

THE SUMMER OF 1953: A WATERSHED FOR ASTROPHYSICS

In 1953, the Michigan Symposium on Astrophysics proved instrumental in shaping our understanding of stellar evolution and in shaping the future careers of many of the participants. One participant gathers his colleagues' reminiscences.

Owen Gingerich

Beginning in 1927 the University of Michigan's summer school in physics became famous as *the* international forum for learning about the latest advances in modern physics. Within a decade of its inception perhaps half of the most renowned European physicists had turned up for its sessions—scientists like Fermi, Dirac, Pauli, Sommerfeld, Goudsmit, Uhlenbeck, Ehrenfest and the astrophysicists Robert Atkinson and E. A. Milne.

Why Michigan and why 1927? European celebrities had long been accustomed to visiting East Coast institutions, but by the late 1920s train and automotive transportation made a visit to the Midwest eminently feasible. A summer school was an efficient route for Americans to assimilate the new physics being developed in Europe at the time.

Between 1935 and 1942 the director of Harvard Observatory, Harlow Shapley (the best-known astronomer of the day), taking a cue from Michigan, organized a series of astronomical summer schools at Harvard. (See David DeVorkin's article in *PHYSICS TODAY*, July 1984, page 48.) These schools brought to Cambridge such well-known astronomers as Otto Struve, Antonie Pannekoek, Knut Lundmark, Henry Norris Russell, Meghnad Saha, Jan Oort, Martin Schwarzschild, S. Chandrasekhar and H. P. Robertson.

When the Harvard summer schools did not resume after World War II, one of the participants, Leo Goldberg, who had meanwhile become director of the University of Michigan Observatory, decided to institute a summer symposium in astrophysics in Ann Arbor in 1953. In particular, he was determined to bring Walter Baade from Mount Wilson and Palomar Observatories as the central

attraction for his conclave. Baade had been recruited to Mount Wilson from Hamburg in 1931. During World War II he had been classified as an enemy alien, unsuitable for war work. Consequently he was able to spend long hours with the Mount Wilson telescopes, taking advantage of the unusually dark skies produced by the wartime brownout of the Los Angeles basin.

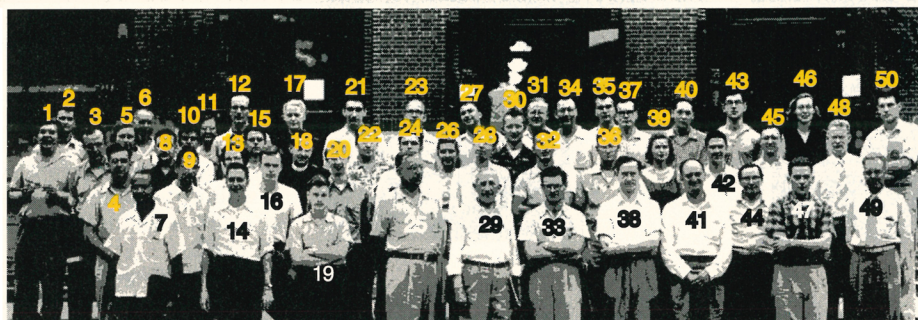
Baade knew that the spiral arms of the Andromeda galaxy M31 tended to be blue, and had no difficulty photographing individual blue stars. However, his persistent attempts to photograph individual stars in the redder galactic nucleus failed. It then dawned on him that two different populations were involved and that the brightest stars of the red population were considerably less luminous than the blue stars of the spiral arms. (See the box on page 39.) The fainter red population of the Andromeda galaxy's nucleus, which was similar to the content of globular clusters, he designated as population II, whereas the brighter, bluer composition of the spiral arms he called population I. Associated with the two populations were two types of Cepheid (see page 19) variable stars that had previously been grouped together. Because the distance scale for galaxies depended critically on the Cepheid variables, Baade's finding called all accepted galaxy distances into question. Armed with his understanding of the two populations, within a few years Baade was able to exploit the newly available 200-inch Palomar reflector to demonstrate that the Andromeda galaxy was at least twice as distant as previously believed, and at the International Astronomical Union meeting in Rome in 1952 his dramatic announcement effectively doubled the size of the known universe.

Goldberg had met Baade during a West Coast trip in the spring of 1940, and the two men had apparently hit it off very well, for Baade wrote to Shapley on 29 May 1940 that, "We enjoyed thoroughly having Goldberg with us for some weeks recently. He is a very nice boy and I had plenty of fun discussing things with him and speculating and fighting wildly."¹

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Participants in the 1953 Michigan summer school on astrophysics included some of the most influential researchers from two generations of astrophysicists. **Figure 1**



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|----------------------|----------------------|-----------------------|---------------------|
| 1. Geoffrey Burbidge | 14. Stan Wyatt | 27. | 40. Lowell Doherty |
| 2. Nelson Limber | 15. Larry Helfer | 28. Paul Mutschlecner | 41. Leo Goldberg |
| 3. Allan Sandage | 16. Gene Parker | 29. Walter Baade | 42. Al Boggess |
| 4. J. B. Oke | 17. Irene Osterbrock | 30. Bob Brownlee | 43. Bernard Pagel |
| 5. Margaret Burbidge | 18. | 31. Lawrence Aller | 44. Dean McLaughlin |
| 6. Freeman Miller | 19. Owen Gingerich | 32. | 45. Ken Yoss |
| 7. John Kraus | 20. Art Cox | 33. Ed Salpeter | 46. Joyce Newkirk |
| 8. Tom Matthews | 21. Marshall Wrubel | 34. | 47. Ed Spiegel |
| 9. | 22. Bob Rubin | 35. John Waddel | 48. Geoffrey Keller |
| 10. Eugenio Mendoza | 23. Frank Edmonds | 36. John Cox | 49. |
| 11. Edith Mueller | 24. Karl Henize | 37. Jum Milligan | 50. Ed Dennison |
| 12. Don Osterbrock | 25. George Gamow | 38. G. K. Batchelor | |
| 13. Nancy Roman | 26. Vera Rubin | 39. Nancy Boggess | |

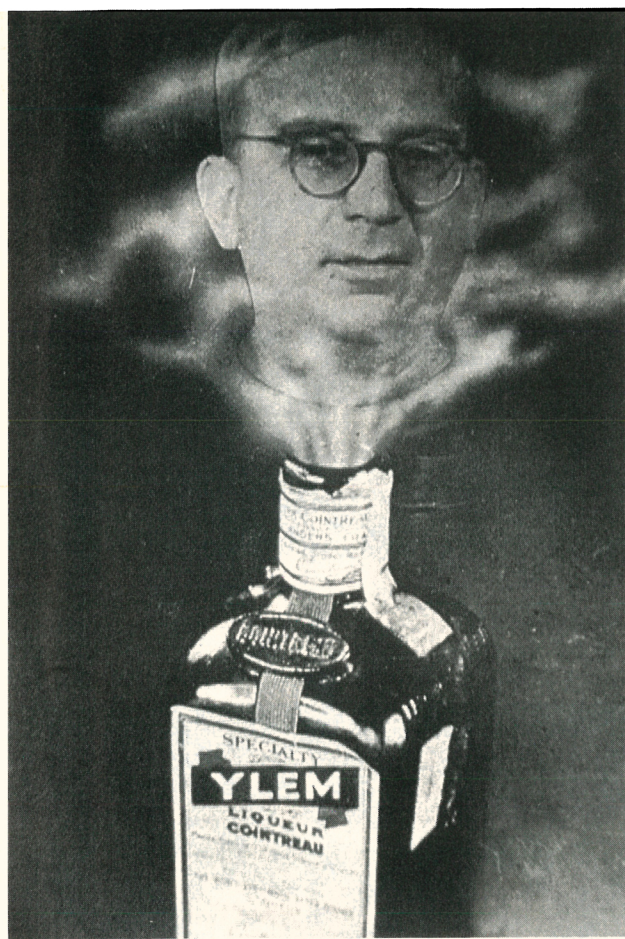
In later years Goldberg recalled: "I was a great admirer of Walter Baade's, going back to the 1940 visit when I used to sit up on the Newtonian platform [at Mount Wilson] with him and just talk and listen to him. I remember going back to Cambridge and writing a note to Shapley, 'How come we don't have Baade?' at the [Harvard] summer sessions. I guess I realized that there was trouble between Shapley and Mount Wilson, but Shapley had actually gotten Baade over to this country to begin with, and Baade didn't have any animosity toward Harvard or Shapley. I got a kind of noncommittal reply from Shapley saying, yes, it would be great, he would argue you under the table, and so on and so forth—but nothing ever came of it.

"So I always had in mind, when I got to Michigan, that it would be nice to get Baade. I used to go out to Pasadena almost every winter, and I'd end up usually

having dinner in Baade's house at least once. So I said to Baade, 'How about coming to Michigan? Let's organize a summer session.'

"That was just after he had announced the work on the populations, just after he had doubled the distance scale. So it was very timely, and in consultation with him, we drew up this cast of characters that we had: George Batchelor, Ed Salpeter, George Gamow and so forth—and I got the NSF involved, so that we could fund at least one graduate student from each of the leading graduate schools in astronomy."²

Batchelor, an expert on cosmic turbulence, came from Cambridge University in England. Salpeter, a nuclear physicist who was just beginning to explore nuclear processes in astrophysics, had just been appointed associate professor at Cornell. Gamow, the most senior and most famous of the three, had for many years been fascinated



George Gamow in his element. Gamow's lectures often included humorous slides that dealt with profound scientific questions. This slide, presented during a lecture on nucleosynthesis, depicts Gamow materializing out of a bottle of "Ylem"—the primordial substance from which the elements supposedly formed. **Figure 2**

by the evolution of stars, hoping that they would reveal something about the origin of the elements; since the mid-1930s he had been teaching at George Washington University in Washington, DC.

Summertime astrophysics at Ann Arbor

It is fascinating that Goldberg received National Science Foundation support for his summer school. Today this would be expected, but then the fledgling NSF was only in its second year and not very well funded. The \$5500 NSF grant enabled Goldberg to bring in a series of distinguished lecturers and to provide fellowships for 14 graduate students or young postdocs from across the United States. (See figure 1.) At the end of June the young fellows turned up in Ann Arbor from across the country, and most of them, together with Baade, took over one of the fraternity houses for living quarters.

Not everyone who turned up for the summer school was in fact funded by the symposium. Vera Rubin, today one of the leading experts on the evidence for dark matter in galaxies, recalled in a letter to me: "I was one of the unsuccessful applicants (I was a PhD student of Gamow's. . .), but I went for two weeks anyway. Women could not live in the house where the students were housed, so Bob [Rubin's husband] and I rented a room. . . . [One of the local organizers], who became a good friend later, came up to me at the first break of my first day and said I couldn't drink the lemonade because I hadn't paid!

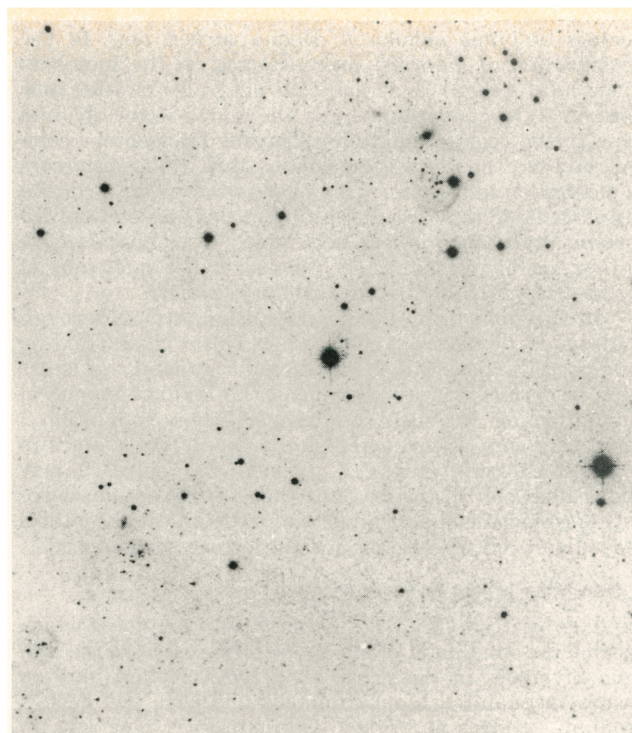
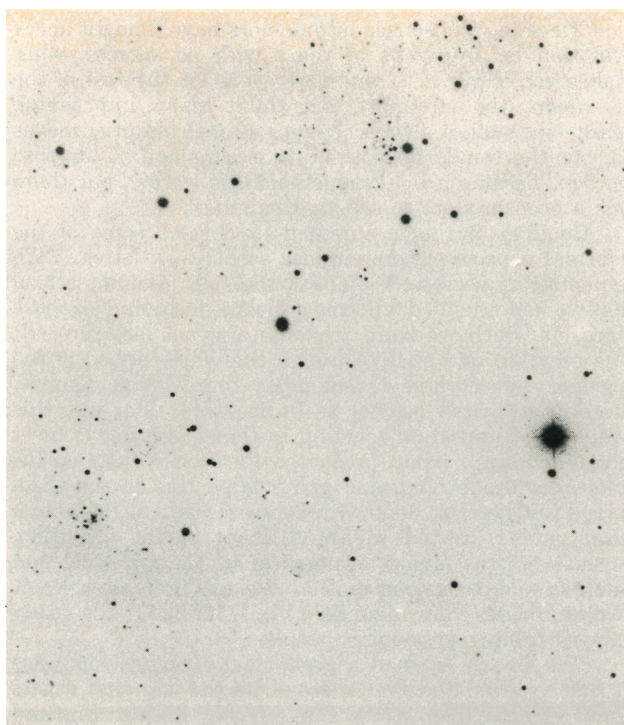
"I remember several *long*—about five hours—conversations with Baade and Gamow about H-R diagrams and

ages (see the box on page 39), during which Gamow finished $\frac{1}{2}$ bottle of something. Gamow embarrassed me by his behavior—sleeping during lectures, asking 'stupid' questions (fundamental, I would now call them), but he understood the astronomy before anyone else."

George Gamow was perhaps the most colorful character who took part in the symposium—even more legendary than Baade himself. Astronomers of the 1950s generation grew up on his *One Two Three Infinity*, which was available as a 25¢ paperback in all the drugstores. In 1953 the steady-state universe was still a live cosmological option, but obviously incompatible with Gamow's view of the explosive origin of the universe. Gamow brought to Ann Arbor a slide of the dictionary definition of the obsolete word "ylem," which he had adopted for the primordial matter of the Big Bang. Gamow modified the dictionary entry to include a sly gesture toward Fred Hoyle, one of the leading proponents of the steady-state theory and the man who had coined the name Big Bang, intending it as a pejorative expression. Gamow also had a whimsical slide of himself materializing out of a cloud of smoke issuing forth from a bottle labeled "Ylem." (See figure 2.)

Gamow spoke about the age of the universe, the basic principles of general relativity, the expansion of the universe and the formation of galaxies. In his final two lectures he turned to the formation of the elements, mentioning the conditions at the initial stages of the universe but (at least as far as the notes show) avoiding Hoyle's term "Big Bang." Gamow had suggested to his thesis student Ralph A. Alpher that the elements had been synthesized in a nonequilibrium dynamical fashion during these early stages of the expansion. Alpher, together with his neighbor at the Johns Hopkins Applied Physics Laboratory, Robert Herman, showed in 1951 that with a straightforward neutron-capture sequence and smoothed neutron-capture cross sections, a pretty good fit to the observed cosmic abundance of the elements could be achieved. (See Alpher and Herman's article in *PHYSICS TODAY*, August 1988, page 24.)

Gamow's picture of the primordial nucleosynthesis of elements was threatened by the lack of stable nuclear masses 5 and 8, which made producing heavier elements a problem. This detail was simply smoothed over in initial theories of primordial nucleosynthesis. There seemed to be little way out of this difficulty, which Gamow humorously depicted in yet another of his cartoon slides, showing Eugene Wigner with his proper Continental manners saying "Please" as he merrily jumped over the mass-5 gap. As part of Salpeter's comprehensive lectures on stellar energy processes both on and off the main sequence he showed how the triple-alpha process circumvented the gaps at masses 5 and 8 to produce carbon under the equilibrium conditions of stellar interiors (as opposed to the nonequilibrium situation in the opening minutes of the Big Bang).



Far arm of the Andromeda galaxy, as imaged by Walter Baade at the 200-inch Palomar reflector telescope, is photographed here in blue (left) and red (right) light. The blue plate highlights the much hotter, more brilliant population I stars, while the red plate shows the dimmer population II stars, as well as HII regions illuminated by bright blue stars. The Michigan summer school occurred shortly after Baade had done his initial work on the two populations. **Figure 3**

In a conversation with me, Salpeter reflected on the 1953 Michigan symposium: "No other meeting I have ever gone to has had more of a shaping influence on my academic career. I was just at a formative point, and I was sensitized by Baade and Gamow in particular. I had worked on nuclear reactions, but stellar statistics and evolution were brand new to me and set me on a really new direction. In fact, the symposium had more influence on me than all the other meetings I ever attended put together."

Baade as a first-magnitude star

Allan Sandage, a participant, remembers the Michigan summer school for two reasons. First, many of the young students attending the symposium went on to very influential careers in astronomy, and second, the course turned out to be very seminal because of the state of astronomy itself. As he recently wrote to me, "it was organized in the earliest of the years of that never-to-be-repeated decade in the history of ideas during which the development of modern stellar evolution was occurring. From our present position of perfect hindsight, 40 years after that magic summer, it is not credible to those who did not apprentice in astronomy during that period that even the most basic understanding of the evolutionary significance of the H-R diagram was not appreciated, even in the previous decade. It was the opening up of the understanding that as a star ages it moves *off* the main sequence—rather than up it, as in Gamow's scheme of the 1930s, or down it as in Russell's scheme of the 1920s—that caused so many facts about stellar astronomy to fall into place in the first years of the 1950s. . . .

"The development occurred as a natural continuation of Baade's population concept of 1944, leading as it did to

the just discovered main sequence of globular clusters in one of the first completed programs using the new 200-inch Palomar telescope. [That program was the thesis project that Sandage had just completed under Baade's supervision.] . . . Baade had done all his work from Mount Wilson on the early population concept, and had come to the summer school to discuss the continuation of this work that had been accomplished in the first 4 years with the Palomar 200-inch telescope."

The star of the meeting was, of course, Baade, a stinting publisher but extremely generous with his knowledge in informal settings such as this. He came armed with Palomar plates and scores of glossy prints from Mount Wilson, which he used not only in his 11 formal lectures but also in the ongoing sessions back at the local fraternity house. Central among his photographs was a portfolio of all the members of the so-called local group of galaxies, including previously unexhibited details of the Andromeda galaxy in both red and blue light. (See figure 3.) Thomas Matthews and I, the NSF fellows from Harvard, spent hours in the darkroom making copies of Baade's photographs. We made smaller sets for many of the participants. Meanwhile, we all took turns writing up lecture notes, which were mimeographed for those in attendance.

Baade gave first a historical lecture, praising Shapley for introducing in 1918 a picture of the Milky Way far larger than astronomers had previously envisioned. But he criticized Shapley for failing to take the next step to the external galaxies—a leap taken a few years later by Edwin Hubble. (Shapley, who had done his work on the scale of the Milky Way while at Mount Wilson, always defended himself by saying that the staff there had been kept under tight rein, and that his assignment had been

clusters and not nebulae.) Baade turned next to the classification of galaxies, concentrating on the members of the local group. That topic led naturally into his new work on stellar populations. By the fourth lecture he had come to the color-magnitude diagrams for globular clusters, and next he discussed variable stars in those clusters as population indicators. This paved the way for a careful assessment of the two different populations of Cepheid variable stars, then a very new topic. After an extensive analysis of population II, he turned to the questions of spiral arms, HII regions and stellar evolution.

In the final lecture at Michigan he gave a thorough analysis of the different regions of a spiral galaxy and he developed the powerful observational argument that stars must be formed continually. Thus, he laid the empirical foundation for his idea that the brightest population I stars were very young, whereas the population II stars in globular clusters, elliptical galaxies and spiral galaxy nuclei had long histories. But these presentations were only one element in a meeting where the informal contacts proved as profitable as the formal lecture schedule.

Astrophysicists in evolution

Sandage writes: "My first memory is of the dorm which became the focal point of our interactions. Across the hall from my room in the dorm lived Larry Helfer, and we became acquainted almost immediately. He had been a graduate student at Yerkes [Observatory]. One day, in the first week before the real work began, Larry said to come with him because he wanted to introduce me to a couple of friends of his. He said, 'You seem to like a good argument and to get sparks off people, so I want to watch how you and my two friends might interact.' So on the first Sunday afternoon, tea time, he took me around to an apartment complex, knocked on the door, [and we] entered, to find a legendary couple (even then) working among piles of manuscripts and calculations. It was the apartment of the Burbidges, who had come down from Yerkes."

Geoffrey and Margaret Burbidge were a British husband-and-wife team of astronomers who had recently come to America. In the ensuing years Geoff as a theorist and Margaret as an observer would play key roles in better defining the concept of stellar populations; and both would become sometime observatory directors and astronomy professors at the University of California in San Diego.

As Sandage remembers, even then Geoff Burbidge was a man of strong opinions: "The memory that will stay with me to the end is of the next several hours. Geoff, never one to dally to the point, began in his best voice of intimidation, 'And so, young man [we are the same age to within less than a year], what is it that you do?'"

"I had just finished my thesis with Baade at Mount Wilson on the H-R diagram of globular clusters and a postdoctoral stay with Martin Schwarzschild at Princeton on the theoretical interpretation of the data. About a year earlier [Halton] Arp, [William] Baum, and I had succeeded in finding the main sequence of M92, and my thesis was the same thing in M3. Schwarzschild had just succeeded (in a series of papers beginning in 1952) in gaining theoretical access for the first time to the giant region of the H-R diagram. It was a heady time, because all of the connections between the various branches of stellar astronomy were laid out in a scene before our eyes, simply waiting to be embraced and understood on a deep level by these ideas of evolution. The way seemed so clear in that summer of 1953, and I said to Geoff, 'Well, sir, I'm working in stellar evolution. We have a way to age date the stars in given clusters, and . . .'"

"He said, 'Nonsense, astronomers have thought about evolution for hundreds of years with no success, what makes you think . . .' and so it went for the rest of the afternoon, the summer, and these many intervening years—the subject always changes after the frontier moves on, but the wonderful sparks have remained. I suppose we *both* became a bit evangelistic (his words), but there *was* a good product to sell that summer."

Geoffrey Burbidge summed up for me some of the informal aspects of the summer school in a letter: "We remember it very well. It was the first summer school that we had attended, and the only one, including all those since, at which we were students and not lecturers. It was very hot and sticky through the whole period of the summer school, and I remember that George Gamow literally perspired alcohol as he lectured. For me, this was a most important meeting in that I first got to meet Allan Sandage. Allan presented the first results on his color-magnitude diagrams and related this to the theoretical work he had done with Schwarzschild at Princeton. Allan and I started to argue about stellar evolution, because he was almost evangelical in his approach, and I simply had to respond to this. We also first got to know Walter Baade, who was a delightful character who really enjoyed talking to younger people. . . ."

"We clearly learned a great deal about astrophysics at that meeting and even more about the way that it gets done. At the time, we were on our way back to England with no clear understanding that we would return, but after that meeting I think it was obvious at least as far as I was concerned, [that] there was far more going on in the US than we could expect to find in England. I had decided while in Michigan to go to the Cavendish Laboratory in Cambridge rather than to the University of Manchester. This was a good decision and led us to interactions with Willie Fowler and then with Fred Hoyle."

In a postscript Margaret Burbidge added: "We had just started to think about nuclear reactions in element synthesis in stars, spurred by Fred Hoyle's 1946 paper to the Royal Astronomical Society and the beginning of abundance determination in old vs young stars and composition differences in evolved stars. The Michigan summer school came at just the right time for us, when we were on our way to Cambridge and ready to start work on spectra taken at McDonald Observatory."

In response to these reminiscences Sandage wrote to me that "there are many people that I remember from first encounters that summer, people who later became fast friends and good colleagues. The most important person for me personally, as it subsequently turned out, was Ed Dennison, who was in the middle of his fundamental work on the intensity profiles of elliptical galaxies. . . . His important data for the giant E galaxy NGC 3379, which he sent me several years later, was used in 1956 in the work I did for Milton Humason and Nicholas Mayall in their redshift paper for the correction of galaxy magnitudes. Without the strong interaction with Ed that summer, this work would not have been done; strange as it may seem at this late date, no reliable galaxy intensity profiles beyond about 20 from Hubble were known then. . . ."

"My final recollection is of the return trip to California after the summer school was over. I had been Baade's student during the previous four years, and we had come to feel somewhat at ease with each other. Baade had decided to buy a new car at the factory in Detroit, to take delivery on the last day of the school, then to drive the long road back to California, asking me to ride with him. Ed Dennison drove us both to the factory pickup place for a new 1953 Chevy. After the car was in Baade's posses-

The H-R Diagram and Stellar Evolution

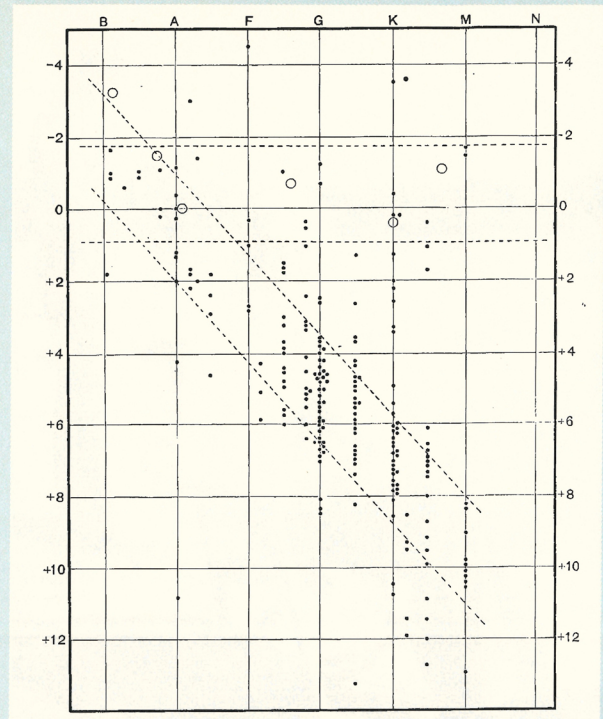
In 1913 the Princeton astronomer Henry Norris Russell plotted the absolute magnitudes of stars with known distances against their spectral types (which was to a first approximation equivalent to plotting their luminosities versus their colors or temperatures). The Danish astronomer Einar Hertzsprung had independently done the same thing in 1911. Both men found that stars seemed to fall neatly into three groupings. (The figure at top right is Russell's original.) The so-called main sequence cut a swath from the upper left to the lower right and contained the majority of stars. The stars that were above (more luminous than) the main sequence were called the "giants," because for two stars of the same surface temperature, luminosity increases with surface area. For the same reason those stars that fell in the lower main sequence or below were called "dwarfs." Although the diagram today carries both astronomer's names, it was Russell who effectively publicized it as a scheme for exhibiting the way stars evolve.

Russell's initial theory—that stars originated as cool giants, gradually heated up (moving left along the giant branch) and then slowly cooled (moving down and right along the main sequence)—was doomed after Arthur Eddington demonstrated that the main sequence was a distribution of stars of different masses, the more massive stars being more luminous.

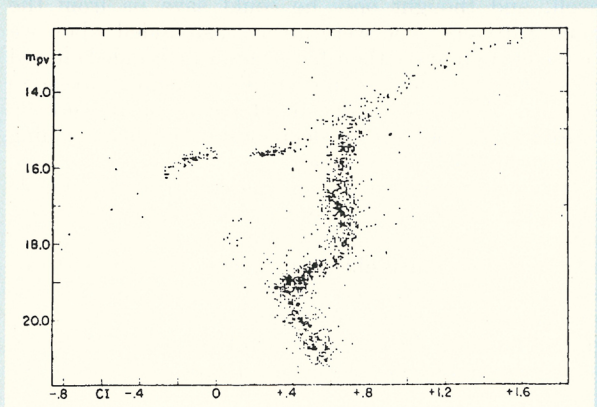
In the late 1920s Russell proposed that stars evolved from right to left across the diagram, with the main sequence resulting from a comparative stasis during which the stars burned some unknown nuclear fuel over vast eons. To make the scheme work, he proposed two fuels, "dwarf stuff" and "giant stuff." The discovery in the early 1930s that stars were primarily composed of hydrogen allowed Carl-Friedrich von Weizsäcker in Germany and Hans Bethe at Cornell in 1938 to determine the specific reactions that could power these stars as they evolved along the main sequence. Meanwhile, despite the best efforts of George Gamow, S. Chandrasekhar and others, the evolution of the giant stars remained a stubbornly recalcitrant mystery until the early 1950s.

Walter Baade's contribution to the understanding of stellar evolution was the recognition that the brilliant, hot, blue stars in the spiral arms of galaxies were part of a pattern (population I) distinct from the redder, less luminous stars characteristic of galactic nuclei (population II). At the time of the Michigan symposium, Baade had just begun to realize that the populations might represent stellar groups of fundamentally different ages, with the brilliantly profligate and luminous blue population I stars clearly youthful and destined to evolve or burn out rapidly.

Recognizing that the population II stars resembled those of globular clusters, Baade put a group of young graduate students, Allan Sandage, Halton Arp and William Baum, onto the problem of measuring the colors and magnitudes of the stars in the globular cluster M92. As a thesis project Sandage measured the globular cluster M3; The figure at bottom right shows his results. To make sense of the H-R diagram for M3, Sandage and Martin Schwarzschild explored the idea that red giants of population II had evolved off the main sequence. The



H-R diagram thereby provided a kind of cosmic chronometer for the age of the cluster, although in 1953 the details were still far from clear. Eventually Schwarzschild, collaborating with Fred Hoyle and with Richard Härm, tracked the red giant stars to a region in the upper right of the diagram where the internal temperature became high enough to ignite helium burning. The giant star, quickly readjusting its internal structure, would then begin a relatively rapid shrinking and surface warming, resulting in a trajectory downward and to the left in the H-R diagram.



sion, Ed said he would follow us for a few miles simply to see if all was OK. We started down the road out of Detroit, toward California. Baade, (essentially) a new driver, was driving half on the road and half on the shoulder (sometimes), or half on his side of the road and half on the other oncoming side, alternately. Ed signaled us to stop and inquired through the rolled-down window

if there was something wrong with the steering wheel. Baade, sensitive, said all was OK, thank you very much, and we would be on our way. A few minutes later, we unfortunately were.

"Baade had no sense of microcompensation of the steering. He would keep the wheel in a rigid position until it was clear that he had to change, not microscopi-



Baade on Bass Lake. Despite the busy schedule of the summer school, the participants found time to relax. This slide shows Baade relaxing by the lake with Tom Matthews fishing in the background.

cally but grossly, long after it became evident that it was required to do so. We went across the country, for 6 days, in large triangles, first steering toward the middle of the highway, and then toward the corn or wheat fields, or later toward the canyon dropoffs, on the right. The first day was, by far, the worst because I was frightened. I offered to drive, but like all great men, Baade believed in himself, and thought only he could save us from the oncoming drivers, who were always astounded when we got close enough to see their faces, and who Baade believed were simply poor drivers that should be denied access to the road. The trip became a bit easier as it wore on because I could not help but sleep most of the day, avoiding the constant thrill of the road.

"To save on expenses, we had agreed to share a double room in the motels along the way each night. However, Baade was a most accomplished snorer, so accomplished that it is simply impossible to describe. The walls shook, the sound often penetrated into the adjacent rooms of the motel, causing pounding on the partitions, *etc.* After two nights of no sleep for me, but restful sleep for the driver, I slept away many of the driving hours after about the third day.

"But the real memory of the trip was the conversation about astronomy on that cross country adventure. Baade, like all scientists of substance, had a set view of how things were put together—to be sure a view to be always challenged by the scientist himself, but defended as well against all less informed mortals who objected without *simon pure* (a favorite Baade expression) reasons. . . . The trip then became a riding commentary on much of the world of astronomy and astronomers. But that is a rather different story, rather than the magic summer of 1953, which in many ways began the outside world's discussion of Baade's ideas that, together

with Schwarzschild's, spearheaded the modern understanding of stellar evolution."

A unique transforming experience

In 1953 the space age was in the dim future, and the astronomical fraternity was actually quite small. Thus a symposium with 50 in attendance represented a comparatively large impact on the astronomical community. Furthermore, astronomy was just regaining the momentum lost during the years of World War II, and it had scarcely been well nourished during the depression years of the 1930s. This configuration of circumstances made the Michigan summer school a unique, unrepeatable experience, although it certainly served as a model for many subsequent astronomical summer schools around the world.

For some of us, who were just in the initial stages of professionalization, a metamorphosis that takes place in graduate school, the 1953 Michigan Symposium on Astrophysics was a wonderfully formative experience. I remember when Tom Matthews and I first encountered Baade in the parlor of the fraternity house. He had arrived about a week after the summer school had begun, but before his own formal lectures were scheduled. Cecilia Payne Gaposchkin (Harvard's very first woman professor to come up through the ranks) had primed Tom and me with a host of questions before we left Cambridge, so without introductions, we started plying Baade with our queries. Some days later he remarked, "I didn't know who you were, but you talked like astronomers and that was enough." That's professionalization in progress!

In retrospect, for me the most memorable parts were the many splendid friendships that I made in Ann Arbor, especially with Baade and Sandage, although I was also to profit immensely from the Baade photographs in my subsequent teaching. The symposium helped to link together an entire post-World War II generation of American astronomers in a way the NSF could scarcely have guessed.

References

1. Shapley Collection, Harvard U. Archives, Cambridge, Mass.
2. Oral history interview conducted by S. Weart, Niels Bohr Library, American Institute of Physics, College Park, Md. ■