller telescopes, so Baade had corresponded with him frequently from Germany, as he continued to do in America. But Shapley, heavily involved in national scientific and educational advisory and advocacy groups, was already finding it difficult to spend large amounts of time in doing research himself, although he was highly effective in directing others.

Many other astronomers made significant but smaller contributions to research, based almost entirely on observations they made themselves at their various university observatories, on or near their campuses, in the time they could spare from their teaching duties. Only at Lowell Observatory, another private but badly underfunded research institution, and at the Naval Observatory in Washington, specializing entirely in positional work, did astronomers do full-time research as at Mount Wilson, Lick, Yerkes and Harvard.

2. Photographic Photometry

Baade's primary job when he began work at Mount Wilson was direct photography and photometry of 'nebulae' (a term still then used to mean both real nebulae and galaxies, which the astronomers knew were actually star systems) and star clusters. He had made himself an expert in these fields with the Hamburg Observatory 40-inch reflector, and Seares had wanted to bring him to Mount Wilson as his own successor in photographic photometry. Seares and his collaborators had defined a zero point and measured accurately their 'photographic' (essentially blue light) magnitudes of the stars in the 'North Polar sequence', a small area of the sky near it, then transferred this sequence to the stars in the small Kapteyn 'selected areas' evenly spaced around the sky. The difficulties in doing this accurately (so that a difference of $\Delta m$ between any two measured stars anywhere in the sky corresponded exactly to a ratio of brightness $10^{0.4\Delta m}$) were enormous, but Baade, from his careful comparisons of selected areas with one another, knew that these Mount Wilson magnitudes were accurate down to $m_{\text{reg}} = 17.2$ or so. However, he needed to go much fainter than this to measure the magnitudes of faint stars in distant globular clusters, and of faint, distant galaxies, the ultimate aim of his program. For somewhat fainter stars the
magnitudes in the selected areas were not accurate, and Seares and his collaborators, taking short exposures with the 60-inch reflector, had not been able to measure magnitudes of the faintest stars within reach of the 100-inch at all.

Baade’s strategy was to choose a very few, well-placed selected areas, and extend the magnitude sequence in each to as faint a level as he could. He tried many methods, just as Seares had; in each the idea was to compare two exposures of the same area, with a ‘known’ difference in brightness of the stars, artificially imposed. Baade’s methods for doing this included reducing the aperture of the telescope, inserting a neutral filter in front of the photographic plate, and comparing longer and shorter exposures. But the non-linear response of the photographic effect, the background sky fog (which limited the exposure time and hence the faintness to which star images could be recorded), and changing conditions, not only of sky transparency, but the more subtle effects of ‘seeing’ and changing thermal distortion of the large primary mirrors, all worked to make each method less accurate in practice than it seemed in principle. Baade never simply took data and hoped for the best; he took many plates of each of his selected areas, compared them carefully, analysed every failure, improved his methods, and came back to the telescope prepared to do better the next month, the next season, or the next year. Thus in the end he knew quantitatively the probable errors of his results, rather than hoping and extrapolating the magnitudes scale “enthusiastically” as he thought his colleague Edwin Hubble was doing.\textsuperscript{4}

Baade’s own work was much less spectacular, slow because he was so careful, but as a result correct in the end. He exactly followed the motto of the great mathematician and Göttingen Observatory director Carl F. Gauss, “paucæ sed matura” (“few [papers] but ripe [ones]”).\textsuperscript{5}

By 1937 Baade had found that the best method was to use a “half-filter”, so that half the field was reduced in magnitude and exposed simultaneously with the other, unreduced part, doing away with some of the seeing and mirror-distortion effects (which he tried endlessly to reduce by various methods of ventilating the dome). The following year he was able to report that he at last had his “final” magnitude scale complete down to \(m_{pg} = 21\) in selected area (SA) 68, and close to ready in SA 57 and SA 61. He was also starting to get results on photovisual magnitudes (measured in yellow light) down to \(m_{pv} = 20\) in the same selected areas.\textsuperscript{6} Shapley and other astronomers were eager to use his faint-magnitude standards, but Baade would release them only when he was certain they were accurate.

The more Baade worked on setting up magnitude scales, the more convinced he became that photographic photometry was not the right way to do it. The non-linear photographic effect imposed tremendous difficulties. Joel Stebbins, the American pioneer of photoelectric photometry, was already a research associate of the Carnegie Institution, enabling him to come out to Pasadena on a regular basis in summers from his faculty position at the University of Wisconsin and use his photometer on the big Mount Wilson telescopes. In those early days,
photoelectric cells and their associated electrometers were relatively insensitive, but Baade, like Hubble, carefully monitored each improvement, and urged Stebbins to push toward measurement of very faint stars, which could be used as magnitude standards. He and his assistant Albert E. Whitford, originally trained as a graduate student in experimental physics at Wisconsin in the latest vacuum-tube circuits and amplifiers, did do as much of this work as they could. In 1945 Baade, who always regarded his own photographically determined standards as merely a stop-gap until photoelectric systems replaced them, wanted to add Whitford to the Mount Wilson staff, but that was not to be. Baade had to continue using his photographic standards until a few years after that.

Seares retired in 1940 at the age of sixty-seven, but continued as a research associate, without administrative responsibilities. Adams did not replace him with anyone to be in charge of the photometric work at Mount Wilson, but Baade became the person on whom Adams called for expert advice in that field.

3. Early Stellar Population Research

Baade did not set up faint magnitude sequences as an end in themselves, but because he needed them for his own research. He was working on globular clusters, concentrating especially on measuring the distances to the faint, distant ones, some of them in outlying regions of our Galaxy, others near its centre. In each he would try to find RR Lyrae variables; then measure their mean magnitudes accurately and thus determine their distances. He always used a magnitude scale from one of the selected areas he had measured himself, which he transferred photographically to a sequence of local standard stars around the outside of the cluster, and then compared the variable stars with them. In some globular clusters, notably NGC 5694, which had been too faint for Shapley to study, Baade could find no RR Lyrae variables, in spite of his very careful search; that meant there was none in it. Then instead he measured the magnitudes of the twenty-five brightest stars, and after carefully classifying the type of this cluster, derived its distance as 40 kpc (with the mean absolute magnitude of RR Lyrae variables he was then using). NGC 5694 is in a direction close to the longitude of the galactic centre, but about 30° above it; Baade carefully studied the field and from the number of faint galaxies, which was "normal", concluded that it was unaffected by interstellar extinction. Thus the distance he had determined was the true distance, and the globular cluster lay far beyond the nucleus of our Galaxy.

An even more distant globular cluster was NGC 2419, which did have RR Lyrae stars, and which lay in the anticentre direction. Baade put it at 56 kpc from the Sun, 64 kpc from the centre of our Galaxy — further than the distances to the two Magellanic Clouds, and comparable with the distances to M 31 and M 33 according to the extragalactic distance scale of the time. Thus whether it was an "independent system" in intergalactic space or a "far-outlying member" of
our system was largely a matter of definition. Shapley referred to it as an "intergalactic tramp", or (in a letter to Baade) as "Baade's tramp". Likewise, on his plates of the globular cluster NGC 5634, but outside it and far behind it, Baade found an isolated RR Lyrae variable 40 kpc from the Sun and 34 from the galactic centre. His discussion of these clusters and this "field variable", like his earlier work at Hamburg on the high-latitude clusters NGC 4147, M 53 and NGC 5466 and the RR Lyrae stars outside and beyond them, make it clear that he had already recognized the vast, more or less spherically distributed "population II" whose discovery he was to announce in 1944.

In 1934 Baade published the results of his long program of finding and studying the variable stars in a field in Cygnus, near the central plane of the Milky Way. He had begun the work in Hamburg, and hoped some day to publish all the detailed observational data there, but never did so. Baade found that the most frequent types of variables in it were eclipsing variables and long-period variables, and from them estimated the distance of what he called "the Cygnus cloud"; but far behind it he detected four very faint Cepheid variables at much larger distances. These were all typical luminous members of what he was to name "population I" in that same paper a decade later. Furthermore, in his 1934 paper he noted that he had found five isolated RR Lyrae variables in the Cygnus field, roughly the same number per unit area as in high-latitude fields. Hence, he concluded, these RR Lyrae stars were actually representatives of the "general [later he would call it 'spherical'] distribution of [such] variables", a key concept that he was to generalize to population II in the 1944 publication. Baade was well on the way to several of its main ideas just a few years after joining the Mount Wilson staff.

4. Clusters of Galaxies

Some of the first research Baade did at Mount Wilson was to find previously unknown faint, distant clusters of galaxies. At Hamburg he had discovered a few with its 40-inch reflector; now he could reach further out into space with the 100-inch. Baade did not make a systematic search, photographing blank fields, but instead inspected each plate he had taken for any purpose, looking carefully at everything that was on it. Since he was an expert in guiding, focusing the telescope critically, and correcting the focus slightly for temperature changes during the long exposures, his plates were particularly good and revealed faint, small galaxies near the limit of resolution, dependent on seeing. Hubble and Milton L. Humason, the former mule driver, janitor and night assistant who had become an excellent spectroscopist of faint 'nebulae', realized that clusters of galaxies were the best objects for pushing out the redshift–distance relationship as far as possible. The brightest galaxy in a cluster might be intrinsically considerably more luminous than the 'average', and thus spectroscopically observable, while the more numerous, fainter ones could be measured for magnitude,
providing a good distance determination.

In his first year at Mount Wilson Baade found the Andromeda cluster, a fairly close one; Leo II, a rich cluster containing three hundred detectable galaxies above the plate limit, $m_{pg} = 19$; and Coma II, the faintest then known, well beyond the limit at which individual field galaxies could be measured. The next year he discovered another very faint one, in Cetus, and two years later still another, in Cygnus, not far from the Milky Way and showing signs of interstellar extinction. Baade measured the colours of the galaxies in several of the unobscured clusters and in others that Hubble had discovered, and found that they all had only a relatively small range of colour index, indicating that in at least this respect they were physically similar and hence possibly similar in absolute magnitude also. Hubble had assumed this; Baade, always sceptical, was trying to check the assumption.

None of these discoveries or tests was important enough to Baade to publish them as papers or notes; they appear only in the annual reports of the observatory for his early years at Mount Wilson. He was working his passage by helping observe for Hubble and Humason's program. But he never joined wholeheartedly in it as a collaborator, and gradually dropped it altogether, as he became more and more involved in his primary stellar-population research. Always friendly, diplomatic and helpful, he had nevertheless resolved, probably long before he arrived at Mount Wilson in 1931, to be his own man there as he had been at Hamburg Observatory.

5. Supernova Research with Fritz Zwicky

The study of supernovae became a hot research topic in the 1930s, and Baade was one of the leading pioneers in it. The concept that there are two types of novae, the 'ordinary' ones and at least one example of a much more luminous type, S Andromae, which reached visual apparent magnitude 7.5 near the centre of M 31 in 1885, was put forward by Heber D. Curtis in 1917. Knut Lundmark, just three years later, further discussed this idea and called the two types "giant" and "dwarf" novae. In 1927 Baade referred to S And as a "Hauptnova" ("chief nova") in a popular lecture, as we have seen. Then in 1933, Curtis, in an influential review in the Handbuch der Astrophysik, amassed the observational evidence that there are two classes of novae, those with absolute magnitude at maximum $-5$, and a much rarer class which reached absolute magnitude $-15$. He listed six more examples in addition to S And 1885, one in our Galaxy, Tycho Brahe's 'nova' of 1572, and five in other spiral galaxies (four discovered by Hubble and one by Williamina Fleming at Harvard). Because they are so very luminous they can be observed to great distances; hence they offered the possibility of being used as distance indicators in the universe. Baade, a voracious reader of astronomical research papers, particularly those on galaxies, was undoubtedly aware of all this long before the publication of Curtis's Handbuch article.
Soon after arriving in Pasadena in 1931, Baade met Fritz Zwicky, a Swiss theoretical physicist on the California Institute of Technology faculty. Hale had built Caltech up from the tiny Throop College of Technology, and brought Robert A. Millikan from the University of Chicago to head it in 1921. The Mount Wilson Observatory offices on Santa Barbara Street were less than two miles from the Caltech campus, and there were many links between the two institutions. Zwicky, five years younger than Baade, had first come to Caltech as a Rockefeller postdoctoral fellow in 1925–27, and the two may have met during the second year while the German astronomer was in Pasadena also. Zwicky, who stayed at Caltech, first as an instructor, then an assistant professor from 1929 until 1942, dabbled in research in almost every field of physics from ferromagnetism and crystal structure to electrolytes and a “gravitational drag” theory of the red shifts of the galaxies. However, he was a flamboyant, highly successful teacher of theoretical physics, with an interest in astrophysics.

Baade and Zwicky became friends, and frequently discussed the two types of novae; apparently they coined the name ‘supernova’ in 1933. Together they wrote a series of three papers published in 1934, summarizing the fruits of their discussions and establishing supernova research as an important field. The first paper, clearly drafted largely by Baade, outlined the observational distinctions between ordinary novae, which occur at a rate they estimated as about 10 to 30 per year in our Galaxy and M 31, with mean absolute magnitude at maximum –5.8, and range of about 3 or 4 magnitudes about it, and supernovae, with absolute magnitude –13 at maximum, which flare up only one per several hundred years in a typical spiral galaxy. Furthermore they estimated the total energy released in a supernova outburst as possibly of the order of a stellar rest mass, comparable with the mass of the star itself.19

Their second paper, no doubt expressing more of Zwicky’s ideas, put forward the idea that supernovae were the sources of cosmic rays (then a very hot subject in experimental physics, with Millikan one of its leading practitioners). Baade and Zwicky noted that in a supernova explosion a large mass of gas, probably comparable with a stellar mass, was expelled with high velocity, perhaps nearly the velocity of light, and conjectured that this mass contained the incipient cosmic particles. Although their suggested mechanism differed in detail from the one Millikan favoured, it agreed with his general idea that cosmic-ray particles were accelerated in “God’s laboratories in the stars”. Baade and Zwicky further noted that the final, stable configuration of matter would be a neutron star, and that supernovae could represent transitions of stars to this state.20 A third joint, short paper, even more clearly originating in Zwicky’s thinking and published in the Physical review, mostly called attention to the earlier two, and pointed out that if their ideas were correct, some of the primary cosmic rays incident on the Earth would be protons and heavier ions. (At that time the composition of the cosmic rays was still unknown, and the existence of a galactic magnetic field was not suspected.21)
Clearly more observational data were needed, and to find just a few supernovae per year would require a fast, wide-angle photographic survey, covering hundreds if not thousands of galaxies on a single exposure. Comparing or ‘blinking’ exposures taken a few weeks to a month apart would reveal any bright supernova that had flared up in the field. Hubble had begun a half-hearted supernova search in the Virgo cluster of galaxies in 1928, using the 10-inch photographic Cooke telescope on Mount Wilson. Baade had continued it after his arrival, but the telescope was not large enough, and its inherent aberrations made it unsuitable for the task. Baade, who had brought the news of Bernhard Schmidt’s newly invented wide-field camera to Mount Wilson Observatory in 1931, undoubtedly realized that a Schmidt telescope would be ideal for such a supernova search. Furthermore, Zwicky would be able to provide the way to get it.

Baade cultivated him, and in either 1933 or 1934 Zwicky began searching the Virgo cluster on a more regular basis, with a 3.5-inch, commercially-available Wollensak camera mounted alongside a visual telescope (used to guide the exposures and for its mounting) on the roof of Robinson Laboratory, the astronomy building on the Caltech campus. It was even less satisfactory than the Cooke 10-inch, but it whetted Zwicky’s appetite and gave him his first serious observing experience. He was in a very good position because Caltech, not Mount Wilson Observatory, controlled Palomar Mountain and the 200-inch telescope that was to be erected there, after its mirror had been completed on its campus. When Hale had secured the funds to build the giant new telescope from the Rockefeller Foundation in 1928, its executives refused to give them directly to Mount Wilson Observatory, part of the rival Carnegie Institution, Corporation and Foundation empire. Instead, the Rockefeller grant for the 200-inch went to Caltech, which already had large chemistry and biology operations financed from the same source. Hale moved John A. Anderson from the Mount Wilson staff to Caltech as executive secretary in charge of the project, and the Mount Wilson astronomers, especially Adams and Hubble, were heavily involved as advisers, but it was Caltech’s project, site and money, and it would be Caltech’s telescope. The 200-inch design group, mechanical shop, and optical shop were all on its campus, but it had no astronomers or astrophysicists on its staff. Clearly it was a niche that Zwicky was ready to fill.

He had first visited Palomar in 1930, soon after it had been selected as the eventual location for the “Big Eye”. Now, coached by Baade, he helped persuade the Caltech authorities to provide the money to build an 18-inch Schmidt camera and erect it there. At Palomar, an excellent dark-sky site, he could carry out a really effective supernova search. Zwicky later claimed that he personally convinced Hale to allot $25,000 for this telescope, but it seems likely that with Anderson, Hubble and Baade, the leading authority in America on Schmidt cameras, all for it, the old “honorary director” could hardly have resisted. The project was approved by the Observatory Council in 1935, and in June of that
year Baade wrote to Richard Schorr, still director of Hamburg Observatory, introducing Zwicky, "a good friend of mine", for whom the 18-inch Schmidt was already under construction. Schorr welcomed him on a visit to the observatory later that summer, and Schmidt exhibited his camera to him, and some of the spectacular photographs he had taken with it. The 18-inch Schmidt corrector plate was difficult to make, but by September 1936 the telescope was in operation on Palomar, and Zwicky, assisted by Josef Johnson, began taking films with it. They had a serious search program underway by November, and within a year Zwicky found his first two supernovae with the new Schmidt.

But by the time Baade and Zwicky published their joint paper on the light curves of these two objects in 1938, tension between them had grown. They never wrote another paper together. Zwicky, an isolated physicist in a new field, not unnaturally feared that the Mount Wilson astronomer would get all the recognition, and that his own contributions would not receive the credit he felt they deserved. He was eager for the publicity that would bring him the salary raises and promotion he had long anticipated. Zwicky was a very prickly, combative person, quick to take offence, who eventually got on bad terms with almost every scientist whom he could consider, in any sense, a rival. In 1936 Cecilia Payne-Gaposchkin, greatly interested in the astrophysics of any kind of variable star, studied Baade and Zwicky's papers carefully, and in 1936 wrote one herself, mildly criticizing some of the details of their estimate of the total energy released in a supernova outburst, and hence their final numerical result. Zwicky, affronted by the pre-publication copy of the paper she sent them, dashed off a hot letter representing himself as speaking for the two of them, in which he criticized it violently, and called her a fool. ("We ask ourselves whom you really wish to make fools of – if not yourself.") Baade, who admired her work in general, and never insulted anyone, in fact had not joined the argument, and soon began dissociating himself from Zwicky. The latter published a short paper on his own, refuting to his own satisfaction Payne-Gaposchkin's criticisms more temperately. In 1938 he published another paper describing his supernova search to that year, giving few of the observational details and minimizing the contributions Baade had made.

Their division of effort from the beginning had been that Zwicky would find the supernovae with the 18-inch Schmidt, and Baade would follow them with the 60- and 100-inch, obtaining their light curves by transferring the magnitudes he had set up in one of his well-measured selected areas to local standards near the new object. The first supernova he followed this way was in NGC 4273 in the Virgo cluster, discovered by Hubble and his assistant Glenn Moore with the 10-inch Cooke telescope in January 1936, the next two were in his joint paper with Zwicky, and by 1938 he was able to publish preliminary light curves of eighteen supernovae, including these three and one more found in the interim by the Caltech astrophysicist; the rest were objects previously recorded, for which Baade had redetermined improved magnitude sequences. Using distance moduli
provided by Hubble for the Virgo cluster and for a few non-member galaxies, presumable from their brightest stars, Baade ended up with ten supernovae with reasonably well-determined absolute magnitudes at maximum. The mean value was $-14.3 \pm 0.4$, with a dispersion of 1.3; it could be used as a first approximation to determine the distance to the galaxy in which a supernova occurred. Baade was rapidly collecting the observational data which, with low-dispersion spectra, would lead to the classification of supernovae and their use as distance indicators.\footnote{Baade also noted that by far the great majority of supernovae were found in late-type spirals, 72\% in types Sc and SBc, 16\% in Sb and SBb, and only 12\% in all the other types together. This was the kind of observational correlation for which he was always looking; in 1944 he would realize that this meant that supernovae belonged to his then-named population I, associated with O and B stars, and interstellar matter. Finally, in this paper, Baade stated that there was observational evidence of two supernovae in our Galaxy, not only Tycho’s object of 1572, but the Chinese ‘nova’ of 1054, whose remnant was the Crab nebula, expanding at a rate of 0.18"/yr, with a radial velocity of 1300 km/sec, as described only the previous year by Nicholas U. Mayall.}

A young member of the Lick Observatory staff, Mayall had done his undergraduate work in astronomy at Berkeley, graduating in 1928, and had continued for one year as a graduate student there. Then, tiring of classes and needing to earn a little money, he had taken a job as an assistant at the Mount Wilson Observatory offices in Pasadena. His main work was measuring and reducing observational data for various staff astronomers, but he managed to get to the mountain with them too, and learned to observe with all the telescopes. He was inspired by Hubble and became especially close to Humason. Mayall returned to the University of California in 1931, did a thesis with the 36-inch Crossley reflecting telescope, earned his Ph.D. in 1934, and joined the Lick staff himself, as an assistant who was soon promoted to a regular research post. There he designed and before long had built a very fast, low-dispersion spectrograph, well-matched to the Crossley reflector, and forming with it an extremely efficient combination for observing globular clusters, galaxies and nebulae.\footnote{Mayall’s interests fitted exactly with Baade’s, and the two were soon in frequent correspondence. The older German was quick to furnish whatever information the young observer needed on specific clusters and nebulae, and Mayall reciprocated with plates from the Lick files of galaxies with supernovae in them, many of them taken by Curtis years earlier. Baade always added helpful, supportive advice, never seeking credit for the results Mayall obtained while using it. Mayall was the first of many hard-working, young American astronomers whom Baade was to befriend, advise and inspire throughout their careers.}

In 1937 Baade had encouraged Mayall to use his fast spectrograph to make a “thorough radial velocity” study of M 1, the Crab nebula, a highly peculiar nebula known to be near the position in the sky where Chinese astrologers had seen and
recorded a “new star” in A.D. 1054. Curtis had included it in his list of planetary nebulae, but had described it as peculiar and unlike any other one. Because of its faintness, V. M. Slipher had been able to obtain only barely usable spectrograms of it, but Mayall could take good exposures with his fast instrument on the Crossley, with the slit along the major axis of the nebula or in any other position. They clearly showed the nebula was expanding with a velocity 1300 km/sec; John C. Duncan’s recent proper motion study of it, with the 100-inch, ruled out the alternative possible interpretation of contraction, and gave 0.18″/yr as the rate. Simply comparing these two values, Mayall immediately obtained the distance to the Crab nebula, approximately 1500 pc. Always conservative, he interpreted it as a nova which had had an outburst in A.D. 1054, because the expansion velocity was comparable with those observed in nova shells, and Duncan’s angular rate of expansion gave it in 1937 approximately the observed size that a nebula would have which had started from a point nine centuries earlier.

However, the fact that the Chinese had seen and recorded the star suggested it had been bright; Baade suspected it had been a supernova, and by 1938 he thought there was “little doubt” of it. The Crab nebula appeared nothing like the little expanding nebular shells that were the remnants of ordinary novae, which all seemed to fade to invisibility within a century or less. Baade had begun obtaining direct plates of the Crab nebula himself at Hamburg, and started again at Mount Wilson, but did not want to publish anything himself that would interfere with Mayall’s work on it. By letters and in discussions when the Lick and Mount Wilson ‘nebular’ astronomers got together, Baade continued to encourage Mayall’s observational study of the Crab nebula, and in 1939 the latter published a popular article on it, in which he called it a probable supernova at last.

Three years later, after Mayall had left Lick Observatory and its telescopes, on leave to do wartime research “for the duration”, Baade did publish a definitive paper on the Crab Nebula as a supernova remnant. Then a few years after the war, the sometime radar ‘boffins’ who energized the new field of radio astronomy discovered that the Crab nebula was one of the brightest radio sources in the sky, drawing Baade into their science, in which he was to become a leader and elder statesman. His close collaborator in those roles was his friend and fellow Mount Wilson staff member, Rudolph Minkowski.

6. Rudolph Minkowski

Minkowski had been Baade’s friend and collaborator in Hamburg, an experimental physicist who was a specialist in spectroscopy and optics, drawn into observational astrophysics initially to study the emission-line profiles in the Orion nebula. He was an associate professor of physics in Hamburg, but he was a Jew according to Hitler’s racial laws, although his family had been assimilated and he was a baptized Lutheran. Minkowski’s father was a medical professor, whose
research on diabetes and the pancreas had made him “the grandfather of insulin”, while his uncle Hermann had been a world-famous mathematician at Zürich and Göttingen. Rudolph, two years younger than Baade, had served as an officer in the German army during the First World War, but that made no difference after Adolf Hitler came to power as Chancellor of the German Reich in 1933. Minkowski knew his days there were numbered. He wrote to his friend in Pasadena, and Baade went straight to Adams. Although he had been a staff member at Mount Wilson Observatory only two years, Baade already had learned exactly how to approach his director most effectively. Would Adams be willing to invite Minkowski to work at the observatory for a year, with the prospect of applying the same interferometric methods he had worked out and perfected in his study of the Orion nebula, now using the big Mount Wilson telescopes to apply them to the planetary nebulae? Baade did not breathe a word about Hitler, Jews or racial laws, but he did report that Otto Stern, the Hamburg physicist famous for the Stern–Gerlach experiment (and soon to leave Germany himself for the United States) had mentioned Minkowski’s case to Max Mason, the president of the Rockefeller Foundation, who “thought it would be possible” to provide funds for his salary for a year.

Adams understood exactly what Baade’s letter meant, and he was prepared not only to welcome Minkowski if he could come to America with a salary, but to help him get it too. The Mount Wilson director preferred to explain all his actions on the basis of scientific research alone, but Minkowski’s famous family name meant a lot, and the fact that Baade wanted to bring him over was all-important. Adams already regarded him as the hope of the future. In addition, the Rockefeller Foundation was funding Palomar, and Mason’s interest in Minkowski further whetted Adams’s willingness to have him at Mount Wilson.

It turned out that Mason had spoken impulsively; since Minkowski still had his job in Hamburg the Rockefeller Foundation could do nothing for him then. However, at the end of March 1934 Minkowski was discharged from his faculty post under Hitler’s edict “For the good of the Government Service”. Many other German scientists were trying to get to America too, and the Rockefeller Foundation had only limited funds. It was further hampered by a widespread feeling that Jewish refugees should not take jobs that young American scientists were seeking. Adams argued very effectively against this reason for not helping Minkowski, writing to Edward R. Murrow, the young assistant secretary of the Emergency Committee in Aid of Displaced German Scholars who later became a famous radio war correspondent, that it seemed “unfortunate[ly]” close to the Nazi principle that Jews were in direct competition with young Nordic scientists for jobs in Germany.

This powerful statement, plus the support of Stern and Rudolph Ladenburg, another German physicist who had already made his way to Princeton, tipped the balance, and in October 1934 the Emergency Committee granted $2,000 to support Minkowski for one year at Mount Wilson Observatory. In November
the Rockefeller Foundation itself added a further $1,000, making Minkowski’s total salary for the year $3,000. Adams was sure this would be enough for him (although he had not been paid since March, and was planning to bring his family and belongings with him, and stay in the New World at least until Hitler and the Nazis no longer ruled in Germany). Minkowski arrived in New York in May 1935, but now a new complication arose. John C. Merriam, the ageing president of the Carnegie Institution, had not been included in the long, complicated negotiations, and now when he was, he refused to accept a cheque from the rival Rockefeller Foundation to pay Minkowski. It was contrary to policy to do so, Merriam said, and he was a great believer in following policy. Adams was on his way to an international scientific meeting in Europe, and Seares, the acting director in his absence, had to spend hours writing long, persuasive letters and telegrams to Merriam. Finally the Carnegie president agreed to “act as agent”, take the Rockefeller money each month, and pay it to Minkowski. While all this was going on, the German scientist had continued on to Pasadena with his family, settled in with Baade’s help, and begun work on 1 June 1935 as a “visiting investigator”. He began observing at Mount Wilson very quickly, and published his first paper in America, on the spectrum of Comet Peltier, in 1936, jointly with Baade.

Before Minkowski’s arrival, Baade had taken spectra of the faint stars in the Cygnus cloud, for instance, but now he turned that side of the work over to his friend, and concentrated almost exclusively on direct photography. Their first big joint program was on the stars and dust in the Orion nebula. Reasonably fast infrared-sensitive photographic plates were just starting to be available in the late 1930s, and Baade realized that they were much better for penetrating the dust clouds of interstellar space than the standard blue-sensitive plates long used by astronomers. He knew that the Orion nebula contained dust, and to investigate what stars were near its centre, took a long exposure of it in the infrared region $\lambda\lambda$ 7200–9000. There he found a previously unknown small cluster, containing some 80 stars within a small region centred on the brightest O star in the Trapezium, $\theta^1$ Orionis. Baade and Minkowski analysed this region in detail, measuring the extinction of light by the dust by spectrophotometry, comparing the continuous spectra of the B stars in it that were heavily reddened with others which were not. Using available computed optical properties of various hypothetical types of interstellar particles, they estimated sizes and tentatively confirmed their composition as small, impure “iron” (and other metal) particles with a large admixture of hydrogen. Though their detailed conclusions were superseded a decade later, these papers did much to define the properties of the associations of hot stars and interstellar clouds, which Baade was later to define as population I.

Minkowski, with his excellent training and experience in spectroscopy, designed a fast nebular spectrograph, built around a Schmidt camera, soon after his arrival in Pasadena. It was built in the Mount Wilson instrument shop, and
he was using it on the big telescopes within a year. Adams recognized his value as a research scientist and instrumentalist and wanted to keep him on the staff, but there were no vacancies, and no new positions expected in those grim Depression days. Hence the Mount Wilson director recommended him strongly for a second year's support by the Emergency Committee. This time it allotted Minkowski $2,000, but pressed Adams for a commitment for a permanent appointment for him. The director explained why he could not make it, but emphasized that Minkowski's research was so valuable that if they had the money, they would take him on. The Rockefeller Foundation's firm policy was not to renew any refugee scientist's grant beyond the first year, so Minkowski's salary in his second year in America was $1,000 less than his first year's.45

A year later that grant ran out, and there was no hope of further support from the Emergency Committee. Adams by now considered Minkowski "a remarkably able physicist with all the modern developments of quantum and wave mechanics at his fingers ends, ... [and] one of the few physicists I have ever known who has also become an admirable astrophysicist". The German refugee was giving a series of lectures on theoretical spectroscopy to a dozen-odd members of the Mount Wilson staff (including Adams himself), for which they 'chipped in' to pay him and thus supplement his income. Adams wanted to keep him on the staff with a temporary appointment, funded by the salary of another staff member who was going on leave for a year, but Merriam would not agree. He had no doubt already earmarked the savings for another project. Thus for his third year in the New World, the only solution was for Minkowski to take a lowly assistant's job, paying $1,500 a year, another decrease in salary. Adams let him continue actually working mostly on his own research, in spite of the change in title.46

However, Minkowski's deliverance was at hand, although from an unfortunate source. In February 1938 Francis G. Pease, an elderly staff member who had come to Pasadena with Hale and Adams in 1904, died, creating the first vacancy since Minkowski's arrival. Adams had meant what he said, and recommended him strongly to Merriam for the position. Minkowski was "the most competent theoretical physicist we have, ... an excellent ... observer, [with] marked ability in the design of instruments ... [and] a very pleasing personality". By then he was regularly observing the spectra of supernovae, to complement Baade's measurements of their light curves. Merriam was "favorably disposed", but wanted to know more about the German refugee's "immigration status". Adams was pleased to report that since Minkowski had entered the United States on an immigration visa, he could stay without any danger of being deported, and that he had taken out his first papers and intended to become a citizen as soon as he could. This satisfied the Carnegie Institution president, and Minkowski became a regular scientific staff member on 1 July 1938, at a salary of $3,000 a year.

Minkowski had earned these accolades (if not more money) especially by his observational work on the spectra of supernovae. Beginning in 1937 with the
supernovae in IC 4182 and NGC 1003, the first two Zwicky had discovered with the 18-inch Schmidt, Minkowski obtained spectra of every supernova he could, from as soon after discovery as he could until they again became too faint to record. His spectra complemented the light curves that Baade was measuring. Previous Mount Wilson observers, especially Humason, had earlier obtained a few spectra of a few supernovae, but with Zwicky and Johnson finding them, Baade measuring their magnitudes, and Minkowski recording their spectra, within a few years they obtained a much better idea of the observational aspects of these very high-luminosity stellar outbursts.

In these first two supernovae he observed, Minkowski confirmed that their spectra consisted of broad emission features, whose redshifts increased with time, reaching a maximum, then decreasing somewhat. None of the emission features could be identified; they did not seem to be lines of either hydrogen or helium, so common in novae, nebulae, and hot stars. Whether the red shifts were due to radial velocities of gas in the shells (as in novae) or were gravitational in origin he could not tell by any test or analysis, but once he had arranged the spectra of these two supernovae in time sequence, he could see that at similar time intervals after maximum light, their spectra were quite similar. With this knowledge he could then interpret the fragmentary spectral information on earlier supernovae, and could see that six of the seven were similar in their development to these two. Only S And, the 1885 supernova in M 31, seemed from the available description of its spectrum to be physically different. 48

In 1940 Minkowski observed a supernova in NGC 4725 with a completely different type of spectrum from those he had previously observed. Soon after maximum and continuing for about a week, it had a purely continuous spectrum, very blue, indicating an extremely hot star. As it decreased in light, the broad emission features began to appear faintly, gradually strengthening. Their spectra were thus much more reminiscent of the spectra of ordinary novae. By June 1941, when he gave a paper summarizing his work on supernova spectra at a meeting of the Astronomical Society of the Pacific in Pasadena, Minkowski had enough data to outline a spectral classification scheme for them. There were two basic types, type I, like the first two he had studied, and type II, like NGC 4725. There were now nine supernovae whose spectra put them in type I, five in type II. Within each type the spectra were similar in their evolution with time. He had isolated two physical classes of supernovae. Only S And did not seem to belong to either, and it was unclear whether that was the consequence of a real difference, or if the observational data from 1885, so early in the days of scientific photography, were faulty. Minkowski’s classification scheme formed the framework for Baade’s ongoing photometric study of supernovae. 49

In spite of his path-breaking results on supernovae, and his spectrographic design skills, so important at Mount Wilson not only for his own work but for Humason’s measurements of the redshifts of distant galaxies, in 1941 Minkowski was still receiving $3,000 a year. Only in 1942, after he came to Adams and told
him that his family was having difficulty living on his salary, did he get a raise — to $3,300. The way of a refugee scientist with no competing offers was hard in prewar America.\textsuperscript{50}

7. Hamburg Directorship

In contrast to Minkowski, Baade had come to America to accept a job, not to find a refuge. Schorr, his director at Hamburg Observatory, had wanted him to stay there; he had visualized Baade as his eventual successor in the post as early as 1927, when he had arranged for him to be appointed Observator. Then Schorr had been sixty years old; in 1934, one year from retirement age, he began his campaign to have his former star staff member named as the next director. He wrote to Baade, and asked if he would return; the younger man, after weeks of indecision “(in the first sleepless nights of my life!)”, replied that he would come back only if he had the great honour to be “called” as Schorr’s successor. The Mount Wilson astronomer was planning to travel to Europe to take part in the IAU General Assembly in Paris the following year, and he would certainly visit Hamburg and discuss the question with him. The next spring, when Baade received an official letter from Hamburg, asking if he were willing to have his name included on the list of candidates for the post, he replied along the same lines, that only a call for the directorship there would cause him to bid Mount Wilson farewell, and that he therefore allowed his name to go forward. But, he now told Schorr, he would not be able to attend the Paris meeting, because under his contract with the Carnegie Institution, to have his trip abroad paid for he would have to declare that he did not intend to resign his post.\textsuperscript{51}

Later that spring Schorr let Baade know, confidentially, that his retirement had been postponed until the autumn of 1936, and that the Hamburg faculty had been asked to send nominations for his successor to the Culture and Education Ministry in Berlin. They had named Baade as the single, first, and only candidate! Schorr had then stated that Baade was “nationally disposed” (a loyal German), not a member of any political party, and of “Aryan origin”, along with his wife. He sent Baade the necessary forms on which to list his and his wife’s ancestries and attest to this statement, and emphasized that they should be sent back directly to him, along with their birth certificates, marriage certificate, and their parents’ marriage certificates (all of which would state their religions). That was part of the process for being named to any academic post in Nazi Germany. Schorr confided that Berlin might refuse to accept a single name, and that the Hamburg faculty had so far been unable to agree on alternative candidates for this eventuality. The next day Schorr sent Baade a shorter, more formal letter, stating the faculty requested his advice, as a former member, on the other candidates for the directorship. Of the four whose names had been suggested, Baade said he could not judge his friend Johannes Hellerich, the only one who was at Hamburg; he praised Otto Heckmann strongly, as by far the best of the
younger German astronomers, but was much less enthusiastic about the other two. In another letter, he privately told Schorr that as he had arranged for the Carnegie Institution to bring Minkowski to Pasadena, he could not leave himself for at least a year and a half, and that two years would be better. As the reality of leaving the land of clear skies and big telescopes came nearer, he was beginning to draw back. But he enclosed the documents for which Schorr had asked, and wrote that their relatives in Germany would deliver the rest to him there. He was willing to play the Nazis' game, as every astronomer still in Germany was.\textsuperscript{52} In his correspondence with Schorr, Baade barely mentioned Minkowski, and gave no hint that he himself had been the impetus in making it possible for him to come to Mount Wilson, preserving the fiction that it would be a one-year visit. On the other hand Baade severely criticized Hans Rosenberg, an elderly pioneer of photoelectric photometry and a refugee from Hitlerism, who had spent some two years at Yerkes Observatory, also supported by the Rockefeller Foundation and the Emergency Committee. He had not produced any scientific results, and failed to land a permanent job in America, finally ending up as director of the Istanbul Observatory in Turkey in 1938. Baade never mentioned that Rosenberg (who had served as an officer at the front in the German Army in the First World War) was Jewish (as he and Schorr both well knew), but was critical of his sparse research results.\textsuperscript{53}

In this same letter, Baade sent Schorr the good news that he would be able to visit Germany over Christmas and January 1936. Adams was taking him to the
annual Carnegie Institution board meeting in Washington in December to describe his latest results to its president and board members. As a reward he would get a three-month leave, and as he would pay for his own passage on the ship, there would be no bar to his resigning later, if he so decided. Schorr notified the Ministry for Science, Education and Culture that Baade, the single candidate named by the faculty for the Hamburg directorship, would be in Germany and available for an interview, but the Nazi bureaucrats ignored it and sent a letter to Baade in Pasadena (which he received only after his return) requesting him to send all his papers proving that he and his wife were Aryans. They made no inquiry into his scientific accomplishments. Baade went to Germany, visited his mother and brother in Minden, gave a lecture at the university on “Spiral nebulae as star systems”, and returned to California in March without ever being interviewed.\(^{54}\)

Schorr continued pushing for Baade’s appointment, and in September 1936 reported that the Dean of the Hamburg faculty thought that “under present circumstances” it would be “good” if Baade joined the Nazi party immediately. Baade’s reply was masterful. He wrote that he could not take such an important step on opportunistic grounds; if he did, it would have no moral value. But if he came back to Hamburg as director, he would show, he believed, that he could work together with the regime. Even before Baade replied, Schorr independently advised him not to take the “suggestion” too seriously, as only two professors in the entire Hamburg faculty of mathematical sciences were then party members.\(^{55}\)

Baade, although he did not go directly to Adams, naturally let his friends on the Mount Wilson staff know of the offer that Schorr at least was sure he would soon be getting. They wanted him to stay at Mount Wilson, and warned the director of the danger of his leaving. Adams badly wanted to keep Baade, and talked seriously with him. By then Baade’s salary had risen to $3,600, but he expected it would be $7,500 at Hamburg. Adams recommended him for a $500 raise for the next year, writing to Merriam, the Carnegie Institution president, that Baade “would be quite capable of doing the work which Hubble has done equally well, if not better”. The Mount Wilson director also asked for funds to allow Baade to travel to Germany more frequently, which he said was his main desire. Merriam authorized the raise, but added a warning that although “the striking prosperity in Germany, the unity of the people, and the great military strength which is now being developed are appearances which deceive many into thinking that the future of Germany is secure . . .”, he “ha[d] the feeling that Germany may be in a more dangerous situation today than at any time in the past half century. I would not wish to see any friend of mine deliberately choose to locate himself for life in Germany at this time.” Adams passed the warning on to Baade, who realized “that the situation is a very uncertain one, and may change radically in a very short time”, but said that many considerations would enter into his final decision. Adams had strong hopes that Baade would stay at Mount
FIG. 3. Mount Wilson Observatory staff photograph, 1939. Frederick H. Seares is fifth from the left in the front row, then John A. Anderson, Walter S. Adams, Paul W. Merrill, and Alfred H. Joy. Milton L. Humason is second from the left in the rear row, Edwin Hubble eighth (behind and between Seares and Anderson), Rudolph Minkowski tenth (behind and between Anderson and Adams), and Walter Baade eleventh (behind Adams) (courtesy of the Henry Huntington Library).
Wilson, but realized he might not.  

More practically, the Mount Wilson director had Humason, Baade’s closest friend on the staff and at the same time Adams’s trusted confidante, take his friend for a “ride in the country” and tell him all the wonder that might be. Adams was committed to remaining as director until the 200-inch telescope was completed, then forecast for 1941, when he would reach retirement age. Everyone, including Humason and Baade, expected that Hubble would then become director of Palomar. Seares, the Mount Wilson assistant director, was slated to retire before that, and if Baade would take out his first papers to become a U.S. citizen, Humason said, Adams would have him appointed to that post, and he would be in line for the directorship himself three years later. The two lectures Baade had given in Washington had convinced Merriam and the Carnegie directors that Adams was right, and that the German researcher (if he became an American citizen) was just the man for the job. Later Adams, “a reserved New Englander”, himself called Baade in and confirmed the essentials of this unofficial offer.  

Baade had no intention of becoming a naturalized American then or later; he always intended to return to Germany some day, as he did fifteen years after Hitler committed suicide in his bunker, his Third Reich in ruins, swept away by the victorious Russian, American, and British armies. But in 1937 the German astronomer realized clearly just how badly Adams wanted to keep him at Mount Wilson. Nevertheless, Baade was strongly attracted emotionally to his homeland. In January and again in March he wired Schorr that he would accept the Hamburg directorship if the government would build a 32-inch Schmidt telescope for him and provide adequate funds to operate it effectively. With it, he said, he could take the lead from the Americans, and show that the land where the Schmidt camera was born could forge ahead of them without spending millions. As Baade reported, the Mount Wilson astronomers, led by Hubble (he carefully wrote, although he himself was a full participant in the project), were planning a large Schmidt themselves, which would certainly be built at Palomar. Baade was genuinely attracted by the idea of going back to Hamburg, and building his own telescope there.  

Finally, in late June, the Hamburg authorities, acting on the advice of the German government, formally offered Baade the directorship. He immediately took the letter to Adams. The canny Mount Wilson director was expecting it and had a plan ready for it. Baade’s salary had just been raised to $4,000 on 1 January; although the Hamburg offer was for the equivalent of $7,500, he would stay at Mount Wilson if it were raised to $6,000 a year. To find the necessary additional money, Adams shifted the salary of Sinclair Smith, a young Mount Wilson staff member who was assigned to work almost entirely on the Palomar project, from half-time at each institution to full-time on the Rockefeller funds. This freed $2,000 a year for Baade’s raise from the Carnegie Institution money. Mason, the former Rockefeller Foundation president now in place at Caltech in charge of completing the 200-inch, agreed readily by telegram from his vacation in
Rhode Island, while Merriam, who was at Yosemite en route to another vacation spot in northern California, raised a few petty objections but soon had also agreed to the deal, under Adams’s careful handling. The ascetic Yankee was the only one of the three actually at work that summer.59

Baade cabled Schorr, “With heavy heart I have decided for Mount Wilson, hoping the homeland will understand”; the old director could only reply “Schade, Schade” (“What a shame!”).60 In a consoling letter to Schorr, Adams diplomatically spelled out the reason Baade had finally decided to stay. At Mount Wilson he already had the 100-inch telescope, with the 18-inch Schmidt at Palomar; without a doubt he would soon have the 200-inch and the 48-inch Schmidt, all in the fine observing climate of California. In Germany he had been promised a 32-inch Schmidt in a climate he knew was much poorer, and although Adams did not mention it, if war came (as it did, two years later), Baade might never use the telescope, even if it were built. The attraction of his science had won out over the tug of his homeland.61

Baade himself wrote a long, unconvincing letter of explanation to the Hamburg Education Board which had formally offered him the directorship, trying to explain his decision. He claimed it would have been easier for him to return to Germany, but that he was staying in Pasadena “in the German interest”, to make sure his native land was represented on the staff of the 200-inch. According to him, this had been the desire of “the official German side”, probably meaning that one of his friends among the officials at the consulate in Los Angeles had urged him to stay. Baade’s chief aim in writing this letter was to keep the plan for the Hamburg 32-inch Schmidt alive for his colleagues, and he signed it under the required official closing, “Heil Hitler!” Baade never was a Nazi, but like Otto Heckmann (eventually appointed Hamburg director in January 1942), and every other German astronomer who stayed in the Third Reich, he realized that he would have to follow their forms to be heard and survive. In a long, emotional letter to Schorr, Baade wrote that his time at Hamburg had been the most beautiful years of his life, that he had not sold out for American dollars, and that he would work with the next director, whoever he might be, to make the 32-inch a reality (as he did after the Second World War). Just a few days later, Baade and Hanni joined in an even more heartfelt message of congratulation to Schorr, as “we two married Bergedorfers in distant Pasadena”. Baade could be extremely sentimental in German, but equally realistic in his decisions. Apparently he never again considered leaving the California big telescopes until after his retirement. By the spring of 1938 he could see tremendous progress on the 200-inch; the dome was nearly finished “like a gigantic cathedral”, and the mounting was approaching completion at the steel works in Philadelphia. Construction of the Palomar 48-inch was to begin that summer. Baade knew his decision had been the right one, although his heart was with the German troops invading Austria.62
8. Photography with Red Light

In the 1930s all the big photographic companies, like Agfa in Germany and Eastman Kodak in the United States, were developing and improving their orthochromatic (green- and yellow-sensitive) and panchromatic (red-sensitive) films, and plates as well. Baade kept closely in touch with these developments, and with the prestige of Mount Wilson Observatory behind him often succeeded in getting some of the most sensitive emulsions, coated on glass, directly from the manufacturers' laboratories. They were tricky, requiring hypersensitization with just the right combinations of water, ammonia and alcohol immediately before use, and all too likely to fog on the slightest provocation, but his careful, experimental approach made him an expert with them. Once he received a shipment of especially good plates, he hoarded them carefully, using them for the most important problems he was working on and never wasting them. He also used panchromatic film (and a red filter) with the 18-inch Schmidt telescope at Palomar to survey a large region around the galactic centre.

His approach paid off. The great advantage of red light is its space penetration; interstellar dust absorbs and scatters blue light much more effectively than red. Hence Baade's direct photographs penetrated the nearby dust clouds in the galactic plane, and revealed the gaseous nebulae and globular clusters behind them. The nebulae themselves emitted the red $\text{H}\alpha \lambda 6563$ emission line especially strongly, making that wavelength region especially well suited for detecting them and photographing their structure. With these panchromatic plates, especially an experimental emulsion called "Hα Special" which C. E. K. Mees, the head of the Kodak Research Laboratory, sent him, Baade found that he could record many more stars in globular clusters near the galactic centre, and outside the clusters in the field itself. He systematically covered this whole region with long-exposure photographs, and discovered the least heavily obscured region at galactic longitude $329^\circ$ (in the coordinates of that time, now relabelled $1^\circ$), galactic longitude $-4^\circ$, now known as "Baade's window". On the other hand, in the region about the globular cluster NGC 6553, which he described as "situated close to the center of our [Galaxy]", his plates showed graphically the contrast between the few stars visible in the blue, and the many more in the red, resulting from strong extinction by dust. Baade announced these new results in a paper read for him at the meeting of the American Astronomical Society held at Williams College, Massachusetts in September 1937. He did not go himself (September is the best observing month at Mount Wilson), but sent slides which Duncan, the professor at Williams and a frequent visitor to Mount Wilson as a guest observer, showed as he described these results. It was at this meeting that Baade first became a member of the Society, six years after he had joined the Mount Wilson staff.

Then, in the summer of 1938, Baade was able to return to Europe, to attend the triennial General Assembly of the International Astronomical Union held at
Stockholm that August. With his now higher salary, he could take his wife with him. Germany was still not allowed to adhere to the Union, founded by the victors of the First World War soon after its close, but Baade was invited as a "consultant" from America, and eight other German astronomers were allowed to attend as "guests". Baade and Hanni spent a month in Germany before going on to Sweden; he visited his mother and brother in Minden for two weeks, then joined his wife in Hamburg where she had been visiting her family. He met Schorr at the observatory in nearby Bergedorf, and had several talks with him and his other old friends there. Then he went on to Sweden and the week-long General Assembly. There at a large "Discussion of galactic structure" he gave an invited talk on his work with red-sensitive plates at Mount Wilson. This time he showed the slides himself and did the talking (in English — it and French were the two official languages of the Union).65

After the General Assembly Baade and Hanni returned to Germany and continued their vacation, "visiting old friends and talking politics". The "politics" meant the crisis over Czechoslovakia. Hitler was determined to take it over, as he had Austria in the spring, and was willing to go to the brink of war to do so. In September the tension reached new heights. War seemed about to break out. Baade abandoned his plans to attend a big German scientific meeting and to visit the Zeiss works in Jena, to discuss glass disks for a corrector plate for the planned 48-inch Schmidt. Instead he and his wife departed two weeks early on the Europa just days before British Prime Minister Neville Chamberlain flew to Munich and handed Czechoslovakia over to Hitler's tender mercies in exchange for "peace in our time", which was to last for less than a year.66

Shapley, who had been at Stockholm, had been impressed by Baade's work on galactic structure with red-sensitive plates, as everyone was who had seen the slides. He pumped Baade for advice, and turned it over to his underlings in South Africa and in Cambridge, but they were not nearly as successful with it as the master observer himself.67 A few years later those panchromatic emulsions, and the direct photographs he took with them, were to play a key role in Baade's recognition of the two stellar populations.

9. The Dedication of McDonald Observatory

Soon after his return to Pasadena from Stockholm and Germany, Baade received an invitation from Otto Struve, director of Yerkes Observatory, to take part in a symposium at the dedication of McDonald Observatory, to be held on its site in the mountains of west Texas the following year. Its 82-inch reflecting telescope, built under the auspices of the University of Texas, thanks to a generous small-town banker who had amassed a fortune before he died, would be the second largest telescope in the world when completed. Struve had brought about a co-operative arrangement under which he and the other Yerkes astronomers would operate McDonald Observatory, for Texas had no observing astronomers and
very few research scientists of any kind. For the dedication he was bringing many of the top American astronomers to isolated Mount Locke for a symposium on current astrophysical research, then mostly on stars, but including papers on galaxies by Hubble and Baade. Struve had hoped to bring several outstanding European astronomers as well, but Hitler’s continuing threats of war kept most of them at home.

Baade gladly agreed to take part in the dedication, which he and Struve had discussed in Stockholm. They both wanted to make it possible for Albrecht Unsöld, the rising young Kiel astrophysicist who had come to Hamburg to work with Baade in 1930, to combine a trip to America for the dedication with a stay to use the big telescopes at Mount Wilson and McDonald Observatories. However, Adams, the Mount Wilson director, was distinctly cool to this idea. He, most of the members of his staff, and all the Carnegie Institution of Washington officials and trustees were strongly anti-Hitler and pro-British by this time. Adams accepted Baade as a great astronomer, but he had no intention of making Carnegie funds or Mount Wilson telescopes available to a visitor from Germany. Thus Unsöld came to America, attended the McDonald dedication and observed there with Struve, but did not get to Pasadena or Mount Wilson Observatory. The only other two Europeans who took part in the Texas symposium were Edward A. Milne, the theoretical astrophysicist from England, and Jan H. Oort, the galactic structure and dynamics expert from Leiden.68

Baade’s talk at the dedication, delivered to an audience of some fifty or sixty astronomers, many of them the research leaders of that generation, including Shapley, Adams, Struve and William H. Wright, the director of Lick Observatory, and the next, was a combination of a detailed description of his photometric methods, and an application of them to a specific galaxy. First he gave a long review of all the problems of photographic photometry, his ways for overcoming them, and the checks and comparisons he applied to be sure he had overcome them. He emphasized the difficulties in trying to measure the stars’ brightnesses down to magnitude 21, but explained his painstaking procedures for reducing the errors to the order of ±0.07 magnitude for a single plate over the whole range, and only ±0.03 near the faint limit.

The application was to IC 1613, the dwarf galaxy within the Local Group which Baade himself had identified as a star system like the Magellanic Clouds at Hamburg a decade earlier. As soon as he joined the Mount Wilson staff he began observing it with the 100-inch telescope, taking long-exposure photographs and searching them for variables. Baade had set up his magnitude scale in Selected Area 68, midway between IC 1613 and NGC 6822, the similar dwarf galaxy which Hubble had studied even before M 31. One of the important checks Baade mentioned in his talk was the photoelectric measurements of the magnitudes of several of the stars in SA 68, which he had arranged for Stebbins and Whitford to make at the 60-inch reflector, on one of their visits to Mount Wilson.
Then, with the magnitude scale set up in SA 68, Baade transferred it to local standards in and near IC 1613, and measured the varying brightnesses of the Cepheid variables he had discovered on his long series of 100-inch plates, dating back to 1932. Thus he was able to derive and plot the period–luminosity relationship for the variables in this dwarf galaxy with periods ranging from 3 to 146 days. In his presentation he showed a slide of this plot, and compared it with published plots for the Cepheid variables in the Large and Small Magellanic Clouds (from Harvard), M 31 and NGC 6822. This comparison showed that his errors of measurement were smaller than in any previously published period–luminosity relation. The distance he derived for IC 1613 was practically the same as that of M 31, but the former was only one five-hundredth as luminous as the giant spiral. IC 1613 was a true dwarf galaxy. But it contained stars just like those in M 31, not only Cepheid variables but high-luminosity blue stars and diffuse emission nebulae, he emphasized. He was close to the population concept he was to publish five years later.69

After the McDonald symposium Oort went on to Pasadena, where he took part in a conference on the structure of ‘nebulae’ (galaxies), especially organized to take advantage of the presence in America that summer of the Swedish astronomer Bertil Lindblad and himself. The direction of rotation of spiral galaxies (arms trailing or leading), on which Hubble and Lindblad had opposite opinions, was the main topic of discussion, but Baade and Oort were both more interested in the analogies between our Galaxy and other spirals. This conference, which Mayall also attended, gave Baade and Oort the chance to get to know and begin to respect one another. They were to become the two world leaders in galaxy research after the Second World War.70

10. Paris Conference

Soon after the McDonald Observatory dedication, Baade travelled to Europe again for his second visit in two years. This time he went to participate in a select conference on novae, supernovae and white dwarfs, held in Paris in July 1939. Sponsored by the wealthy Princess de Polignac, attendance was by invitation only, with all expenses paid from the funds she provided. Baade naturally planned a stay in Germany after the week-long conference to visit his mother again, and to see the Hamburg astronomers. His wife did not accompany him; her travel would have been at their expense.

There were only fifteen participants at the conference, headed by Arthur S. Eddington, Henry Norris Russell, Knut Lundmark, and F. J. M. Stratton, a long-time nova observational spectroscopist of the older generation. But most were the younger, upcoming theorists and observers, including Bengt Strömgren, Gerard P. Kuiper, Subrahmanyan Chandrasekhar, Cecilia Payne-Gaposchkin, Bengt Edlén, and Pol Swings. Only one French astronomer was present, Henri Mineur, and no other Germans but Baade. Walter Grotrian had originally been
scheduled but the rising tension between Germany and France kept him at home. They had sessions every day for a week, sitting around one big table in a room at the Collège de France, with one or two talks each morning, and another in the afternoon, each followed by general discussion of the topic. Russell, the chairman of the conference, gave a general introduction on Monday morning, and Eddington closed it on Saturday morning with his version of the theory of white dwarfs, which unfortunately was wrong because he could not accept the upper limit to the mass of a white dwarf that Chandrasekhar had derived using the correct theory! Baade was at his best in this type of conference, a small group of top scientists discussing their results and theories in an informal setting.71

His talk was on Thursday morning, on supernovae. In it Baade emphasized that all the observational data that had been obtained since his and Zwicky’s first paper fully confirmed the uniqueness of supernovae — that they were quite different from ordinary novae. He gave full credit to Zwicky for his work with the 18-inch Palomar Schmidt. It had led to the discovery of nine supernovae, all followed up by Baade with the big telescopes at Mount Wilson. As a result there was a coherent observational picture of the frequency of supernovae (roughly one per “typical” spiral galaxy per 600 years, from the preliminary data), their light curves, their average absolute magnitude at maximum light ($M_\text{R} = -14.3 \pm 0.3$ with a dispersion of 1.2 mag.), and their energy release. But their spectra, which showed a continuum with very broad, unidentified emission bands, had not yielded to analysis, so the physical nature of the supernovae remained unknown.

Zwicky would continue the supernova search to the end of 1939, Baade said, and then re-discuss all the data, which would be much more complete. As a result, the parameters of supernova magnitudes and light curves would be more accurately known, but the fundamental problem of their nature would remain. Baade carefully calculated the total energy released in each of the three supernovae that had been caught before maximum light, deriving numerical values comparable with the total “heat content” (potential, thermal and ionization energy), indicating a “radical change in the constitution of the star”, he said. However, by itself the result could not confirm or deny the idea that a supernova represented the collapse of a gaseous star to a “degenerate state” (a white dwarf), as Milne and Chandrasekhar had suggested. Baade did not even mention Zwicky’s idea that a significant amount of rest mass disappeared in a supernova outburst; all his own estimates had been on the conservative side, while Zwicky’s had been in the opposite direction. At the end of his talk, Baade described the work he had begun on the Crab nebula, and how it might help to solve some of these riddles in the near future. Baade’s paper was a masterful observational summary of the supernova phenomenon as it was known then, largely as the result of his, Minkowski’s and Zwicky’s efforts.72

After the conference, Baade hurried to Minden. His mother was in failing health, and he knew each visit might be his last chance to see her. He wrote to
Schorr at Bergedorf to arrange to meet him there. However, his former director was going to a meeting of the German Astronomical Society at Danzig, the “free city” which Hitler was demanding be “returned” to the Reich. Schorr urged Baade to come to the meeting of the society to which he had belonged for years, and discuss astronomy with his friends who would be there. Holding the meeting in Danzig had been a provocative act; Baade knew that no English or American astronomer would attend, as some always had in previous years. He replied that he had to stay in Minden to take part in his aunt and uncle’s golden wedding celebration; “they would kick [him] out of the family if [he] were not there for it”, he replied to Schorr in his picturesque style. Baade always could find a ‘reason’ for avoiding a public stand that would get him in trouble in America or Germany. But in a long letter to Schorr, he expressed his strong support for Heckmann for the Hamburg directorship, as a true scientist, and his opposition to Johannes Helligreich, the Nazi education officials’ candidate for the post. It would be a catastrophe if he succeeded to the directorship, Baade wrote; there would be no fresh initiatives, and only lethargy at the observatory. Schorr agreed fully with Baade, and assured him that the Hamburg faculty felt the same way, and would block the appointment. They planned to meet at Bergedorf in late August, after Schorr’s return from Danzig.

However, as the crisis escalated Baade, who had earlier thought there would be no war, now decided to get out of Europe while he still could. He advanced his departure date, hastened back to France, and managed to get to Le Havre and board the Île de France before the French mobilized their army and closed their borders. An Austrian biology professor at Stanford whom Baade knew had not moved quite fast enough; he was trapped inside France and “landed in a concentration camp”. The war began on 1 September, two weeks after Baade sailed. Home safe in Pasadena, Baade inquired anxiously about Schorr’s three sons, who were all of military age, and railed against “the holy democracy” (America) and President Franklin D. Roosevelt’s policy of “cash-and-carry” neutrality. He made it very clear to Schorr, if not to Adams, that his sympathies were all with the German people. But Baade was never to see his former director again, nor his own mother.73

11. Nova and Supernova Shells

The Paris conference and preparing his paper for it revitalized Baade’s interest in supernovae. He was convinced that the Crab nebula was a supernova remnant; hence examining closely every peculiar nebula might turn up another, previously unknown remnant. Baade’s skills with the telescope enabled him to obtain excellent direct images, showing the finest detail the atmospheric conditions would allow. Mount Wilson frequently has superb seeing, and he made the most of it. He had quickly grasped the fact that the red-sensitive plates with a sharply defined red filter, isolated a narrow spectral region around the strong nebular

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emission lines Hα, [N II] λλ6548, 6583, ideal for photographing these objects.

One of the objects to which he early applied this method was “Campbell’s hydrogen envelope star”, a nearly stellar object with strong emission lines. From his plates and spectra taken with the 36-inch Crossley reflector, Curtis had thought in 1918 that it was a tiny planetary nebula, but most astronomers called it a nebulos star, and it was included in both the Bonner Durchmusterung and Henry Draper star catalogues. In 1940 Baade obtained direct photographs with the 100-inch reflector in fine seeing which clearly showed the object is a ring about 5” in diameter, a small version of the much larger, more famous Ring nebula in Lyra, NGC 6720. The narrow wavelength band he used had suppressed the light of the star, and kept its overexposed image from hiding the ring. Now there was no doubt it was a planetary nebula. Probably Baade was disappointed that it was not more abnormal, potentially a supernova, but he sent copies of his photographs to Struve and Wright. They had both worked on the “hydrogen envelope star”, and were suitably impressed. Baade never bothered to publish his result.74

Another, more interesting object was the shell around Nova Herculis 1934. A tiny nebulosity had first been glimpsed (by skilled observers with large telescopes) in 1938, and Baade succeeded in photographing it in Hα, [N II], and also in the green [O III] lines (using a yellow filter and the natural blue photographic plate sensitivity to isolate a narrow spectral region around them). The little nebula was ellipsoidal rather than circular in the sky; evidently the shell was elongated. In August 1940 Baade obtained these images, the dimensions of the shell were 2.7” × 3.5”. Earlier, observing it visually at Yerkes Observatory, Gerard P. Kuiper had thought the nova had split and become a double star. As Baade realized from his direct plates, Kuiper had seen the two ends of the shell and interpreted them as two components of a binary; the position he had measured and reported was in fact the position of the major axis of the little nebula. The self-confident Kuiper found it hard to conceive that he could have been mistaken, but as the nebula expanded and became fainter, by 1942 he clearly saw the shell and admitted to Baade that “[y]our description is certainly closer than my own”. The Mount Wilson astronomer was always sympathetic, putting the best possible construction he could on earlier, less accurate observational results. He never gloated nor boasted, and except for Zwicky, a notoriously difficult person, never made an enemy.75 Baade did publish his results for Nova Herculis in two short papers, including the distance he derived to it by comparing the angular expansion of the shell with the spectroscopically measured radial expansion velocity. It was a ‘normal’ nova, as he knew it would be, with absolute magnitude at maximum light $M_P \approx -6.5$. His direct plates showed a different distribution of [N II] emission over the shell from [O III], indicating the ionization conditions, like the shape, were not circularly symmetric, but cylindrical.76

The most interesting object for Baade, and the one for which his direct exposures in various wavelength regions showed the most structure, was the Crab nebula. He knew it was a supernova remnant, and had encouraged Mayall to
publish his results on it. When Oort had come to America for the McDonald dedication in 1939, and afterward to Mount Wilson to work with the telescope and discuss research, Baade learned that he also was extremely interested in nova shells and in this supernova remnant. Oort and Mayall were still not quite as certain as Baade was that it was one. They were concerned about uncertainties in the Chinese accounts of just where in Taurus the “guest star” had appeared, and the difference between Mayall’s measured radial velocity of expansion (1300 km/sec) and the widths of the broad emission lines in observed supernovae (given as 6000 km/sec in some early papers). Baade encouraged them to go on trying to learn more from the early records, but reasoned correctly that the Crab nebula was a unique object and as such it had to be the supernova remnant.⁷⁷

After he got back to Holland in early September 1939, just days after the outbreak of the Second World War, Oort encouraged two of his fellow faculty members, professors of Chinese and Japanese, to dig further into the old writings, and himself tried to look into Arabic, Jewish and Byzantine sources. The mails between America and the continent of Europe were often delayed for months, and after the Germans occupied Holland in April 1940, Oort knew that it might be completely cut off at any time. He suggested that Mayall publish the Chinese and Japanese translations in the United States. The young astronomer discussed the idea with Wright and the other senior astronomers at Lick Observatory and with Baade; they all agreed he should publish what he could in a joint paper with Oort. The Dutch astronomer agreed and finally in June 1941 sent the completed manuscript of a paper by J. J. L. Duyvendak, the ancient Chinese scholar, containing the relevant translations and their interpretation. It reached California in early August, and all the astronomers who read it now agreed that the Crab nebula was certainly the supernova remnant. Mayall wrote a companion paper, analysing the astronomical evidence from Duyvendak’s translations as well as the corroborative material from Japanese records. He sent the paper, naming Oort and himself as coauthors, to Holland, but it never got through. The Leiden astronomer had authorized him to publish it if he did not hear from him, and Mayall did so in 1942. Oort never saw it until after the Germans had been driven out of Holland and the war had ended; he was very pleased with it. Duyvendak was still alive too, and was pleased with his paper also; a true scholar, he nevertheless immediately sent Mayall a list of corrections which the latter had not caught in the proofs.⁷⁸

Baade was holding up his own paper on the Crab nebula, waiting until after he knew Duyvendak, Oort and Mayall’s work would be published. He was particularly enthusiastic that Duyvendak’s translations revealed to him for the first time how very bright the supernova in A.D. 1054 had been. The Chinese had compared it with various planets and bright stars, making possible a reconstruction of its light curve. From this, Baade classified the supernova of nine hundred years ago as type I. His first direct photographs of the Crab nebula with narrow-band red filters revealed clearly its true structure, a network of filaments emitting
Ha, [N II], [O III] and other typical nebular forbidden lines, surrounding and interpenetrating a diffuse, amorphous nebulous region which emitted only a continuous spectrum, with no emission lines. Minkowski obtained spectra with the slit of the fast spectrograph on the 100-inch placed across some of the brightest filaments; they confirmed Baade’s statements beautifully. They searched for an "exciting star", which they thought must be emitting high-energy ultraviolet photons that ionized the nebula. There was a close pair of sixteenth magnitude stars near the centre of the Crab nebula. Baade found one too red, and Minkowski confirmed that it was an F or G star, too cool to be a candidate. The other was somewhat bluer, and on Minkowski’s very low-dispersion, faintly-exposed spectrogram showed no absorption lines (meaning that any actually present must be very weak), indicating it could be a hot star. Minkowski tried a spectrophotometric approach, in which he derived the “color temperature” of the diffuse, amorphous nebulosity as approximately 36,000 K. The difficulty was that it should have shown an easily observable Balmer discontinuity, but did not. Minkowski tried to explain this away and thought he could by stretching all the observational errors and unknown interstellar reddening to their maximum values. He ended up with the blue star as an object with a fantastically high temperature, T = 500,000 K, and a correspondingly small radius, R = 0.02 R\_so. This fitted well with the idea which Chandrasekhar had put forward, that a supernova outburst represented the collapse of a ‘normal’ star to a white dwarf.\textsuperscript{79}

However, this picture left completely unexplained the absence of any emission lines from the diffuse nebulosity, and the source of ionization and excitation of the gas in the filaments. Baade was rightly sceptical about this analysis but discussed fully the published proper-motion measurements of the expansion, which he now clearly identified with the filamentary system. The 120-year discrepancy between when the Chinese observed the bright supernova and when the present rate of expansion, projected backward as constant, indicated the remnant would first have appeared, Baade stated must have resulted from an initial rapid acceleration, followed by coasting at constant velocity. He fully confirmed Mayall’s value for the distance to the Crab nebula and the supernova’s magnitude at maximum light. The older German was generous in his praise of the young Lick spectroscopist, and continued suggesting further work he could do on the Crab nebula and other supernova remnants.\textsuperscript{80}

Baade actively continued his investigations of historic supernovae. He put his own knowledge of Latin, Italian and German to use to comb through old records of the observations of Tycho Brahe’s bright ‘nova’ of 1572 and Johannes Kepler’s of 1604, confirming his earlier published statements that they both were supernovae, and reconstructing their light curves. From these he classified them both as type I supernovae. At the position of Kepler’s supernova he found, with very long narrow-band red exposures, wisps of nebulosity which he identified as its gaseous remnant. It was in a very heavily reddened region in Ophiuchus and the nebulosity did not show at all in the blue. Minkowski obtained two
spectrograms, each a four-night exposure totalling sixteen hours, which showed 
Hα, [N II], [S II], [O I] and [O III], quite similar to the line spectrum of the Crab 
nebula filaments and confirming the identification. Baade also searched very 
hard for a remnant of Tycho’s supernova, but did not find one. From the light 
curve he was convinced that a remnant must be present, just a little too faint to 
detect with the 60-inch, which he had to use since this object is at declination 
+64°, too far north for the 100-inch to reach. At the end of his paper Baade 
concluded that there was every reason to hope that it would be found when the 
200-inch reflector went into operation.81

Indeed, just a few years later, Baade and Minkowski were to begin observing 
supernova remnants with the new reflector, and to become the world leaders in 
identifying newly discovered radio sources with them. They had the jump on 
everyone, not only in telescope aperture, but in observing skills and knowledge 
of supernovae and their remnants. Though their idea about photoionization of 
the remnant nebulosity by the remnant star was wrong, they were quickly to 
grasp the new concepts of synchrotron radiation, and Baade and Oort were to 
confirm them in the Crab nebula.

12. Sculptor and Fornax Dwarf Galaxies

Baade had long been interested in dwarf galaxies. His aim was to learn all the 
properties of all the types of star systems in the universe, rather than hoping that 
their absolute magnitudes had a relatively small dispersion about a well-defined 
mean, as Hubble did. At Hamburg, Baade had thoroughly investigated IC 1613, 
which he proved was a dwarf irregular galaxy of the same general type as the 
Large Magellanic Cloud and NGC 6822. Since these dwarfs by definition have 
relatively faint absolute magnitudes, they are difficult to find at large distances. 
Encouraged by Baade, Zwicky had begun inspecting all the films he took with 
the 18-inch Schmidt for dwarf galaxy candidates. Soon he started taking high-
latitude fields in a systematic search for such objects. The arrangement was that 
Baade would use the 100-inch to investigate the candidates Zwicky found. By 
1940 when Hubble wrote a semi-technical article on galaxies, Baade had checked 
six of the best candidates, and three of them had turned out to be dwarf irregular 
galaxies. Unfortunately, although Hubble described the arrangement correctly 
in the text, in the captions of the photographs he called one of them “Baade’s 
system in Sextans”, and the other “Baade’s system in Leo”. This infuriated 
Zwicky. He demanded, and got, a handsome retraction. This incident, together 
no doubt with the fact that Baade had been an invited star of the Paris collo- 
quium on supernovae, while Zwicky had been excluded from it, brought growing 
tension between the two into the open. Zwicky, a beginner in astronomy 
who had not been very successful in any branch of physics he had tried earlier, 
was highly insecure. Hungry for recognition, he struck out against the “high 
priests and sycophants” who had achieved it. Baade was rapidly becoming a prime
target; Zwicky, an independent, strongly anti-Hitler Swiss, could not hurt the
dictator but seemed to regard Baade as a convenient proxy for him. They never
 collaborated after 1940, and Zwicky afterward constantly criticized Baade, as well
as Hubble, Minkowski, the “high pope of American astronomy Henry Norris
Russell”, Allan Sandage, Maarten Schmidt and many other famous astronomers.82

Because of his interest in globular clusters, dwarf galaxies and related ob-
jects, Baade undoubtedly read two papers by Shapley, published in 1938, with
keen attention. Both described stellar systems “of a new type”, discovered on
plates taken with telescopes at Harvard’s Boyden Station in the southern hemi-
sphere, and shipped to Cambridge for Shapley and his assistants to inspect. He
called the first discovered, in the constellation Sculptor, a “large rich cluster” of
stars, quite extended, with brightest stars about nineteenth magnitude, number-
ing roughly a thousand, many more only slightly fainter than that. The Sculptor
system had no emission nebulae, no supergiant stars, no “open [galactic] clus-
ters”, but in form and size it was like the Magellanic Clouds. On the other hand
the luminosity function of the stars it contained resembled that of a globular
cluster, with brightest stars $M_{\text{pg}} \approx -1.5$ (Shapley had no red or yellow plates of it,
from which to measure colour indices), but its size and low surface brightness
were quite unlike those of any globular cluster he knew. The second object, in
Fornax, was similar, but the brightest stars were even fainter, indicating it was
more distant than the Sculptor ‘cluster’. He planned to have more plates taken
with the 60-inch reflector at the Boyden station to look for variable stars, Shapley
wrote. Both these systems, he stated, had some properties in common with globu-
lar clusters, some with elliptical galaxies, and still others with the Magellanic
Clouds. At the end of his paper he mentioned that the much more distant Virgo
cluster of galaxies was known to contain a number of seventeenth magnitude,
small, low surface-brightness objects, which he speculated might possibly be
“clusters of the Sculptor-Fornax type” much further away.83

As Baade wrote to Shapley in February 1939, as soon as he and Hubble had
read Shapley’s paper they had begun “play[ing] around a little” with the two
new systems.84 They probably each decided independently to find out more about
the two systems, and then agreed to share the project. The 100-inch was a larger
and much better telescope than the Boyden 60-inch, and although they could
only get at these far southern objects (both at $\delta = -34^\circ$) for a few hours each
night, they could take good direct photographs which went considerably fainter
than Shapley’s. Baade and Hubble quickly confirmed Shapley’s descriptions,
but went far beyond them. They found some forty variable stars in the Sculptor
system, nearly all of them RR Lyrae stars, as shown by the fact that their periods
were shorter than a day. With an accurate magnitude system established by Baade
by comparison with SA 68, the distance modulus turned out to be $m - M = 19.6$,
corresponding to a distance of $8.4 \times 10^4$ pc, an absolute magnitude $-10.6$, and
linear dimensions $1000 \times 1100$ pc. Two variable stars had longer periods, of the
order of a week, and were brighter, corresponding to $M_{\text{pg}} = -1.4$. Though Baade
and Hubble could not determine light curves for these two variables from such a short series of observations, all their properties were consistent with their being Cepheid variables, and tended to confirm the distance derived from the RR Lyrae variables. There were no supergiants, very blue, or very red stars (they had taken direct exposures in yellow light), but large numbers of yellow giants fainter than $M_{pg} = -1.4$. Neither Sculptor nor Fornax had a well-defined nucleus, or much structure; they were both diffuse, with a uniform distribution of stars.

The Fornax system was more distant, and Baade and Hubble’s plates showed no RR Lyrae variables. But it contained two globular clusters, both of which were partly resolved into stars in their outer regions. The brighter globular cluster had even been catalogued earlier, as NGC 1049. Baade and Hubble could see that its brightest stars were essentially the same apparent magnitude as the brightest ‘field’ stars in the Fornax system. From these brightest stars, Baade could determine the distance to NGC 1049 using the same standard methods originated by Shapley, which he used to measure the distances to globular clusters. Its distance modulus was $m - M = 21.4$, corresponding to a distance of $1.9 \times 10^5$ pc; this was thus the distance of the Fornax system in which it lay. Its size then came out to be $1900 \times 2800$ pc, and its absolute magnitude $M_{pg} = 11.9$. These systems were comparable in luminosity and size with IC 1613, true dwarf extragalactic systems, Baade and Hubble wrote. But unlike IC 1613 they contained no supergiants, no highly luminous blue or red stars.

But, they emphasized at the end of their paper, this lack of supergiants was not unique to the Sculptor or Fornax system. M 32 and NGC 205, the round and elliptical dwarf galaxy companions of M 31, shared it, as was shown by the fact that they were not resolved into stars, though high-luminosity ones brighter than $M_{pg} = -1.5$ would have already been detected, if present, with the 100-inch. A long footnote at the end of their paper stated that “as a working hypothesis it could be assumed that ... supergiants are lacking in the [galaxies] usually described as ‘elliptical,’ [and] in the central regions of ‘early’-type spirals”. But, they abruptly declared, “discussion of the data now available would be largely speculative, and hence of little permanent value”. That last sentence undoubtedly had been written by Hubble; his idea of science was to report data and check theories. The previous description of elliptical galaxies containing exactly the same types of stars as globular clusters must have been Baade’s. His goal was always to draw new insights from observational data, guided by theory, and to go beyond current theories. Just five years later, in the midst of the Second World War, left on his own without Hubble to hold him back, Baade was to prove that this same supergiant-free population of stars was present in M 32 and NGC 205, and to describe it as population II, his greatest discovery.

13. The Second World War and the Two Stellar Populations

America entered the Second World War on 7 December 1941, while Baade was completing his paper on the Crab nebula. After the Japanese attack on Pearl
Harbor, the United States was suddenly at war with all the Axis nations, including Baade's homeland. Now there was no hope of his communicating with his family in Germany, nor with any of his astronomer friends there, such as Unsöld, whom he had helped get to America briefly in the spring of 1939, or Karl Wurm and Erich Schoenberg, who had hoped to see him in their country that fateful August. With the coming of the war the German consulate in Los Angeles was closed, and the professional diplomats were sent home. German citizens like Baade (and there were many more in Hollywood) had to register and were restricted in their movements, but were not sent to concentration camps in the interior of the country, like the Japanese and their American-born children.

Even before the United States entered the war, Mount Wilson Observatory, like many university and other research centres, had begun an in-house government-supported weapons development program. Most of the older astronomers, including Minkowski, who had become a naturalized citizen in 1939, just as soon as he could, knew enough physics to do optical design and testing of range finders, gun sights and the like, and to calculate fields of fire of American and German bombers, and how best to defend or attack them, respectively. Baade, an enemy alien, could not participate in any of this work, and was not supposed to know what was going on. Three of the younger astronomers on the Mount Wilson staff went on leave to work on rocket development at a large project centred at Caltech, which had test-firing ranges in a canyon near Pasadena, and at Inyokern in the Mojave desert.

In April 1942 military security briefly caught up with Baade. The army district issued a curfew order requiring all enemy aliens to be in their homes between the hours of 8 p.m. and 6 a.m. Literally interpreted, it meant that Baade could not go to Mount Wilson to use the telescope, and was thus barred from observing. Adams, who was to stay at his post of director past retirement age until the war ended, sent a letter to Vannevar Bush, who had recently succeeded Merriam as head of the Carnegie Institution of Washington. Bush was the czar of American science during the Second World War, the head of the Office of Scientific Research and Development, so his advice counted. Adams wrote that Baade had taken out his first papers in 1939, waiting that long (he said) partly because he wanted to protect his relatives in Germany, and partly because he was an impractical scientist! One of the options Adams considered was sending Baade to McDonald Observatory for the duration; it was far enough from the coast that these army security restrictions did not apply there. But Adams wanted to keep him at work at Mount Wilson if he possibly could, because he was so productive. Baade, who no doubt had his observing plans completely mapped out for the 100-inch, certainly did not want to make this move, although Kuiper, who as one of the main users of the McDonald telescope, visualized himself as being 'traded' to Mount Wilson for Baade, was eager to go ahead with it. Bush, after a lot of good advice about making very sure that Baade was completely
isolated from the war project (and that everyone knew that he was, to keep Mount Wilson's and Carnegie's reputations spotless), advised Adams simply to wait and let the matter work itself out, "take the inconvenience, and look for the proper time to raise the question". The Mount Wilson director, always eager to push matters along, was already pulling strings long before he received Bush's advice by air mail. Adams and Seares, both with good old American names and widely known in Pasadena, wrote letters strongly vouching for Baade. Adams went so far as to say that he knew "definitely" that Baade's feelings were "strongly anti-Nazi" (which was undeniably true by then, especially when he was talking with the director), "and that was one of his reasons for coming to the United States and remaining here" (which was indeed a strange reading of his motives in 1931 and 1937). Most importantly, Adams sent Humason, a practical, down-to-earth, lodge-brother type who knew how to win friends and influence people very well indeed, together with Baade, a personable and attractive individual, to see the provost marshal in charge of preserving security in Pasadena. The colonel wilted, and by May the army command issued an exception allowing Baade to spend "regular observing periods" at Mount Wilson "as part of his professional work". He had only missed one month, April 1942, when the weather is often bad at Mount Wilson, and when M 31 and its companions, his primary targets, were in any case not well placed in the sky.

Then in the summer of 1942 Hubble departed, on leave for the duration of the war, to head the exterior ballistics work at Aberdeen Proving Ground. His astronomy professor at the University of Chicago, Forest R. Moulton, had had much the same job during the First World War. To an astronomer an artillery shell's trajectory is simply an orbit in the Earth's gravitational field, with the added complication of a resisting medium, the air — but amenable to both calculation and highly accurate measurement. Hubble himself did not know much about the scientific problem, but his great prestige, his presence, his service as an officer during the previous war, and his authoritative way of talking made him ideal for the job. His departure freed his telescope time, which Baade, the only person on the staff not immersed in war work, gladly took over. It also left Baade free to work on his own, uninhibited by a senior, ageing legend in his own time who abhorred "speculation".

Baade had decided to turn his full attention to using yellow- and red-sensitive plates, to see if he could not photograph individual bright yellow and red giants in the central region of the 'bulge' of M 31, and its companion galaxies. He had already drawn the analogy with the Sculptor and Fornax systems, in which he knew from his own careful photometric measurements, made after their joint paper, that the brightest stars had the colours of K giants. Hence he believed that he had a real chance of 'resolving' the apparent amorphous regions in M 31 into individual bright stars, seen against a background of the fainter, more numerous, still unresolved members of the system.

It required all of his skills and experience, together with the beautifully clear,
stable late summer and early autumn nights at Mount Wilson. The skies were
darkened by the wartime ‘brownout’ (a partial blackout) still in force in Los
Angeles and the San Gabriel Valley. Baade nursed the 100-inch telescope’s mir-
ror, arranging the ventilation to keep its temperature constant all night long, and
hence its form rigorously paraboloidal. He could even adjust the focus slightly
during the four-hour long exposures by eye, watching the coma pattern of the
star he was ‘guiding’ on with a high-power eyepiece rigidly attached to the
plateholder.

In the autumn of 1942 he tried very hard with yellow-sensitive plates and
almost achieved the resolution into stars he was seeking, but not quite. Just a
few more tenths of a magnitude were all he needed, and this he achieved with
red-sensitive plates in August, September and October 1943. M 32, the ‘inner’
amorphous region of M 31 itself, NGC 205, its fainter elliptical companion, and
NGC 185, one of its still fainter, more distant elliptical companions, all yielded
to this treatment. In all of them the brightest stars were yellow giants with abso-
lute magnitudes $M_p \approx -2.5$, $M_y \approx -1.2$ and colour indices $+1.3$. There were no
supergiants, no highly luminous blue or red stars in any of these galaxies, just as
in the Sculptor or Fornax systems, and evidently just as in more distant giant
elliptical galaxies, he now realized. None of them, even the nearest, showed any
individual stars, but some of them, like M 87 in the Virgo cluster of galaxies,
did contain globular clusters. Evidently these elliptical galaxies were all one
family.

In his paper on M 32, NGC 205 and M 31, he dubbed these types of stars,
which occur in great numbers in elliptical galaxies, globular clusters, and the
central regions of spiral galaxies “population II”, to contrast them with “popula-
tion I”, the “ordinary” stars (familiar in our Galaxy near the Sun), whose bright-
est stars are highly luminous O and B stars, and red supergiants. In his paper
Baade published a schematic Hertzsprung–Russell diagram, which graphically
differentiated the two populations; it was to become the keystone for future work
in galactic structure, just as Hubble’s redshift–distance plot had become the chart
for ongoing research on galaxies.

Baade recognized that population II was spread throughout our Galaxy; the
RR Lyrae variables and globular clusters he had studied for so many years were
its markers. The RR Lyrae stars were also one of the types of the “high-velocity
stars” which Oort had recognized and singled out years before. On the other
hand the O and B stars of our Galaxy were known to be the opposite extreme,
“low-velocity stars”. Thus working back and forth between Sculptor and Fornax,
M 31 and its companions, and our Galaxy, Baade had put together a new picture
which linked many facets of galactic structure and kinematics, stellar spectroscopy and motions, elliptical and spiral galaxies. Other astronomers, like
Oort and Lindblad, W. W. Morgan and P. C. Keenan, had earlier caught glimpses
of it, but Baade, with his very wide knowledge of published research, and his
own well-planned and skillfully executed observing program with the biggest
telescope in the world, had discovered it.\textsuperscript{88}

When he began this program in 1942, Baade had written to Shapley that he was working on magnitude systems in M 31 and M 33, but he did not reveal any of his ideas about the stars in elliptical galaxies. He urged Shapley to carry out several specific programs in the southern hemisphere, to measure accurately the bright ends of the luminosity functions of the two Magellanic Clouds. This was closely related to Baade's population concept, but he kept that part to himself. Shapley, on the other hand, mostly wrote about why he could not carry out such programs (most of his staff were also engaged in war work, not galaxy research, and a fast spectrograph had been lost \textit{en route} to South Africa, in a ship torpedoed by the Germans) and tried to find out what Baade was doing. Humason finally revealed the news to him in 1944, shortly before Baade published his results.\textsuperscript{89}

On the other hand, Baade's relations with the Lick astronomers, whose work he admired, was open and friendly. Early in 1942 Mayall had gone on leave to the Massachusetts Institute of Technology Radiation Laboratory to work on radar, but neither the cold Eastern climate nor his assignments there suited him very well. In the summer of 1943 he managed to transfer to the Mount Wilson Observatory war project in Pasadena, and there he, Baade and Minkowski ate lunch together on a daily basis. They always discussed astronomy, and presumably never revealed any military secrets to the enemy alien in their midst! Mayall borrowed filters from Lick for Baade, had photographs of M 31 and its companions sent to him, and saw Baade's results as he got them. Wright, now retired but continuing his research, and Joseph H. Moore, nearly as old and now the interim wartime director, learned all about Baade's resolution of the brightest population II stars in M 31 and its companions within a month of his accomplishing it. They applauded his work.\textsuperscript{90}

The few other American astronomers who still were not completely occupied in wartime laboratories learned of Baade's feat, and the two-population concept, in August 1944, when his two papers appeared in the \textit{Astrophysical Journal}. He knew that many of the readers especially "doubting Thomases, [o]ne of them ... Shapley", would be sceptical, and wanted to reproduce one of his plates as proof that could not be denied. Struve, the director of Yerkes Observatory and editor of the \textit{Journal} agreed. He had been impressed himself by seeing high-quality prints of Baade's direct photographs, and he arranged to have enough copies made at Yerkes Observatory so that one could be bound into every copy of the issue containing the two papers. Two young assistants printed all seven hundred of them (the total combined circulation of the \textit{Astrophysical Journal} and the \textit{Contributions of the Mount Wilson Observatory}) in the basement of the observatory as part of their summer jobs. Baade was very pleased with the result, and extant copies still show the resolution well today. But most astronomers, in America and abroad, only learned about this great discovery, and the new concept that came from it, after Hitler and Japan had been defeated and they came home.\textsuperscript{91}
14. Baade's Window and the Nucleus of our Galaxy

When Baade published his two papers on population II in August 1944, the Allied armies, securely lodged on the continent of Europe, had just broken out of Normandy and were sweeping through France. The Russian army groups were closing in from the east. The Germans held out and that winter achieved a brief tactical victory in the Battle of the Bulge, but as soon as the skies cleared and the American and British bombers could get back in the air, the Allies regained control and Hitler's days were numbered. He committed suicide in April 1945 and in May the Germans surrendered.

As these events were unfolding, Baade was working up his paper on two distant globular clusters in our Galaxy, clusters now recognized by him as representatives of population II. There were no new insights in the paper; he could have written it ten years earlier. But he was mulling over the implications of his discovery.92

Baade had long considered that our Galaxy was most probably an Sb, intermediate spiral galaxy like M 31. Then, like M 31, it should have a huge central bulge, composed of population II stars, strongly concentrated to its centre. RR Lyrae variables were the invariable markers of that population. Hence in our Galaxy there should be a strong concentration of RR Lyrae variables to its centre. He decided to look for it.

The best place to look was along a line that passed as close to the centre of the Galaxy as possible. From his previous surveys made in red light with the 18-inch Schmidt and the 100-inch reflector, Baade knew that the only relatively clear region was the field centred at $l = 1^\circ$, $b = -4^\circ$ (in our present galactic coordinates), which we call Baade's window. Thus in September 1945, just two weeks after the Japanese surrender on the U.S.S. Missouri, he began taking direct exposures of this far southern field with the 100-inch telescope, several on each night he had it to use. On them he found a very large number of RR Lyrae variables, 152 from his intercomparisons of the first ten plates. This corresponded to roughly 400 of these stars per square degree, more than thirty times as many as in any Milky Way field anyone previously surveyed. Obviously, there were even more RR Lyrae variables in this field which had not yet been discovered (as subsequent searches have abundantly confirmed).93

The great bulk of these RR Lyrae variables had apparent magnitudes between 16.5 and 18, the number decreasing at fainter magnitudes. Thus there was a maximum number per unit magnitude interval between $m_{pg} = 17.0$ and 17.5. Since all the RR Lyrae variables have the same absolute magnitude ($M_{pg} = 0.0$ was the value Baade used), this would give the distance modulus to the concentration of population II along this line $4^\circ$ from the galactic centre, or very close to the galactic centre. Because of interstellar extinction, which though relatively small in this field was certainly not zero, it was an upper limit to the distance to the centre. Rather than use it, Baade determined the distance modulus to the globular cluster NGC 6522 (now frequently called "Baade's cluster"), which
lies in the centre of the field. From its brightest stars its apparent distance modulus was 17.3, which indicated to Baade that it lay close to the region of greatest density of RR Lyrae variables. Stebbins and Whitford, the photoelectric experts from the University of Wisconsin, had measured the colour index of this cluster at Baade’s request. Comparing it with the ‘normal’ colour of an unreddened globular cluster, he derived the “color excess” due to interstellar extinction, and from it the amount of this extinction. In this way he derived the distance modulus to the galactic centre \( m - M = 14.7 \), corresponding to a distance of 8.7 kpc. He was to improve the accuracy of this determination several times in the next decade, but the final value remained much the same. We today can see all kinds of improvements to make, but the actual numerical result has changed remarkably little in the intervening half-century. Apparently Baade was not only intelligent, dedicated, hard-working, and skilful, but lucky as well.\(^{94}\)

Baade presented this paper orally in 1946 at the first meeting of the Astronomical Society of the Pacific since 1941. He had joined the ASP in 1934, three years after becoming a Mount Wilson staff member. Travel restrictions, particularly severe along the west coast, had prevented the Society from holding even a single meeting while America was at war. The 1946 meeting was held on the campus of the University of Nevada in Reno in June, and as he gave his paper and participated in the scientific discussions in the physics building, lounged in the spacious dormitory alongside an artificial lake in this pleasant, shaded oasis in the desert, or ate his hearty meals at the cafeteria, Baade must have thanked his lucky stars that he had left Germany in 1931, and decided not to go back in 1937. He had been in correspondence with fellow German astronomers since the summer of 1945. He knew they not only were not observing with a 100-inch telescope while waiting for a 200-inch to be completed as he was, but that they were spending almost all their time trying to find food and shelter for themselves and their families, while they wondered whether or when they would lose their jobs because they had been Nazi Party members.\(^{95}\)

15. After the War

In 1946 and 1947 Baade was waiting impatiently for the 200-inch telescope to be finished, but he never stopped working with the 100-inch. Ira S. Bowen, the Caltech laboratory spectroscopist who had solved the riddle of the nebular emission lines in the 1930s, proving they arose in forbidden transitions of ions of O, N, Ne, and S, became the new director, replacing Adams who retired on 31 December 1945. Bowen supported Baade and his research very strongly, appointing him to the new program committee he set up to plan the transition to doing research with both telescopes, and he saw to it that the discoverer of population II was assigned plenty of observing time with the 100-inch in the interim.\(^{96}\)
Baade now realized that diffuse emission nebulae, or H II regions as they were later called, must be considered population I objects. He began a systematic program of photographing the entire body of M 31 in red light, which he knew would best show the emission nebulae in it, just as in our Galaxy. He used the narrow band around Hα and [NII] λλ 6548, 6583, and for comparison, a band in the near infrared, defined by 103-U plates with a deep red filter which suppressed these same lines, and thus recorded only the continuum light from the stars. He soon saw that there were many emission nebulae in M 31, many more than Hubble had seen in his earlier studies. He had used unfiltered blue light, recording chiefly Hα, [O III] λλ 4959, 5007, and [O II] λ 3727, all but the last considerably weaker than the lines in the red, and all, especially λ 3727, subject to much stronger interstellar extinction. Baade got Humason to take spectra of some of the doubtful nebulae, but as he was devoting most of his time to working on redshifts of galaxies for Hubble, progress was slow. So Baade sent the charts of his nebulae in M 31 to Mayall at Lick, who was happy to obtain their spectra with his very fast spectrograph on the Crossley reflector. From his own direct photographs and Mayall’s spectra, Baade concluded that emission nebulae, extinction by dust, and high-luminosity O and B stars were always associated. They were ‘pure’ population I objects or features. And the spiral arms were features seen in these emission nebulae, not concentrations of stars as all previous observers had thought and as all theorists had tried to explain. The spiral arms almost disappeared in photographs taken in the deep-red continuum, Baade discovered in 1947. As he put it in a letter to Mayall, “the pop[ulation] II is the backbone of the whole [galaxy] and the spiral pattern is a flashy but rather inconsequential adornment”. It was another outstanding discovery.

Only a minority of American astronomers had read Baade’s first two papers on stellar populations when he published them. Most of them were out of touch with astronomy, at war-time weapons development projects or teaching mathematics, physics, and navigation to student soldiers, sailors and cadets. After the war ended, as the astronomers came home to their observatories and colleges, they caught up on their own specialties, but Baade’s papers with titles about resolving galaxies were out of the mainstream. Most astronomers then did research on stars. Baade’s work first received wide attention at the American Astronomical Society meeting held on the campus of Ohio State University in Columbus, at the end of 1947. The organizers scheduled a symposium on “the relations between spectral characteristics and motion of stars”, led off with a 45-minute invited lecture by Baade on a “survey of the problem of the two stellar populations”.

He was very well prepared for this, his first major invited talk in English. In the two years 1946 and 1947 he had given four reports on his work on the two populations and the comparison of the structure of our Galaxy with that of M 31 in the Journal Club, the informal research discussion group made up of all the
astronomers on the Mount Wilson staff together with visiting scientists and Caltech physicists interested in astronomical research. His notes for these talks show the growth of his understanding of the various manifestations of the two populations, applied to M 31 and our Galaxy. All four talks were clearly detailed, high-level scientific presentations. Whether or not the other Pasadena astronomers, all of them quite specialized in their own different fields, understood them, these talks provided an impetus for Baade to work out his ideas in detail.\textsuperscript{98}

Some 150 astronomers were present at the meeting in Ohio at the end of 1947. Baade was among friends; on the first evening of the meeting Adams presented the second Henry Norris Russell lecture on his very high-dispersion spectroscopic work on interstellar absorption lines, revealing clearly the cloud structure of the interstellar medium. (Russell himself had given the first lecture the previous year.) The next day all the astronomers went by bus to Perkins Observatory, some twenty miles away near Delaware, for the symposium. Baade’s lecture, as he wrote it out beforehand in English on fifteen large sheets, was excellent. He presented all the ideas of the two stellar populations in clear, logical form, going back to his early work on RR Lyrae variables in the field, Oort’s analysis of the high-velocity stars, and his and Hubble’s study of Sculptor and Fornax. Baade continued up to his discovery of population II in M 31 and its companions, and his observations of the concentration of RR Lyrae variables near the centre of our Galaxy. He did not include (at least in his extant notes) his latest results on the spiral arms as features in the interstellar gas.\textsuperscript{99}

After Baade’s lecture, six shorter invited papers were presented, including one by Minkowski, who spoke on planetary nebulae, and concluded from their radial velocities and their space distribution that they are population II objects. Cecilia Payne-Gaposchkin spoke on variable stars, and introduced the idea of treating population as a continuous variable, with RR Lyrae variables being extreme population II, Cepheid variables extreme population I, and various types of long-period variables between them. That evening they all went back to Columbus for the Society dinner, held at the Ohio State University faculty club, with many speeches, presided over by Stebbins, a noted raconteur. The next morning, 31 December, was devoted to more papers, and then the meeting broke up. Baade, Minkowski and Adams started back to California by train on New Year’s Eve.\textsuperscript{100}

Baade’s lecture had been a tremendous success. He inspired many who were there to apply their own particular research skills to the two-population concept. All those who were present recognized that it was an important one. They all remembered Baade and his expressive way of speaking. He had described the two populations and their defining characteristics well. But he had not explained the physical reason for the difference between the two populations, because he did not understand it himself. As we shall see in the final instalment of this paper, several theoretical astrophysicists had already recognized that it was stellar
evolution; population I was composed of young, recently formed stars, and population II of old, evolved ones. One theorist had even recognized it years before Baade discovered it. But the great observer who always sought physical understanding had rejected this explanation as late as June 1947. He was still puzzled at the time of his Columbus lecture. But soon afterward he became convinced of his error, and in the last twelve years of his life he was a leader in unlocking the secrets of stellar evolution. He inspired a whole generation of astronomers and astrophysicists to work along many different lines on these problems, by his frequent invited lectures, delivered at meetings, symposia, summer schools and courses.101

16. Acknowledgements

I am most grateful to the many archivists and individuals who helped me obtain research material for this book. All those named in the acknowledgements of the first paper in this series I thank most sincerely once again, especially Owen Gingerich, whose help at Harvard and with his own collection was invaluable. I wish particularly to acknowledge the great help of Karl-Jochen Schramm, who allowed me to use his fascinating book-length manuscript, Die Sterne über Hamburg: Die Geschichte der Astronomie in Hamburg as general background material, and provided me with copies of all the correspondence with Walter Baade that was available at the Hamburg Observatory. Theodor Schmidt-Kaler was also most helpful in sending me useful information on Baade’s family. I am also very grateful to Spencer Weart (American Institute of Physics) and Bernard Schermetzler (University of Wisconsin Archives) for their help. Finally, although most of the translations from German are my own, I am deeply grateful to Peter Bodenheimer, Andreas Burkert, Alfred Gautschy, Wolfgang Hillebrandt, Willi Kley, and Harold Yorke for their assistance with some of the (to me) more difficult passages.

REFERENCES

ABBREVIATIONS USED

(a) Archival Sources
AIP American Institute of Physics Microfilm Sources for the History of Modern Astrophysics
Correspondence — Jan Hendrik Oort
CIW Carnegie Institution of Washington, Washington D. C.
Institution Files
HCO Harvard College Observatory Records, Harvard University Archives,
Pusey Library, Cambridge, Massachusetts
Harlow Shapley Director’s Papers
HHL Mount Wilson Observatory Collection, Henry E. Huntington Library,
San Marino, California
Walter S. Adams Papers
Walter Baade Papers
Ira S. Bowen Papers
Frederick H. Seares Papers
HO Hamburg Observatory, Hamburg, Germany
RAC Rockefeller Archive Center, North Tarrytown, New York
            International Education Board Records
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
UWA University of Wisconsin Archives, Madison
            Department of Astronomy Papers
YOA Yerkes Observatory Archives, Williams Bay, Wisconsin
            Directors' Papers

(b) Individuals
WSA Walter S. Adams
RGA Robert G. Aitken
WB Walter Baade
ISB Ira S. Bowen
VB Vannevar Bush
CPG Cecilia Payne-Gaposchkin
WMG Walter M. Gilbert
EH Edwin Hubble
MLH Milton L. Humason
AHJ Alfred H. Joy
GPK Gerard P. Kuiper
MM Max Mason
JCM John C. Merriam
NUM Nicholas U. Mayall
RM Rudolph Minkowski
JHM Joseph H. Moore
ERM Edward R. Murrow
JHO Jan H. Oort
DEO Donald E. Osterbrock
RS Richard Schorr
FHS Frederick H. Seares
HS Harlow Shapley
JS Joel Stebbins
OS Otto Struve
WHW William H. Wright
FZ Fritz Zwicky

(c) Publications
AJ Astronomical Journal
BMNAS Biographical Memoirs of the National Academy of Sciences
CIW Carnegie Institution of Washington Year Book
JHA Journal for the History of Astronomy
PA Popular Astronomy
PAAS Publications of the American Astronomical Society
PASP Publications of the Astronomical Society of the Pacific
PNAS Proceedings of the National Academy of Sciences

3. WB to [HS], 15 Mar. 1937, HCO.
4. Some of Baade’s unexculpated statements about Hubble’s hopeful treatment of magnitude scales are preserved in WB, Evolution of stars and galaxies (Ann Arbor, 1980), a facsimile reprint of the mimeographed notes from a graduate course he gave at Harvard in 1958, prepared at the time by R. B. Rodman from a tape recording.
5. Baade’s progress is briefly described in successive annual reports, similar to the one for 1931–32 cited in ref. 2, WSA, “Annual report of the director”, CIW, xxxii (1933), 127–65; xxxiii (1934), 125–57; xxxiv (1935), 157–90; xxxv (1936), 157–94. These annual reports, drawn up by Adams on the basis of draft paragraphs and sections prepared by individual staff members like Baade, are full of information and are used throughout this paper without being referenced each time.
7. HS to WB, 3 Mar. 1937, HCO.
8. JS to WB, 23 Nov. 1935, 19 Nov. 1942, UWA.
9. WB to [NUJM], 19 Nov. 1945, SLO; WB to [IS]B, 12 Sep. 1946, HHL.
11. WB to [NUJM], 10 May 1936, SLO.
15. WB to [RS], 21 Oct. 1935; [RS] to WB, 18 Nov. 1935; HO.
17. See ref. 1.
24. R. Müller, Fritz Zwicky: Leben und Werk des grossen Schweizer Astrophysikers, Raketenforschers und Morphologen (Glarus, 1986). This is by far the best source for an outline of the subject’s life and career, but any statement in it based on retrospective writings by Zwicky himself must be taken with a large grain of salt, if it is not corroborated by contemporary written evidence. The author reveals this indirectly many times in the book, not in the text itself but in the references in the appendix. Several more discrepancies have turned up, in the research for the present paper, between Zwicky’s later statements and written letters from the 1930s in archives cited.
25. WB to [RS], 7 June 1935, [RS] to [WB], 19 Aug. 1935, HO.
27. FHS to FZ, 31 Aug. 1937, HHL.
28. HS to [WB], 19 Aug., 4 Sep. 1936, HCO; FZ to CPG, 8 Aug. 1936, Müller, op. cit. (ref. 24), 153. I have not had access to this letter, which Shapley and Müller describe; Müller quotes directly the one sentence from it which I have quoted in the text, translated back into the English in which Zwicky presumably wrote it.
33. WB to [NUJ], 15 May, 30 June 1936, 13 Jan. 13 Mar. 1937, SLO, are just a few of the early examples of such letters.
34. WB to [NUJ], 16 Jan. 1937, SLO.
37. DEO, "Rudolph Leo Minkowski", *BMNAS*, liv (1983), 271–98; Th. Schmidt-Kaler, "Rudolph Minkowski", *Sterne und Weltraum*, xxxiv (1995), 436–40. Minkowski’s first name was Rudolf, in the German spelling, until 1938 when he Americanized it to Rudolph, but I have used the later spelling throughout.
38. WB to [WS]A, 15 June 1933, HHL.
39. W. E. Tisdale memorandum, 24 July 1933, WSA to MM, 31 July 1933, RAC.
41. ERM to WSA, 19 Oct. 1934, WSA to W. Weaver, 26 Oct. 1934, Weaver to WSA, 1 Nov., 16 Nov. 1934, RM to WSA, 25 Feb. 1935, HHL.
42. ERM to WSA, 17 May 1934, FHS to JCM, 27 May, 3 June (telegram and letter) 1935, JCM to FHS, 29 May, 31 May (both telegrams), 31 May (letter) 1935, R. F. Hanson to JCM, 24 June 1935, JCM to Hanson, 13 July 1935, WSA to JCM, 9 Aug. 1935, HHL.
45. WSA to Director, Emergency Committee, 25 Mar. 1936, J. Whyte to WSA, 10 Apr., 14 May 1936, WSA to Whyte, 30 Apr. 1936, WSA to JCM, 18 May 1936, HHL.
46. WSA to JCM, 14 Jan., 3 May 1937, JCM to WSA, 8 Mar 1937, HHL.
47. WSA to JCM, 11 Apr., 25 Apr., 17 May 1938, JCM to WSA, 14 Apr., 5 May 1938, HHL.
50. WMG to WSA, 19 Dec., 26 Dec. 1942, 5 June 1942[2], WSA to WMG, 22 Dec. 1941, HHL.
51. WB to [RS], 22 Oct. 1934, 1 Apr. 1935, HO.
52. RS to [WB], 13 May, 14 May 1935, WB to [RS], 7 June 1935 (second letter of this date), HO.
53. WB to [RS], 21 Oct. 1935, 21 July 1936, HO.
55. RS to [WB] 14 June, 30 Sep., 15 Nov. 1936, WB to [RS], 7 Dec. 1936, HO.
56. WSA to JCM, 29 Sep., 7 Oct. 1936, JCM to WSA, 1 Oct. 1936, HO.
57. WB to [RS], 7 Oct. 1936, HO.
59. WSA to MM, 6 July 1937 (telegram), 10 July 1937, MM to WSA, 7 July (telegram) 1937, WSA to JCM, 7 July, 19 July, 28 July, 16 Aug. 1937, JCM to WSA, 8 July, 9 July, 21 July, 1 Aug. 1937, G. L. Streeter to WSA, 20 Aug. 1937, HHL.
61. WSA to RS, 19 July 1937, HHL.
62. WB to Councilor Niemann, 29 July 1937, WB to [RS], 4 Aug. 1937, 13 Mar. 1938, WB and Hanni Baade to [RS], 10 Aug. 1937, HO; GPK to “Dear Colleague”, 14 June 1946, YOA. Kuiper was a member of the ALSOS mission, which followed close behind the Allied armies, debriefing and interrogating many Dutch and German astronomers and physicists during and immediately after the war. This letter summarizes his observations that there were no anti-Nazi astronomers left, except K.-O. Kiepenheuer.
64. WB, “Stellar photography in the red region of the spectrum”, *PAA*, ix (1939), 31–32.
67. HS to WB, 2 Nov. 1938, HCO.
68. WB to [O]S, 28 Nov. 1938, YOA; WB to RS, 8 May 1939, HO; D. S. Evans and J. D. Mulholland, *Big and bright: A history of McDonald Observatory* (Austin 1986).
69. WB, “Paper at dedication of the McDonald Observatory”, [hand written text (in parts) and notes (in other parts)], HHL.
70. WB to [NUM], 20 May 1939, SLO; JHO to AHJ, 23 Dec. 1939, HHL; WB to [JHO], 17 Oct. 1940, 6 Oct. 1941, [JHO] to WB, 23 June, 22 Nov. 1941, AIP.
71. WB to RS, 13 July 1939, HO; WB to E. P. Varela, 5 June 1939, CIW; WB to [HJS], 1 Sep. 1939, HCO; “Programme du Colloque de la Foundation Singer-Polignac, Paris, Juillet 1939”, HHL.
77. JHO to WHW, 13 May, 20 June, 24 July 1939, WHW to JHO, 22 June, 1 July, 26 Aug. 1939, WB to [NUM], 19 Oct. 1940, 27 Feb. 1941, SLO.
80. WB to [NUM], 13 Nov., 27 Nov., 6 Dec. 1941, 14 Jan., 29 Jan. 1942, SLO.
82. EH, “Problems of nebular research”, *Scientific monthly*, liii (1940), 391–408; EH, “Zwicky’s systems in Sextans and in Leo”, *Scientific monthly*, liii (1941), 486; FZ, *Catalogue of selected compact galaxies and of post-eruptive galaxies* (Guemeligen, 1976). The phrases quoted in the text are sample "descriptions" from this last book. See also ref. 24, which however downplays Zwicky’s
hostilities and outbursts.


84. WB to [H]S, 2 Feb. 1939, HCO.

85. WB and EH, “Two new stellar systems in Sculptor and Fornax”, PASP, li (1939), 40–44. EH to V. M. Slipher, 11 June 1941, HHL states very clearly Hubble’s position on observers producing data and “theoretical men” speculating.

86. E. Schoenberg to WB, 13 July 1939, K. Wurm to WB, 18 July 1939, HHL.

87. WSA to VB, 6 Apr. 1942, VB to WSA, 8 Apr. 1942, WSA “To Whom It May Concern”, 8 Apr. 1942, FHS to ditto, 8 Apr. 1942, WSA to Selective Service Draft Board, Altadena, 9 Apr. 1942, WSA to Col. Severin, 30 Apr., 14 May 1942, 100-inch Telescope Logbooks 1 and 2, HHL; GPK to OS, 17 May 1942, YOA.


91. WB to [O]S, 26 June, 8 July, 31 Aug. 1944, OS to WB, 1 July, 12 July, 6 Sep. 1944, YOA.


93. WB to [W.] Dieckvoss, 26 Sep. 1945, HHL.


96. ISB to WB, 4 Jan. 1946, HHL.


98. WB, “Population II; 1947 Group meeting (January); Journal Club; Structure of our Galaxy (comparison with M 31)”, [Notes for four different talks, one dated “Journal Club 1947 April 30”, another clearly from 1946 from an internal reference, fourth probably also for 1947]. HHL.

99. WB, [Survey of the problem of the two stellar populations], [1947], 15 hand-written pages of text of his invited talk.


101. WB to F. L. Whipple, 2 July 1947, HHL.