STUDIES OF THE MILKY WAY 1850-1930: SOME HIGHLIGHTS

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"The Copernicus of the sidereal system is not to be expected for many generations". So wrote R.A. Proctor[1] in his Essays in Astronomy in 1872. Indeed things did look bleak at this time for those who hoped for a good understanding of the size and structure of the Galaxy. Why was this so, and why was there to be such an astonishing transformation of this situation between 1918 and 1930? Certainly these twelve years saw the widespread acceptance of no less than six fundamentally new ways of viewing the Galactic System. These profound shifts, occurring in such a short time, form, I would suggest, one of the most exciting chapters in the entire history of astronomy. And in this paper I shall attempt to describe and analyze what these changes were, what led up to them, as well as to examine the events surrounding them.

EARLY IDEAS

But to put the developments of the 1910's and 1920's into context, let's first travel back to the years around 1850 and then work our way forward. How, then, was the Galactic System viewed in 1850? An astronomer who wanted an authoritative account of the latest thinking of the Galaxy would quite likely have turned to John Herschel's volume Outlines of Astronomy[2]. Here our astronomer would have found that Herschel had emphasized the complexity and irregularity of the Milky Way, the consequence of his having spent many hours observing in both the northern and southern hemispheres. Herschel further identified four great clouds of stars that he argued were distant extensions of the Milky Way. One of them was the Orion Nebula. The conviction that the Orion Nebula was a huge star cloud stemmed chiefly from the observations made with Leviathan of Parsonstown, Lord Rosse's 72-inch telescope in Ireland. Rosse, it seemed, had resolved the Orion Nebula into stars, and given its apparent size, we can understand Herschel's belief that the Nebula was a giant star cloud. This seems very odd now, but we need to remember that in the 1850's it was widely accepted that all nebulae were star systems.

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The Orion Nebula and other similar great star clouds were moreover employed by Herschel as models for the Galaxy itself, since, as he wrote, "could we view [the Galaxy] as a whole, from a distance such as that which separates us from these objects, [it] would very probably present itself under an aspect quite as complicated and irregular." But Herschel's thinking on the matter was not settled, for at other times he inclined toward a ring model. In this the Sun was in a relatively empty region of space, separated from a denser ring of stars. However, the important point here is that Herschel's conception of the Galaxy was very dependent on his beliefs about other stellar systems. Here indeed is a theme that runs through the history of galactic studies between 1850 and 1930, and the theme is that the way astronomers viewed the Galaxy depended intimately on their opinions and beliefs about other galaxies.

Now there were a number of different views to John Herschel's, and there was no consensus in the early 1850's on the nature and size of the Galaxy. Without good distances progress in galactic studies seemed a remote goal, and this realisation helps explain why during the nineteenth century astronomers were, in general, little concerned with the distribution of the stars. For most of them, for most of the time, it was sufficient that the stars' positions be catalogued, in order that the motions of Solar-System objects be followed more exactly. Those who researched the structure of the Galaxy or the nature of the nebulae, were thereby placing themselves somewhat out of the mainstream of astronomy. For example, in a text-book published in 1852, the Reverend Robert Main, First Assistant at the Royal Observatory, Greenwich, devoted only 14 of the book's 155 pages to the stars and nebulae and he referred to the Milky Way only in passing. The nineteenth century thus ended with astronomers knowing for sure little more than William Herschel's starcounts had shown in the 1780s: that is, that the plane of the Milky Way contains more stars than are to be found in other parts of the sky.

There had, nevertheless, been some developments towards the end of the century that, with the benefit of hindsight, we can see sowed the seeds of a spectacular blossoming of galactic astronomy.

First, there was renewed interest in the idea that the Galaxy is a spiral. This had first been proposed in the middle of the nineteenth century, but owed most to the Dutch amateur astronomer Cornelis Easton. In 1900, for example, he wrote on 'A new theory of the Milky Way'[3]. Here he contended that the latest observations of nebulae had demonstrated the spiral to be a much commoner form than had previously been supposed. This was due in part to the habit of astronomers in the late nineteenth and early twentieth century to see 'spirality' in all sorts of objects. Might the spiral structure, Easton asked, be the plan on which the Galaxy was designed? He fleshed out his hypothesis with a sketch of the stellar system as a spiral, though he warned that it was not intended to give even an approximate representation of the actual Galaxy.

Easton's hypothesis gained in popularity during the first two decades of the century. The major reason for this was that many astronomers were ready to admit the existence of visible external galaxies, or island universes as they were sometimes called. The candidate island universes were the spiral nebulae, a remarkable turn around from the position near the turn of the century, when they had generally been believed to be merely proto-solar systems. For example, when one of Isaac Roberts' photographs of the Andromeda Nebula had been shown at a meeting of the British Royal Astronomical Society in London in the late 1880s, it had caused a sensation. Many years afterwards one witness vividly remembered the reaction of the Society's Fellows: "One heard ejaculations of Saturn, the nebular hypothesis made visible, and so on". However, by the 1910s, the spiral nebulae, including the Andromeda Nebula, were widely claimed as external galaxies, and since they seemed to possess a spiral structure, many astronomers inferred that the Galaxy itself was a spiral.

The second major development around the turn of the century to affect galactic astronomy involved a handful of astronomers who were giving rise to a new kind of astronomy that some hoped would eventually enable them to discover the true arrangement of the stars. This was statistical astronomy. Dr. Paul elsewhere in this volume describes the evolution of statistical astronomy; so I shall say little about it here except to note that one important product of the endeavours of the statistical astronomers was to help bring studies of the Galaxy to a more central place in astronomy, to a place where its problems would become the concern of an increasing number of astronomers.

Now despite their hopes of achieving a better picture of the structure and size of the Galaxy, Kapteyn and other statistical astronomers laboured under a major handicap. As Kapteyn himself admitted in 1909,[4]

> Undoubtedly one of the greatest difficulties, if not the greatest of all, in the way of obtaining an understanding of the real distribution of the stars in space lies in our uncertainty about the amount of loss suffered by the light of the stars on its way to the observer.

Astronomers were particularly uneasy because the observed change in density seemed to place the Sun in a nearly central, and apparently privileged, position. It had been known since the time of William Herschel that dark regions exist in the Milky Way. During the nineteenth century it had been generally accepted that they were genuine holes or rifts, not dark clouds of obscuring matter. By the mid-1910s, many astronomers had moved toward the view that, while localised obscuring clouds did exist, the general absorption of star light is significant. This shift was due mainly to the examination by Harlow Shapley, then at Mount Wilson Observatory, of the colours of stars in globular clusters. In 1915 Shapley had estimated the Hercules globular cluster to be about 100 000 light-years away,[5] a very much larger distance that the contemporary estimates of the diameter of the Galaxy. But Shapley found no indication that the cluster stars had been reddened, and despite the fact that other astronomers believed that they had detected such an effect for distant stars, Shapley claimed that the extinction and reddening of star-light could effectively be ignored in researches on the Galaxy, and Shapley's result swayed many people.

Shapley's studies certainly freed Kapteyn of any reservations he had over assigning a nearly central position to the Sun. As Kapteyn himself told Shapley:[6] "Unless there be still a systematic error in your color indices, it seems that we may really, at least provisionally, neglect the consideration of this absorption in the study of the structure of the Milky Way system. It is almost too good to believe...I congratulate you on this achievement with all my heart."

Let's pause briefly to consider how the Galaxy was viewed in 1917, an apparently arbitrary date, but which I shall show is not so. A common view of the Galaxy was well summarized by Eddington.[7] In his widely-read Stellar Movements and the Structure of the Universe he had argued that, to give a general idea of the scale of the main part of the stellar system, "it may be stated that in directions towards the galactic poles the density continues practically uniform up to a distance of about 100 parsecs; after that at 300 parsecs it is only a fraction (perhaps a fifth) of the density near the Sun. The extension in the galactic plane is at least three times greater. These figures are subject to large uncertainties." This was one view, but just about all astronomers in 1917 regarded the Galaxy as a flattened or lens-shaped structure, as certainly no larger than 30 000 light-years in diameter, and perhaps considerably smaller. Almost all accepted that the Sun was close to the centre, while some suspected that the Galaxy's stars might be distributed in a spiral pattern.

SHAPLEY'S MODEL

Imagine, then, the astonishment that greeted Shapley's[8] announcement in 1918 that the Galaxy has a diameter of about 300 000 light-years, a staggering increase on the then current sizes. Morever, Shapley placed the Sun in an eccentric position tens of thousands of light-years from the galactic centre, a centre he argued was defined by the globular clusters.

Shapley's model was the product of a prodigious amount of intensive work, and its origins can be traced back at least to 1916.

Before 1916 Shapley had assumed, along with everybody else, that the Sun was roughly central, and that the stellar system had a radius of the order of a few thousand light-years, but in November 1916 Shapley found faint blue stars in some galactic star clusters. He calculated that if they were ordinary stars of types B and A, then the accepted dimensions of the Galaxy would need revising. Shapley was also well aware that the globular clusters were crowded into one section of the sky, a fact so well known at the time that Eddington had even called the Sagittarius region the "home of the globular clusters". Early in 1917 Shapley reported on his research to Kapteyn.[9] He described how "the work on clusters goes on monotonously - monotonous as far as labor is concerned, but the results are a continual pleasure. Give me time enough and I shall get something out of the problem yet." Certainly Shapley was making progress for, as his investigations of the colours and magnitudes of stars in globular clusters had advanced, he had been able to secure the distances of an increasing number of clusters. In consequence, by late 1917 all the main elements for Shapley's model were present: (1) his concern for the highly asymmetrical distribution of the globular clusters across the sky; (2) values for the distances of the clusters that he believed were reasonably accurate; (3) a suspicion that the Galaxy was much larger than his contemporaries conceded, and (4) a conviction that the existing galactic models were inadequate. Sometime late in 1917 these seemingly disparate elements became meshed together in Shapley's mind and he invented a startling galactic model: the Big Galaxy. By January 1918, Shapley could write to a correspondent that "with startling suddenness and definiteness" the globular clusters had elucidated the "whole sidereal structure".[10] He now had values for all the globular clusters, and he had found that the equatorial plane of the system of globular clusters was identical with the galactic plane, and so he was now proposing that the stellar system and the huge system of globular clusters had the same centre and were co-extensive, the globular clusters actually outlining the Galaxy.

It is worth emphasizing, because present-day text-books ignore this, that the model embodied an imaginative vision, for Shapley was also proposing an evolutionary theory for the Galaxy. He hypothesized that the Galactic System "may have originated in the combination of two clusters and has grown, as it appears to be growing now, by the accretion of other stellar systems-adding the smaller units such as the globular clusters with ease, and the larger ones such as the Magellanic Clouds with some difficulty, if at all. It appears to be an example on a grand cosmic scale of survival of the fittest, that is, survival of the most massive and most stable."

How was Shapley's model received? There is no doubt that it soon met with some strong support. For example, by late 1918 Eddington was telling Shapley:[11] "I think it is not too much to say that this marks an epoch in the history of astronomy, when the

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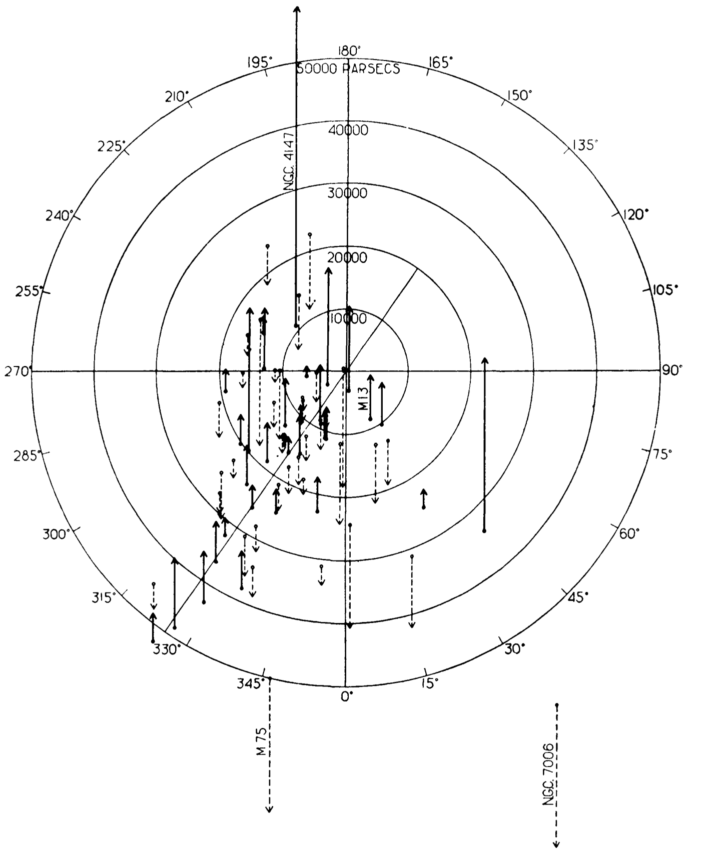

FIG. 2.—Distribution in space of globular clusters. The galactic plane is the plane of the diagram; distances above and below are shown to scale by full-line and broken-line vectors, respectively. Galactic longitudes are indicated in the margin and the scale of distances along the vertical radius. The sun is at the origin of co-ordinates. The diagram illustrates the remarkable distribution in longitude, with a maximum frequency at 325°, and by the absence of very small or zero vectors shows that globular clusters are not found within 1000 parsecs of the plane of the Milky Way. Cf. Fig. 1 of the twelfth paper.

Shapley's 1918 plot[12] of the distribution of globular clusters. Reprinted by permission of The Astrophysical Journal, published by the University of Chicago Press; copyright 1984 The American Astronomical Society.

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boundary of our knowledge of the Universe is rolled back to hundred times its former limit". But Shapley's model of the Galaxy was radical. It was also very ambitious, for he was attempting to solve many of the problems of galactic astronomy in one very broad attack. Not surprisingly, the daring set of proposals that his model embodied met with some hostility, and even his strongest supporters were critical of certain aspects of it. Walter Baade was later to describe the reception of Shapley's model:[13] "I have always admired the way in which Shapley finished the whole problem in a very short time, ending up with a picture of the Galaxy that just about smashed up all the old school's ideas about galactic dimensions. It was a very exciting time, for these distances seemed to be fantastically large, and the old boys did not take them sitting down."

Why was this so? First, Shapley's model was unexpected. There was no sense of crisis, no feeling that galactic astronomers were widely off the mark, and that something drastic needed to be done to put galactic astronomy onto a sound basis. Hence most astronomers had no inclination to demolish the contemporary notions of the size and structure of the Galaxy. Thus Shapley's model did not halt the publication of research founded on the more traditional approaches, and in the early 1920s Kapteyn wrote two papers, in part with P.J. Van Rhijn, that capped his life's work in galactic astronomy.[14] In the second Kapteyn wrote what he described as a "First attempt at a theory of the arrangement and motion of the sidereal system". Now he calculated that the limits of the Galactic System were found at roughly 1700 pc at right angles to the plane and 8500 pc along the plane. Kapteyn thereby advanced dimensions that, though small compared to Shapley's, were far larger than the estimates of the mid-1910s, and so the Kapteyn Universe, as this model became known, was itself a notable departure from the previously prevailing hypothesis.

The second reason for the sometimes hostile reception of Shapley's model was what astronomers saw as weaknesses and flaws in Shapley's distance scale. Shapley obtained his distances with the aid of three interlocking methods. Some of the larger, and apparently closer, globular clusters contained stars that Shapley identified as Cepheids. Shapley argued that he could thereby secure the distance from the period-luminosity relationship. To reach those clusters that contained no visible Cepheids, Shapley examined the thirty brightest stars in a cluster or a cluster's apparent diameter.

As so often, an argument in galactic astronomy was revolving around the accuracy of the available distance indicators, for it was to the initial Cepheid calibration that astronomers objected most strongly. If Shapley had erred at this point, then the distances he claimed for the globular clusters, upon which his model rested, would be undermined. The reality of the period-luminosity relation was far from generally accepted and it would be several years before it would be so. Hence Shapley began his distance determinations with what some saw as a very dubious step indeed, particularly as his calibration relied on the statistical analysis of only eleven stars.

One of the astronomers who disputed Shapley's model was H.D. Curtis of Lick Observatory. And of course Shapley and Curtis met in the so-called 'Great Debate' on 'The scale of the Universe'. Until recently it was assumed that the papers published after the Debate were a verbatim record of what transpired that night in 1920 at the National Academy of Sciences in Washington D.C. In fact, it seems likely that the previous accounts bear little relation to actual events. First, the debate nearly did not take place, as a number of other topics were discussed as possible subjects. At one time vivisection was considered, and the Prince of Monaco was shortlisted as a speaker on oceanography. Secondly, it seems almost certain that Shapley was apprehensive that his encounter with Curtis might hinder his chances of becoming Director of the Harvard College Observatory, a post then vacant. Shapley reasoned that if Curtis crushed his arguments he was unlikely to be offered the post, and Shapley's actions, before and during the debate, are much more intelligible when we take this belief into account. Anyway, the meeting took place on 26 April 1920. To minimise the possibility that Curtis, an experienced public speaker, would demolish his case, Shapley spoke at a non-technical level. Although members of the public were admitted to the meeting, it is hard to see what other explanation can be offered for the facts that Shapley only reached the definition of a light-year after seven pages of his script of nineteen pages, and that he devoted the last three pages to an intensifier he had developed to photograph very faint stars. The intensifier had little bearing on the theoretical argument, but probably Shapley reasoned that it would impress those members of the audience, like Mr. Agassiz of the Harvard College Observatory Visiting Committee, concerned with the future activities of the Harvard College Observatory. Curtis, who had expected a more technical presentation, was left throwing his verbal blows at a non-existent opponent. It was only in their correspondence after the debate and with their papers in a <u>Bulletin of the National Research Council</u> that Shapley and Curtis finally got to grips with each other's arguments.[15] I should stress that there was no animosity between Curtis and Shapley-far from it. Before their encounter they conducted a cordial correspondence about the meeting's procedure and their post-debate letters were also friendly, this despite Curtis's suggestion that during the meeting they should go at each other "hammer and tongs", and that he would be wielding his shillelah! Rather, Curtis's quarrel was with Shapley's distance indicators. In particular, he argued that the existence of the period-luminosity relation was very uncertain, and at this time many astronomers agreed with him. Furthermore, Curtis inclined towards a mixture of the spiral and ring theories of the Milky Way.

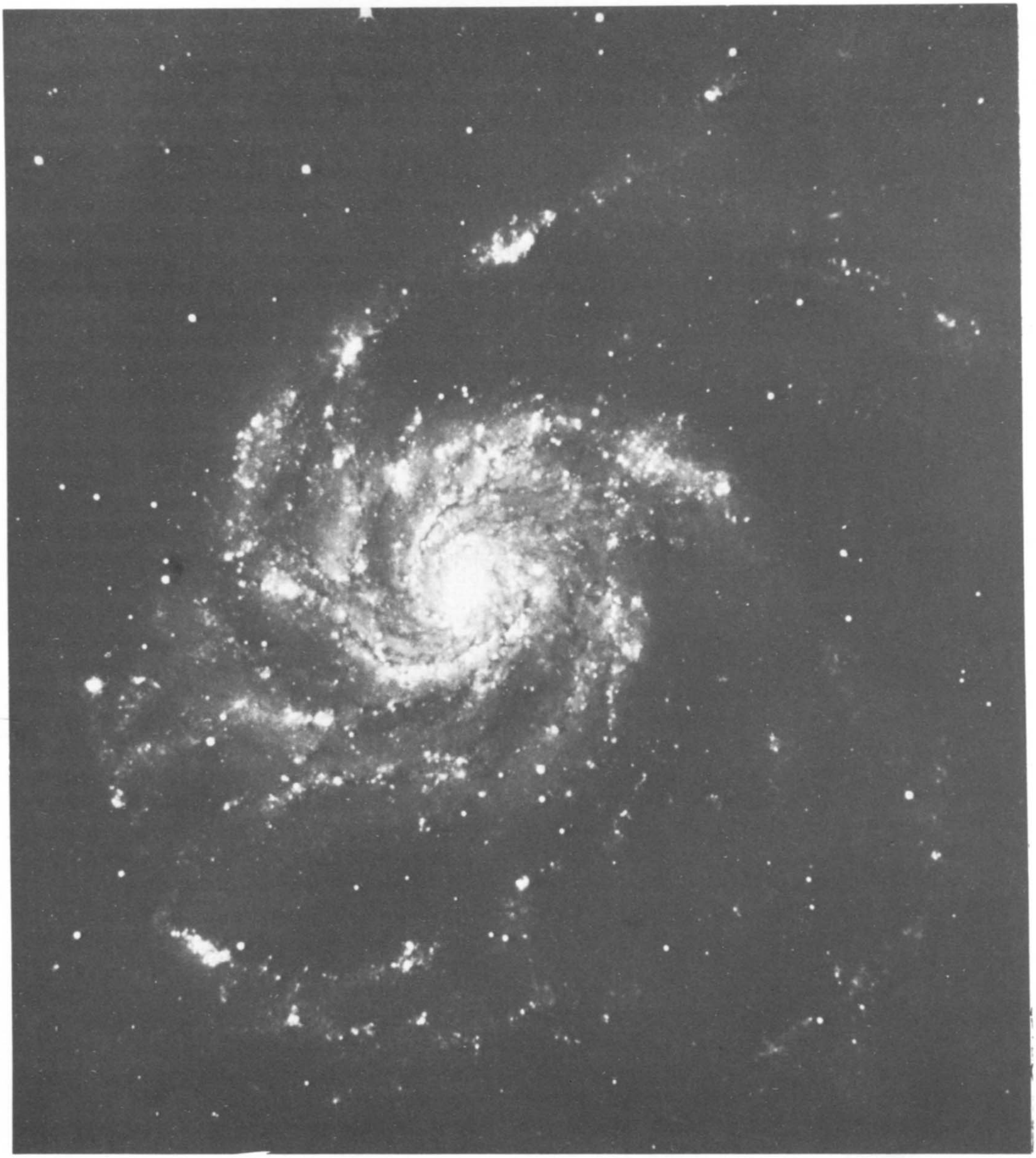
Indeed there was a strong move to the spiral theory in the 1910s and early 1920s. As mentioned earlier, this was a consequence of the revival of the island-universe theory, a revival due to a series of new observations of spiral nebulae made principally at the observatories in the West of the United States. The spiral nebulae that had for so long been seen as members of the Galaxy, were thus often viewed by the early 1920s as likely island universes. It was then extremely tempting to contend that, if observed from a great distance, our own stellar system might itself be seen as a spiral.

Yet, as the rival views of Shapley and Curtis help to show, there was no consensus in early 1920 on the size, nature, or form of the Galaxy. Nor was there any close agreement on the best ways to tackle these problems, no agreement on whether the key lay with the techniques of statistical astronomy, those employed by Shapley, or some fusion of the two.

Within a few years however, there were to be several developments that would drastically affect this situation. First, in late 1923, amidst the continuing confusion about the true nature and distance of spiral nebulae, Edwin Hubble detected a Cepheid in the Andromeda Nebula.[16] Hubble's momentous discovery set him on a course that would soon bring the long-standing debate on the existence of external galaxies to a swift end. It thus became accepted that many galaxies do have spiral shapes, and so this lent credence to the theory of our own Galaxy as a spiral. Moreover, Hubble based his investigations of the nearby galaxies on the period-luminosity relationship. By doing so, he helped it to gain acceptance. And if the period-luminosity relationship was not the spurious product of meagre data, then Shapley's distances to the globular clusters had to be taken seriously.

ROTATION

Hubble's findings were soon followed by others that supported the main structural features of Shapley's model. During 1927 and 1928 there was a rapid acceptance by many, probably the majority of, astronomers that the Galaxy rotates differentially. That the Galactic System rotates had long been suspected, and its flattened form seemed to be a natural consequence of rotation. The spectrographic measurements of the rotation of spiral nebulae in the 1910s and 1920s had further assured astronomers that the Galaxy, which many accepted was a spiral, itself rotated. But suspicion is a very long way from proof. Where did the proof come from? In 1924, Bertil Lindblad had been driven to consider a rotation of the Galaxy through his attempts to interpret star-streaming. By 1925 he had decided that the motions of the constituents of the Galaxy were explicable on the hypothesis that the Galaxy is divided into a series of sub-systems, each of which has rotational symmetry about a common rotational axis.[17] Each sub-system has the same equatorial extent, but possesses a different speed of rotation and hence a



M101, as photographed with the 60-inch reflector at Mount Wilson in March 1910 (Courtesy of Mount Wilson and Las Campanas Observatories, Carnegie Institution of Washington).

different degree of flattening. He reasoned that while high-velocity stars do not belong to the same dynamical system as those of low velocity, they must be related to the rest of the Galaxy, since their motions are symmetrical with respect to the galactic plane. Lindblad then explained the motions of the so-called high-velocity stars by arguing that the Sun and other low-velocity stars in fact have high speeds of rotation about a remote centre, and that the so-called high-velocity stars have much smaller speeds of rotation (and so form a more nearly spherical system), thereby falling behind as the Sun overtakes them. The high-velocity stars will thus appear to move asymmetrically. So, once again, a crucial development arose from looking at a set of well-known observations in a new way, this time seeing the high-velocity stars as actually slow-moving stars. Further, Lindblad (as Oort was soon to do) calculated the dynamical centre of the Galaxy to be very close in galactic longitude to the centre of the globular-cluster system as determined by Shapley.

Jan Oort was deeply influenced by Lindblad's researches, and in 1927 he announced that, through his attempt to verify directly Lindblad's theory of galactic dynamics, he had secured firm evidence of a differential rotation of the Galaxy.[18] Oort had found that the proper and the radial motions of the nearby stars exhibited the small but systematic effects to be expected of differential rotation. The genesis of Oort's analysis has recently been recalled by Bart Bok:[19] "...Jan Oort was presenting (on Monday afternoon at four) a series of seminars for Doorn, Kuiper, Oosterhoff and Bok on Lindblad's theories of galactic rotation. As I remember it - others may have different recollections!-, Jan told the four of us one Monday that he had got bogged down in Lindblad's complex mathematics and that there would be no lecture the next Monday afternoon. And, as I remember it, there were no lectures for two Monday afternoons to follow. And then there came the first Monday after the crisis, a lecture in which Jan Oort basically developed the simple formulae for the double sine-wave effect of galactic rotation in radial velocities and the corresponding formulae for the effects in proper motions. The four of us realised that we were listening to an amazing new step in the understanding and interpretation of stellar motions...." Moreover, the validity of Oort's inferences about galactic rotation was corroborated by other astronomers, particularly by the Canadian J.S. Plaskett who analyzed the radial motions of hundreds of O- and B- type stars. The detection of galactic rotation had been "in the air" and Oort had presented what was generally seen as its observational proof. In addition, Oort and Lindblad had seemingly shown that the Galaxy rotates about a point that lay in almost the same direction as Shapley's proposed direction to the galactic centre. Nevertheless, there was one point where the researches of Oort and Lindblad did not mesh with Shapley's model: the distance of the Sun from the centre of the Galaxy. Oort had reckoned that the distance to the centre about which the stars rotated was roughly 6000 pc, about one-third of the

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size of Shapley's estimate. But despite this discrepancy, the discovery of differential galactic rotation swept away the opposition to the eccentric position of the Sun within the Galaxy. Shapley and Oort's estimates were, furthermore, soon to be brought into a close agreement by the demonstration of the existence of a general interstellar absorption.

ABSORPTION

In the late 1910s Shapley had found no evidence of significant absorption in his examinations of the globular clusters, and he had proceeded to argue that, except for isolated dark clouds, space is effectively transparent. This view continued to be very influential until the implications of R.J. Trumpler's study of the open clusters within the Galaxy had been fully grasped.

As his chief working hypothesis Trumpler had taken the open clusters of similar constitution to have, on average, the same dimensions; by comparing the observed angular diameter of a cluster with the average linear diameter of the sub-class to which it belonged, he derived a value for the distance to the cluster. Trumpler had also examined the magnitudes and spectral types of the stars within the clusters. Then by constructing for a cluster the Hertzsprung-Russell diagram and comparing the observed diagram with a standard diagram, Trumpler secured another value for the cluster's distance. He thereby found that the two distance indicators gave systematically different answers: the more distant the cluster, the more the two distance values differed. Trumpler argued (and astronomers soon agreed) the the reason for this deviation was interstellar absorption. Although, with the benefit of hindsight, a number of earlier investigations can be seen to have pointed towards the existence of a general interstellar absorption, it was Trumpler's analysis of the open clusters - probably because it was more extensive and complete than earlier researches - that convinced astronomers of the presence of obscuring matter throughout the Galaxy.

In his calculation of the size of the Galactic System Shapley had not allowed for this dimming effect. As a result he had overestimated the distance of the Sun from the centre of the Galaxy. Yet the confirmation of galactic rotation and interstellar absorption was instrumental in bringing into wide acceptance the two central structural features of Shapley's Big Galaxy - the eccentric position of the Sun and the role of the globular clusters in outlining the Galaxy. But there was still no agreement on whether or not it possessed a spiral structure, or on the Galaxy's size. The resolution of the spiral problem would have to await the 1950s, as Gingerich shows elsewhere in this volume. The size problem was particularly puzzling because diameter estimates of about 100 000 light-years were now common. This meant that it was very much larger than any other galaxy. For example, Hubble argued that the mean diameter of galaxies ranged from 360 parsecs for E0's to 2500 parsecs for Sc's. This puzzle led a few astronomers to propose that the Galaxy is in truth an assemblage of galaxies. But others adopted a more sceptical attitude. One of these was Eddington. In his words,[20]

> The lesson of humility has so often been brought home to us in astronomy that we almost automatically adopt the view that our own Galaxy is not specially distinguished - not more important in the scheme of nature than the millions of other island galaxies. But astronomical observation scarcely seems to bear this out. According to the present measurements the spiral nebulae, though bearing a general resemblance to our Milky Way system, are distinctly smaller...Frankly, I do not believe it; it would be too much of a coincidence. I think that this relation of the Milky Way to the other galaxies is a subject on which more light will be thrown by further observational research, and that ultimately we shall find that there are many galaxies of a size equal to and surpassing our own.

CONCLUSIONS

This outstanding anomaly should not blind us to the fact that, towards the end of the nineteenth and in the first three decades of the twentieth century, there were great strides taken in galactic astronomy. First, the Galaxy is much larger than had been realised. Second, the Sun is eccentrically placed. Third, the globular clusters surround the Galaxy. Fourth, there is a general interstellar medium. Fifth, the Galaxy rotates differentially. And sixth, the Galaxy is not alone in space, but there are vast numbers of other galaxies. These developments arose largely from new ways in which astronomers sought to discover the size and structure of the Galaxy. And it is these tools and techniques, both observational and theoretical, that were, I think, the most important fruits of galactic astronomy between 1850 and 1930.

NOTE

For a fuller treatment of the topics dealt with here, see Robert W. Smith's The Expanding Universe: Astronomy's 'Great Debate' 1900-1931 (Cambridge, England, and New York, 1982), and the references cited therein. But see also Oort and the Universe: A sketch of Oort's Research and Person (Dordrecht, 1980), edited by H. van Woerden, W.N. Brouw, and H.C. van de Hulst.

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DISCUSSION

J.H. Oort: In connection with your remarks on the correspondence between Kapteyn and Shapley concerning the smallness of the absorption in the Galaxy, shown by the absence of change in colour of clusters with increasing distance, I should like to draw attention to other evidence in Shapley's system of clusters which in my opinion (even in

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those early years) gave convincing proof of the existence of strong absorption in the galactic plane, viz. the striking deficiency of clusters at galactic latitudes less than about 3°. Shapley himself thought that the absence of clusters close to the galactic plane was due to their being disrupted by encounters with the abundant stars in that region. But it is (and was at that time, at least to me) clear that such perturbation would be entirely insufficient to disrupt the clusters during the short time of their passage through the galactic layer.

H. van Woerden: Was this argument, that the dip in the latitude distribution of globular clusters must be due to insterstellar absorption, made in the literature?

Oort: No.

Van Woerden: About the sizes of galaxies compared to the Milky Way, I remember a lecture in 1945/46 at Leiden, where Oort pointed out that the Andromeda Nebula and especially the Galactic System were among the very biggest galaxies known. (And when I asked whether that was not peculiar, Oort blushed.) Only the changes in the extragalactic distance scale, in 1952 (Baade) and thereafter, have changed this; by 1945 we still had a Hubble constant of 500 km s^{-1} Mpc^{-1}.

J.V. Villumsen: When you go to the southern hemisphere and look up at the sky, you can see the galactic bulge clearly. How did Kapteyn explain that?

Smith: Indeed, if you look at Sagittarius, it seems obvious that you have the centre there. Kapteyn had, in his 1922 model, the Sun slightly displaced from the centre, but the direction of the centre was still towards Cygnus. I do not know precisely why. For Kapteyn, the most important evidence was the mathematical evidence rather than looking up at a brightness distribution in the sky.

F.J. Kerr: Do you know who first introduced the word "parsec", and when?

Smith: There was a great deal of debate in the 1910s and early 1920s. Kapteyn played a role in this, but I cannot remember the details. The reason for jumping between "parsec" and "lightyear" in my talk was that I was following the original writings. The matter of parsec vs. lightyear certainly had not been settled by 1920.

M. Schmidt: Is it correct that Lindblad proposed galactic rotation in order to understand the ellipsoidal distribution of peculiar velocities?

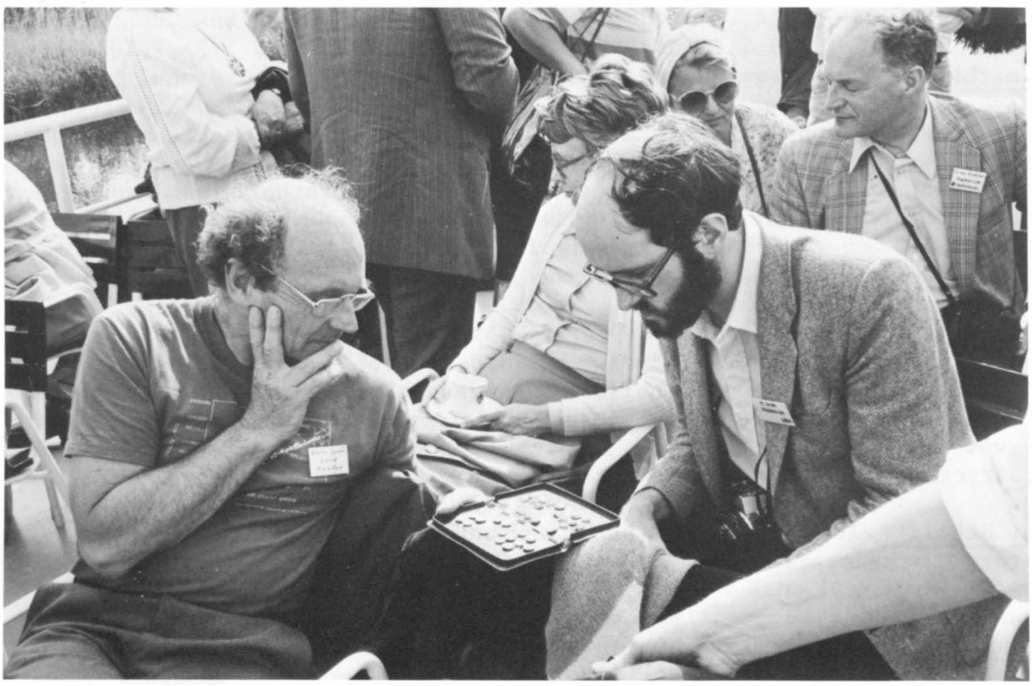
Smith: Indeed. He was trying to understand star-streaming, and this led him on to consider galactic rotation. Kapteyn had explained star-streaming by having the two streams revolve around the Galaxy in different directions.

J.H. Oort: The explanation of the ellipsoidal distribution of velocities was one of the most important developments besides galactic rotation.

A. Blaauw: I think that the Strömberg relation between asymmetric flow and velocity dispersion was an essential thing in the development of Lindblad's theory.

Smith: That is true. Similarly to what you said in your article in "Oort and the Universe", I was striding with large boots from mountain top to mountain top; given time, I should have mentioned Strömberg. He certainly pointed out the asymmetry in high-velocity stars, although during the 1920s he tied these in with a rather strange idea about velocity limitations. This was when relativity was still very new, and he thought this asymmetry might be some sort of relativistic effect, that stars would have particular limitations on their velocities; and in fact Strömberg even considered a very crude kind of velocity-distance relationship for galaxies around the same time.

M.A. Hoskin (Chairman): Before relinquishing this chair, let me express the appreciation of the historians to the Organizing Committee for the welcome we have been given, and for the opportunity to take part in this splendid week.



Robert Smith and Gösta Lyngå playing checkers during Wednesday boat trip. In background: Katrin Särg, Ria and Hugo van Woerden. LZ

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