THE DISCOVERY OF THE SPIRAL ARMS OF THE MILKY WAY

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Abstract Attempts in the 1930s and 1940s to determine the spiral structure of the Milky Way by star counting methods, essentially the continuation of the work of the Kapteyn Astronomical Laboratory, failed to reach this goal. A new foundation for the search was laid by Walter Baade in his studies of stellar populations. With the recognition that highly luminous objects, especially H II regions, would outline the spiral structure, W.W. Morgan and his young associates Sharpless and Osterbrock carried out the observational program that first delineated, in 1951, the nearby arms of the Milky Way. The full paper was never published, so the historical details have remained somewhat vague, primarily because the 21-cm discoveries so quickly overtook the optical researches.

In 1937, in his The Distribution of Stars in Space, Bart J. Bok concluded:

> Working models for the galactic system have, at various past stages of development, been of value for the co-ordination of existing knowledge and the effective planning of future research. Shapley's model, in which the local system played the role of an important subsystem, has proved eminently satisfactory for the past twenty years. The time has now come to go one step farther and consider a more detailed working model. Several astronomers have stressed the probable similarity in structural features between the galactic system and some of the larger spiral nebulae. Seares has suggested that our stellar system may well have a structure similar to that of Messier 33, the well-known spiral nebula in Triangulum. Our sun would then be located in one of the spiral knots, at a distance equal to two-thirds of the radius of the nebula from the center.
>
> It is surprising to note how well such a model agrees with the general impression obtained when the Milky Way is viewed from the tropics. The best view may be had at sidereal time 15-16 hours, when the Carina region is setting, Sagittarius is well up in the sky,

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> and the cross of Cygnus is rising above the horizon. No one who had the privilege of thus seeing the Milky Way in all its grandeur would ever deny that the Sagittarius cloud marks the central region of our galactic system. Individual stars do not stand out particularly against the brilliant continuous background of the Milky Way in Sagittarius; but the impression is quite different for the Cygnus and Carina clouds, in which a multitude of individual stars is seen projected against a faintly luminous background. The observer in the tropics should not find it difficult to accept as a working model for our Milky Way system one with a distant center in Sagittarius and in which a spiral arm passes from Carina through the sun toward Cygnus.

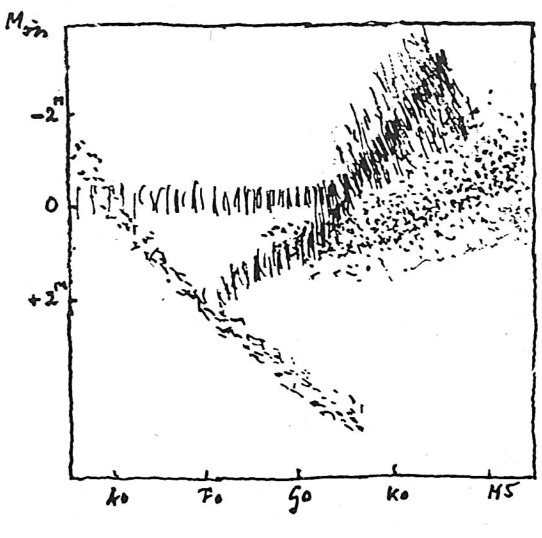
Bok had originally been inspired to study astronomy by the popular writings of Cornelis Easton in _Hemel en Dampkring_. Easton, a journalist and skilled amateur observer, had made his mark on astronomy by his careful drawings of the Milky Way and his speculations that spiral structure could be discerned visually. At the instigation of Kapteyn, Easton received an honorary doctorate from Groningen in 1903. In 1913 his Milky Way studies culminated with an article in the _Astrophysical Journal_.[1] Although he depicted the sun just off the center of a spiral whose nucleus lay in the direction of Cygnus, he was careful to state that "I am well aware that the great problem of the Milky Way can never be solved in this way, and that we may aim only at a plausible interpretation of known facts, and at a working hypothesis... The figure in the center of our plate does not pretend to give even an approximate representation of the galactic system, but only to indicate in a general way how the stellar accumulations might be arranged so as to produce the phenomenon of the Milky Way--on the supposition of a spiral galaxy."

Within a few years Harlow Shapley proposed an entirely different layout for the Milky Way, and Easton abandoned his earlier ideas of visible spiral structure. As a consequence of Easton's writings, Bok too became an enthusiastic Shapley supporter. At Leiden Bok studied under Ehrenfest and Oort, at the time when Oort worked out his famous equations for the rotation of the Milky Way, which turned out to be one of the most convincing arguments for Shapley's model. In 1929 Bok took a fellowship at Groningen under Kapteyn's successor, P.J. van Rhijn; then at Harvard he mined the observatory plate collection for his dissertation research on Eta Carinae, but he actually received his doctorate in Groningen for this work. Since both Oort and van Rhijn had been students of Kapteyn, Bok can be considered a third-generation astronomer of this distinguished Dutch school. More than anyone else he developed and applied the numerical methods originated by Kapteyn toward the problem of Milky Way structure, and in particular toward the delineation of its spiral features.

The history of astronomy shows repeatedly that well-defined problems are often solved by totally unexpected lines of research, and this proved to be the case for the spiral structure of our galactic system. Over a decade of devoted starcounting and analysis, particularly under Bok's direction at Harvard, failed to disclose the expected stellar density concentrations that could be identified with the spiral arms. The solution to this puzzle lay elsewhere, with the observational analysis of the Andromeda Nebula and other nearby galaxies. This work was carried out by Walter Baade at Mount Wilson, and it took particular advantage of the dark skies produced by the wartime blackout of Los Angeles and Hollywood.

Like Bok, Baade had been inspired by the work of Harlow Shapley, in his case by the physical nature of pulsating stars. In 1931 Baade received an offer to join the staff at Pasadena, an opportunity he had accepted immediately. He eventually applied for American citizenship, but lost the papers, and with a characteristic disdain for bureaucracy never reopened the matter. Thus, when the United States entered World War II, he was classified as an enemy alien, unfit for war work, and consequently he had free rein with the 100-inch reflector during those dark years. Baade pushed the Hooker telescope to its very limits in order to resolve the inner portions of M31 and its satellite galaxies M32 and NGC 205. The results were published in 1944 in a famous article in ApJ 100, in which he distinguished between two stellar populations and introduced the terms Type I and Type II. He concluded by stating that[2]:

> Although the evidence is still very fragmentary, there can be no doubt that, in dealing with galaxies, we have to distinguish two types of stellar populations, one which is represented by the ordinary H-R diagram (type I) the other by the H-R diagram of the globular clusters (type II). Characteristic of the first type are highly luminous O- and B-type stars and open clusters; of the second, globular clusters and short-period cepheids.... Both types coexist, although differentiated by their spatial arrangement, in the intermediate spirals like the Andromeda nebula and our own galaxy.



A sketch of the H-R diagram of the two stellar populations sent by Walter Baade to Cecilia Payne-Gaposchkin in 1947. Giant branches of type I are shown by the dots, of type II by the vertical hatching.

Baade also added that the same two types of stars had been recognized, from their differing motions, by Oort in 1926. Because of their common interests in galactic structure, a lively correspondence ensued after the publication of Baade's classic paper. In 1946 Baade wrote to Oort[3]:

> You mention in one of your remarks that the classical cepheids would be objects par excellence from which to determine the spiral structure. I think it is not certain yet that the longer period cepheids are especially concentrated in the spiral arms (they occur in the same regions in which the arms occur). But the B-stars of high luminosity are strongly concentrated in the spiral arms as my UV-exposures of the outer parts of M31 show most convincingly. I am therefore wondering, after reading Blaauw's fine paper about the Scorpius-Centaurus cluster whether this extraordinary aggregation of B-stars is not in reality a short section of a spiral arm, the more so because in its orientation and motion it would fit perfectly into the expected picture (the arms trailing).

Early in 1947 Oort replied, saying[4]:

> I quite agree that a study of the early B-type stars would be one of the most important steps for finding the spiral structure of the Galactic System. I have been discussing this subject with Van Rhijn for some time, and when Van Albada left Holland in order to pass a year at Cleveland we suggested to him that he should try to start a program with the Schmidt camera for finding faint B-type stars in the Milky Way (Van Albada, by the way, is a very intelligent and original young astronomer, a former student of Pannekoek). This is a large programme, however, and I don't think the Warner and Swasey people are sufficiently interested yet to start it on a sufficiently big scale. How about future possibilities with the large Schmidt cameras on Mt. Palomar?[16]

But, in fact, J.J. Nassau at the Warner and Swasey Observatory did eventually start just such a large-scale program, of which more presently. Although Baade himself did not get involved with such a program at Palomar, he began to appreciate that this must be the direction for solving the problem of spiral structure, rather than by less discriminating methods of star counting. On this latter topic he wrote rather scornfully to Leo Goldberg in 1949. Baade had been asked to give the opening address at the dedication of the Curtis Schmidt telescope at Michigan, and to advise on a related symposium. "The idea to celebrate the dedication of your new Schmidt with a symposium on galactic structure seems to me most appropriate," he replied, adding[5]:

> I shall be glad indeed to pitch in to the best of my ability. I have only some doubts whether I would be the proper man for the proposed opening lecture. People expect on such occasions to be edified and uplifted by tales of heroic achievements and I fear I could not accomodate them in this respect re galactic structure if I went much beyond old William Herschel. But the main thing is the symposium

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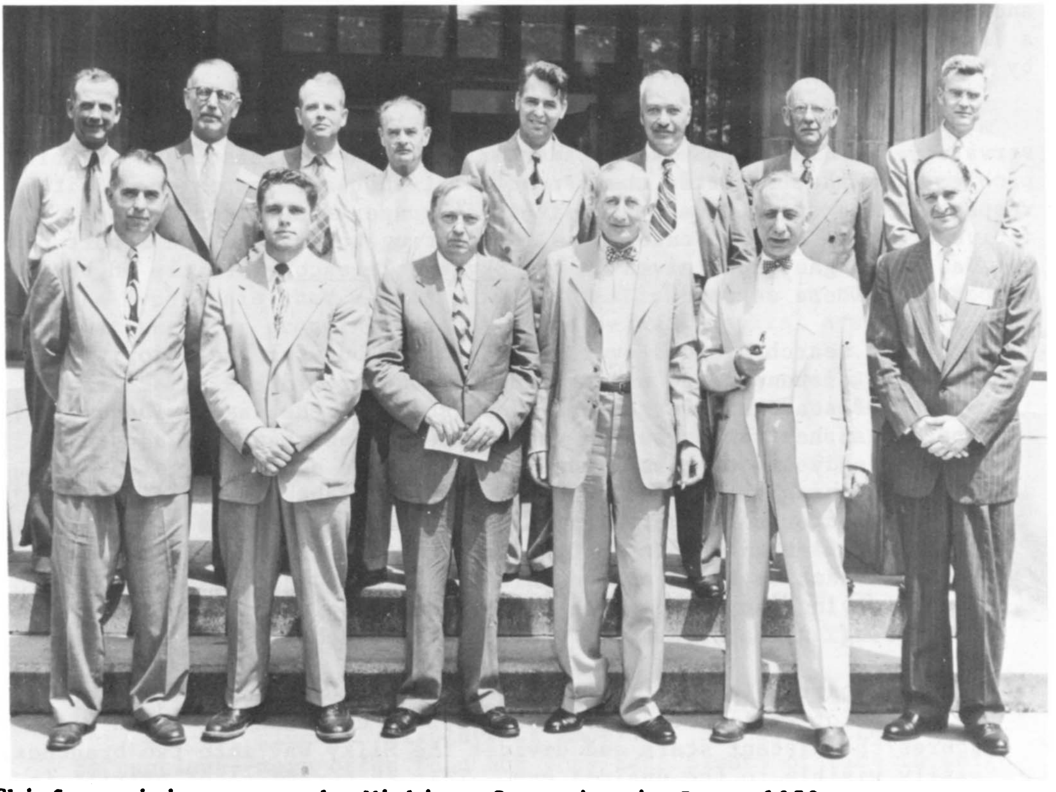
> and I hope you can arrange the program in such a way that it revolves around the really fundamental questions. No papers about the stellar distribution in a field in Cepheus, etc. What we all would like to know is: What is the large scale structure of our galaxy and which roads appear promising at present.... I realize that with the inadequate equipment of their observatories many astronomers were simply forced to restrict their research to our immediate solar neighborhood with the natural result that the fin de siècle ideas regarding what constituted the problem of galactic structure survived longer than they otherwise would have had. But with large Schmidts coming up now everywhere it is time to reassess the situation and your symposium would offer a splendid opportunity to do just that.

Indeed, the symposium was convened in June 1950, with a nucleus of the most eminent researchers present. Baade had particularly expressed his hope to Goldberg that Oort would accept an invitation, and Oort's absence was perhaps the most disappointing feature. Baade gave both the opening lecture and the first symposium paper itself, on "Galaxies--Present Day Problems." He addressed a wide variety of issues before he



Chief participants at the Michigan Symposium in June, 1950. Front row (l. to r.): F.D. Miller, K.G. Henize, H. Shapley, W. Baade, J.J. Nassau, and L. Goldberg. Back row: G. Abetti, B. Lindblad, W.W. Morgan, A.N. Vyssotsky, N.U. Mayall, R. Minkowski, J. Stebbins, and S.W. McCuskey.

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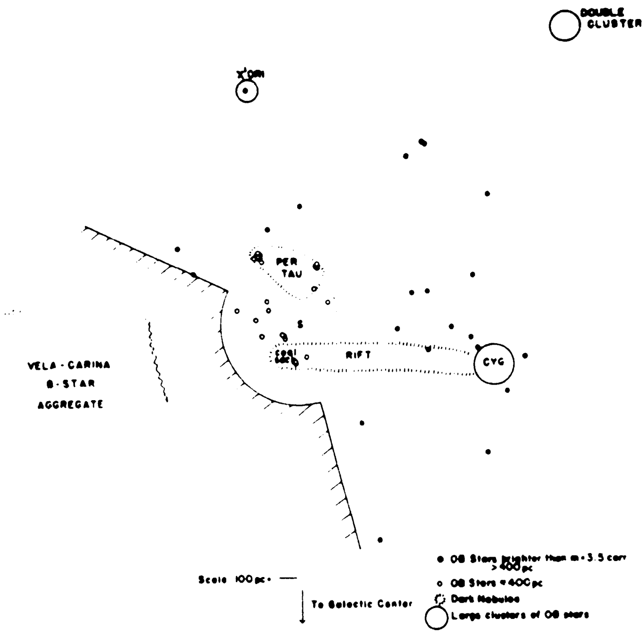
came to his final point, "Our Galaxy as a Spiral Nebula."[6] Baade wrote, "We have, I think, convincing evidence now that our galaxy is an Sb spiral, because it has a nucleus similar to that of the Andromeda nebula and not to that of M33." He described first his attempt with Sergei Gaposchkin to probe for the variable stars embedded in the galactic bulge. He reiterated his view that our galaxy had a nuclear lens made up of population II stars. Then he turned to the spiral structure itself as another problem ready for attack. "The procedure in this case is obvious. Since the supergiants of the population I are restricted to the spiral arms, we have to study their spatial arrangement in the solar neighborhood. The most promising stars for a first test are undoubtedly the O and early B stars, on account of their high frequency in spiral arms. But we will need for each star accurate data on the following, in order to determine its position relative to the sun: apparent magnitude, absolute magnitude, and color excess. Since apparent magnitudes and color excesses of most of the O and early B stars brighter than 7.5 visual and north of declination -30^{o} are already known, their individual absolute magnitudes are the only remaining desideratum. W.W. Morgan's spectroscopic luminosity criteria for O and B stars should fill this gap and it is, I think, no secret that Morgan and Nassau are now engaged in a large program of determining the absolute magnitudes of O and B stars by this method."

It was, of course, no secret that William W. Morgan of Yerkes Observatory and Jason J. Nassau at Warner and Swasey were at work on this problem, for they reported their results at the same symposium.[7] With respect to the spiral arms, however, their paper took a very conservative stance. In this case it is fascinating to examine the report of the Nassau-Morgan paper given in the Sky and Telescope article on the symposium. There we read[8]:

> The search yielded over 900 OB stars, but for the majority of them the distances are undetermined. However, for 49 relatively nearby OB stars and for three groups shown on the diagram below, Dr. Morgan has collected the required data. Combining the results with already existing knowledge of many facts about the galaxy and other galaxies, these astronomers suggested that the sun is located near the outer border of a spiral arm. The arm extends roughly from the constellation Carina to Cygnus. The fact that many faint and hence distant OB stars are found toward Cygnus indicates that we are observing the stars in the extension of this arm beyond the clustering in that constellation, that is, beyond 3,000 light years.
>
> The part of the spiral arm near our sun contains a large cloud, or groups of small clouds, of interstellar dust and gas which obscures the distant stars and divides the Milky Way into two branches, easily visible to the unaided eye. This obscuring cloud or rift is in the shape of a slightly bent cigar and is over 3,500 light years long. At one end of it is the southern Coalsack and at the other the brilliant group of OB stars of the Northern Cross... Dr. Nassau cautioned, however, that the evidence is insufficient to preclude



The plot by Nassau and Morgan of 49 OB stars and 3 OB groups for which distances were determined, from the Michigan Symposium volume (p. 50). The position of the sun is shown by S and the cross-hatching designates the limit of the survey.

the hypothesis that a great disorganization exists in the galaxy and that the star groupings do not trace definite spiral arms.

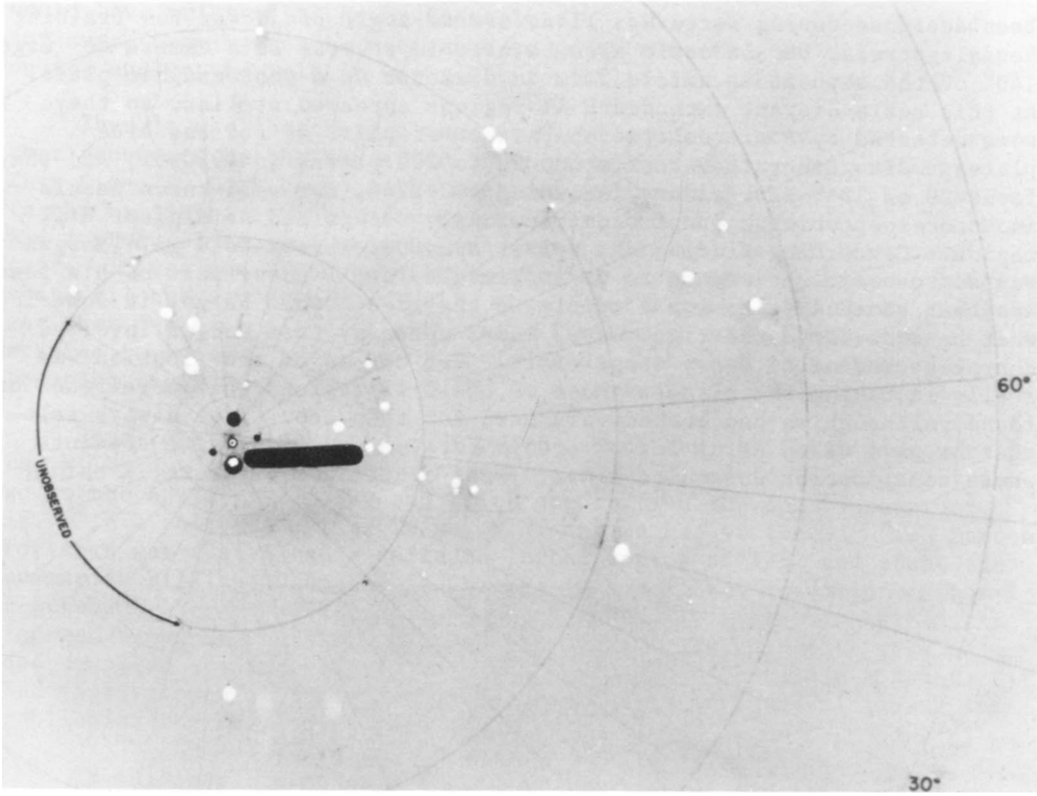
An entirely different reception to these studies came approximately 18 months later at the 1951 Christmas meeting of the American Astronomical Society in Cleveland, where Morgan presented new results based partly on the OB stars but largely on an investigation of H II regions. By examining the distribution of emission nebulae, Morgan, together with Stewart Sharpless and Donald Osterbrock, was able to delineate segments of two spiral arms, one that passes through the sun and the other at a distance of over 6,000 light years in the direction away from the galactic center, that is, about twice as far as the limits of the earlier Nassau-Morgan work. Concerning the Morgan-Sharpless-Osterbrock paper, Otto Struve wrote[9]:

> Astronomers are usually of a quiet and introspective disposition. They are not given to displays of emotion. Moreover, they tend to be cautious--more often than not they take plenty of time to weigh the evidence of any new and startling development before they accept it. But in Cleveland, Morgan's paper on galactic structure was greeted by an ovation such as I have never before witnessed. Clearly, he had in the course of a 15-minute paper presented so convincing an array of arguments that the audience for once threw

caution to the wind and gave Morgan the recognition which he so fully deserved.

From a historical perspective we need to look more closely at the research pattern that made the difference between the Michigan Symposium and the Cleveland AAS meeting. Although, as Baade had stated, the procedure was obvious, carrying it out was not. Finding the 900 OB stars was only the first stage. Determining luminosity criteria and finding the really faint specimens was more difficult. Morgan had long appreciated that for studies of galactic structure, accurate spectral types with luminosity classifications would be required to get absolute magnitudes, and to that end he had produced in 1943 with P.C. Keenan and E. Kellman the MKK Atlas of Stellar Spectra. Furthermore, high luminosity objects would be crucial, and for these accurate color indices would also be essential in order to correct the photometric distances for absorption. Thus, Morgan had correctly analyzed the approach for finding the large-scale galactic structure, and had established the basis for a successful program. What remained was to find the truly distant high-luminosity objects.

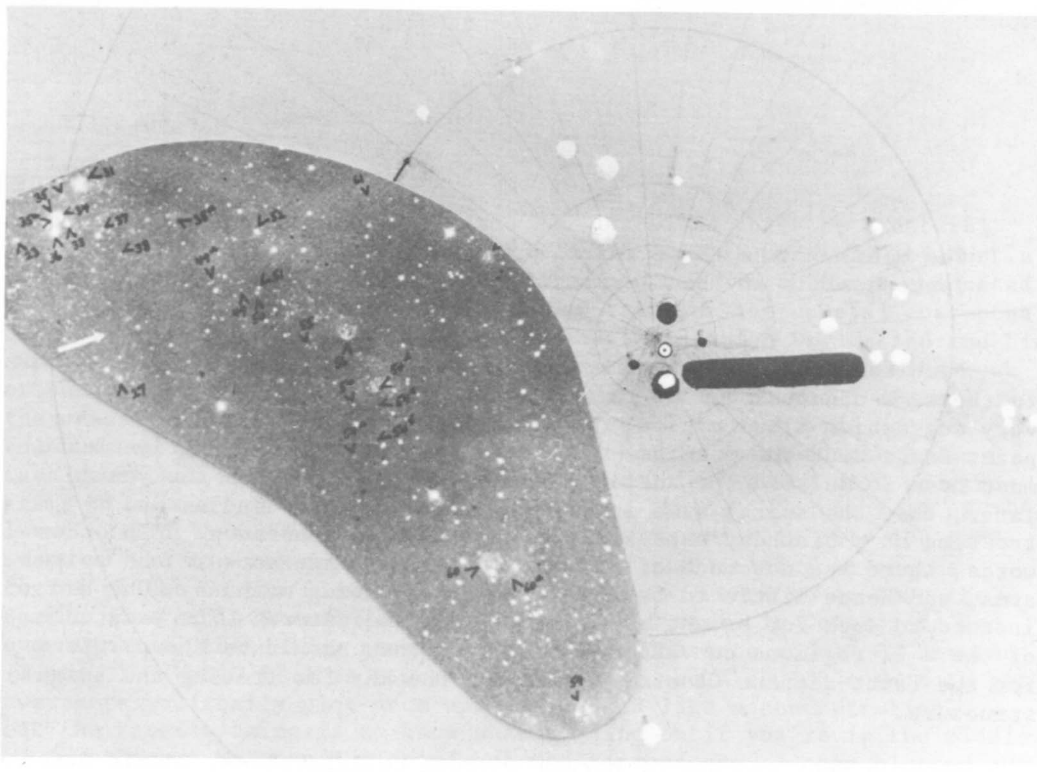
In an oral history interview made a few years ago at Yerkes Observatory by David DeVorkin of the American Institute of Physics,[10] Morgan remarked that he had two papers at the Michigan Symposium, a joint one given by Nassau on the arrangement of the B stars in space, "which at the time had not gone far enough to show anything but a beautiful Gould belt... But my own paper in it was a description of what was called natural groups in stellar spectra." In that second paper Morgan had coined the expression "OB stars" and had described these as a natural group with little spread in luminosity. "It made it possible, by just a glance, a few seconds at each spectrum ... to tell if a star was located in this area [of the H-R diagram].... Now this was the crucial conceptual development. This was then applied to a program which Dr. Nassau and his associates and helpers worked on. I used to go to Cleveland for a week or so every few months, for a number of years. Nassau and I did all of the classifying.... We had a belt I believe 10 degrees wide, as far south as we could get around the sky, and this [furnished] the basic catalogue that was used here [i.e., at Yerkes] for taking slit spectrograms of as many of those stars as possible. Anyway, in the fall of 1951 I was walking between the observatory and home, which is only 100 yards away. I was looking up in the northern sky, just looking up in the region of the Double Cluster, and it suddenly occurred to me that the Double Cluster in Perseus and then a number of stars in Cassiopeia and even Cepheus, that along there I was getting distance moduli of between 11 and 12. Well, 11.5 is two kiloparsecs, and so I couldn't wait to get over here and really plot them up. It looked like a concentration.... but the hardest thing is to know what's going on if you're in the middle of something. So when I plotted out the Perseus arm, I then plotted out the other stars, and it turned out through the sun there was this narrow lane parallel to the other one. So that's the way it happened. It was a burst of realization. It was not a question of a reasoned process of steps."



The model of the three spiral arms, as shown by Morgan on a lantern slide at the Cleveland AAS meeting in December, 1951.

Simultaneously with that work, another line of evidence was leading to the same conclusion. At the Michigan Symposium Baade had shown a very suggestive illustration, of the H II regions in M31, and at some point Baade sent the original plate to Morgan. Around this time Baade must have formulated the analogy, which I heard him use a few years later, that the spiral arms in M31 are much like the candles and frosting on a birthday cake--all show and little substance. In other words, there was not much of a stellar density difference in the spiral arms, and hence little to be found by star counting methods. One had instead to look for bright and showy spiral indicators. The existence of the H II regiions convinced Morgan that they should be the pointers for the faint-distant OB stars and hence the key for tracing the spiral structure.

About a year ago one of Morgan's young collaborators, Donald Osterbrock, provided me with details concerning the Yerkes search for the galactic H II regions.[11] As assistants for Morgan, Osterbrock and his fellow graduate student Stewart Sharpless set up the wide-angle Henyey-Greenstein camera for their survey. This complex optical system had been designed during World War II as a wide-angle projector for training aerial gunners, but it could also be used in reverse as a camera to image 140° of the sky onto a circle 2 cm in diameter on a photographic plate. At this scale distant extended H II regions appeared stellar, so they were detected by a microscopic comparison of pairs of red and blue plates. Altogether they took about 50 to 100 plates in 1950-51, and they found 20 or 30 H II regions, including NGC 2244, the well-known nebula in Monoceros, which had not been previously recognized as a giant H II region. Concerning finding the spiral arms Osterbrock told me, "Morgan wanted to do it, he wanted to do it himself, and I guess part of his fear was that somebody else would tumble to the idea before he got it done in what he considered the right way. And I must say that Morgan involved Sharpless and me in every stage of it. Yet our major contribution was really in taking the plates. Most of the H II regions that were found he found, although we had looked very hard for them too. I've always felt that he gave us an awful lot of credit for two young graduate students whose contribution was quite minor. Many other investigators, I think,



The cover for the April 1952 Sky and Telescope cropped off the Perseus arm, but included a detail of the H II regions from Baade's plate of M31.

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would have written the paper themselves. It was an idea that he had had for many, many years; he was asked to go to a meeting to deliver a paper on it, and yet he felt it was right to include us as authors."

Thus it was that Morgan took to Cleveland a jointly authored paper that proved to be the sensation of the December, 1951, AAS meeting. This time Jan Oort came to the meeting, and in fact chaired the session. In the oral history interview Morgan described the situation[12]: "Oort had introduced me, and when he sat down to listen, he sat down in my seat. It was one of those steeply sloping classrooms at Case with the seats all the way up high. Well, when I got through, the first thing was that I had no place to sit down. The second thing was people started to applaud by clapping their hands, but then they started stamping their feet. It was quite an experience."

Remarkably enough, the full paper describing the first discovery of the Milky Way's spiral arms was never published and it is necessary to go to the April, 1952, Sky and Telescope to find the best account of it.[13] In a poignant letter to me, W.W. Morgan has written,[14] "The reason for this was that I had a collapse in the spring of 1952 and spent the summer in Billings Hospital in Chicago in a helpless condition. When I returned to Yerkes in October, I had my partially written paper waiting for me, begun in the early part of the year; I was unable to work on it and complete it; instead, I wrote the UBV paper with Harold Johnson. The rapid growth of radio astronomy resulted in my never finishing and publishing the original paper."

In this way a problem that had stood at least three decades, and which had been one of the critical goals of the kind of studies in galactic structure pioneered at the Kapteyn Astronomical Laboratory, finally found its solution by a quite different avenue from the numerical star-counting procedures. Yet the analysis that grew out of the earlier Dutch studies soon found another and even more powerful application in the interpretation of the line profiles of the 21-cm radiation from neutral hydrogen, radiation that had been discovered in the same year, 1951, as the optical discovery of the spiral structure from the ionized hydrogen. Almost immediately Oort and Bok and their students began a vigorous investigation of the Milky Way structure using radio wavelengths, and by 1952 the spiral structure had been confirmed and extended using the radio methods.[15]

Nevertheless, I think there is some larger justice in the circumstance that the optical studies, on which so many decades of effort had been spent, narrowly won the race with the new and powerful radio astronomy to establish the fact that our galaxy really did have spiral arms, as had long been conjectured.

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2. W. Baade, Astrophys. J. 100, pp. 137-150, 1944.

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4. J. Oort to W. Baade, 11 January 1947.

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6. W. Baade, Publ. Univ. Michigan Obs. 10, pp. 7-17, 1951.

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9. O. Struve, Leaflet Astron. Soc. Pacific No. 285, January, 1953.

10. Oral history interview with W.W. Morgan, American Institute of Physics. I wish to thank Prof. Morgan and the Niels Bohr Library, AIP, for permission to quote from this interview. The quoted passage has been somewhat condensed.

11. Oral history interview with D.E. Osterbrock, on deposit at American Institute of Physics. I wish to thank Prof. Osterbrock for permission to quote from this interview and for his comments on the draft of this article. Photographs of the H II regions made with the Henyey-Greenstein camera are found in S. Sharpless and D. Osterbrock, Astrophys. J. 115, pp. 89-93, 1952.

12. Interview with Morgan [note 10].

13. Sky Telesc. 11, p. 138, 1952; reprinted in K. Lang and O. Gingerich (eds.), Source Book in Astronomy and Astrophysics, 1900-1975, pp. 638-642, (Cambridge, Mass., 1979); the abstract of the Morgan, Sharpless, Osterbrock paper appeared in Astron. J. 57, p. 3, 1952.

14. W.W. Morgan to O. Gingerich, 25 March 1982.

15. See K. Lang and O. Gingerich, op. cit. [note 13], pp. 643-651.

16. The Van Albada mentioned here is G.B. van Albada, later Director of the Lembang Observatory and of the Astronomical Institute at Amsterdam. (Editor.)