KAPTEYN AND STATISTICAL ASTRONOMY

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1. INTRODUCTION

If Jacobus Cornelius Kapteyn (1851-1922) were here today, he would undoubtedly be among those asking the new questions on the cutting edge of contemporary astronomy. It is likely that he would even find a survey of his own contributions to the study of the Milky Way irrelevant. Nevertheless, Kapteyn's life-long interest in the Milky Way shaped the work of many astronomers, including, of course, his students, Willem de Sitter, H.A. Weersman, Pieter van Rhijn, and through van Rhijn: Jan Oort, Bart Bok and many others. But Kapteyn's influence extended far beyond his native Holland. After Kapteyn became a close colleague of George Ellery Hale and a Research Associate at the Mount Wilson facilities in 1908, Hale began to employ a number of Dutch astronomers, including van Rhijn, Adriaan van Maanen, and Kapteyn's Danish future son-in-law, Ejnar Hertzsprung. Moreover, Kapteyn's astronomical colleagues world-wide found his enthusiasm and penetrating insights infectious.

Equally significant were the important methods Kapteyn developed to investigate the complexities of the Milky Way. The techniques and models he obtained provided astronomy with tools and concepts needed by its practitioners to explore with increasing reliability the Milky Way system. Kapteyn's interests were motivated principally by his desire to solve the sidereal problem, viz., what is the arrangement of the stars in space? All of his efforts were directed to the realization of this one great, astronomical project. In this review paper, I will survey Kapteyn's contributions to the rise of statistical astronomy, principally his studies of systematic stellar motions and his analysis of the sidereal problem, with some attention devoted to his great star cataloguing efforts.

2. EARLY DEVELOPMENTS

Kapteyn's attack on the sidereal problem may be divided roughly into two periods, both of which represent a somewhat different conceptua-

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lization of the same problem. Until about 1904, Kapteyn was interested primarily in using stellar-motion data to understand the distribution of the stars in space. These efforts culminated in his dicovery of "star-streaming", first publicly announced in 1904. This discovery represented a watershed in his thinking. Not only did he confirm the existence of a preferential, dynamic stellar system, which Eddington considered at the time one of the five most important events in the history of astronomy during the past century, but afterwards he began to realize the inadequacy of using only stellar motion studies to assess the sidereal problem. Thus, after this discovery he emphasized the luminosity and density functions vis-a-vis the stellar velocity law as the key element for unraveling the arrangement of the stars. Let me not overemphasize this distinction, though, since his earlier motion work was essential to his later studies of the luminosity and density functions.

Kapteyn graduated in 1875 with a physics doctorate. Although jobs were difficult to find, he obtained a position as observer at the Leiden Observatory. Trained mostly in the rarefied atmosphere of mathematics and physics, Kapteyn immediately set about learning the practical necessities of his new profession. In his new post, his abilities were soon recognized, and early in 1878 he was elected to the newly instituted Chair of Astronomy and Theoretical Mechanics at the University of Groningen. He chose as his opening address "The Parallax of the Fixed Stars", a topic that showed he already regarded stellar distances as requisite knowledge for an understanding of the sidereal problem. Recognizing early the importance of a broader-based set of stellar data, Kapteyn hungrily searched the star catalogues. Even the great Durchmusterung catalogues of Argelander and Schoenfeld were limited, not only to magnitudes and position, but equally they lacked the important stellar motion data.

Thus, it is fitting that the International Astronomical Union, the founding of which Kapteyn ironically did not initially endorse, celebrate the 100th anniversary of the founding of Kapteyn's Astronomical Laboratory. For it is the concept of Kapteyn's Laboratory that perhaps best represents the kernel of Kapteyn's success. Kapteyn recognized early that he possessed neither the financial resources nor the proper geographical climate to undertake successful observational studies. Hence, if he were to explore the nature of the Milky Way system, it would be necessary to possess massive amounts of the right kind of data. Thus, as he set his mind on finding the solution to the sidereal problem, he recognized the necessity of good, reliable data. Coincidentally, it was during this period that Sir David Gill, then the leader in practical astronomy and the director of the observatory at the Cape of Good Hope, was attempting to fill the hiatus created by Schoenfeld's delimitation of the southern Bonner Durchmusterung to -22 degrees declination. Recognizing the importance of Gill's work, in 1886 Kapteyn offered without solicitation his aid to Gill for measuring the many photographic plates and cataloguing the numerous stars. It took nearly thirteen years of Kapteyn's constant attention before the Cape Photographic Durchmusterung was finally published between 1896 and 1900.

Thus began Kapteyn's widely publicized efforts at international co-operation in astronomical research. By the time the Cape project had gone to press, Kapteyn's laboratory had become institutionalized, and christened the "Astronomical Laboratory at Groningen". After his Plan of Selected Areas received international sanction, Kapteyn's astronomical laboratory continued to receive a flood of data for reduction and analysis. Thus, his American colleague, Frederick H. Seares (1922), later wrote that "Kapteyn presented the unique figure of an astronomer without a telescope. More accurately, all the telescopes of the world were his".

Equipped now with extensive experience with the data of his profession, during the 1890's Kapteyn published a series of papers on the nature of stellar motion, all with the idea of eventually solving the sidereal problem. Ever since William Herschel in the eighteenth century enunciated his project, the "Construction of the Heavens", an understanding of the arrangement of the stars in space had been a major problem. A central theoretical concern dealt with the kind of data that could be used accurately to measure stellar distances. Herschel had suggested stellar brightnesses, even though he was aware by 1817 of the contradiction implied in unequally luminous members of binary star systems. But many others throughout the nineteenth century expressed similar concerns, and sought for an alternative measure of stellar distances.

Beginning with Edmond Halley's discovery in 1718 of the motion of stars and Herschel's investigations of preferential stellar motions toward the solar apex, astronomers mostly during the latter half of the nineteenth century began to use stellar-motion data as a means to represent distances to the stars. Using Gauss' least-squares technique, astronomers, such as Friedrich Argelander and George Airy, began to distinguish random fluctuations in stellar motions. Eventually they concluded that random irregularities were due, not to any systematic errors of observations, but rather to the peculiar motions of the stars. Thus, they among others recognized the preferential nature of proper-motion data, and emphasized the need to reduce the data to peculiar motions. During the last half of the century there was wide-spread use of the assumption of random motions among the real motions of stars.

This assumption was coupled with the belief that proper-motion data, to be useful as a measure of stellar distances, must be correlated to a definite stellar yardstick. Following Bessel's discovery of stellar parallax in 1838, and after enough parallaxes had become available, many astronomers argued that a star's distance is inversely proportional to its proper motion. The determination of stellar distances using parallactic techniques would thus be greatly enhanced, since proper-motion data were available relatively abundantly. For each known parallax there were, of course, scores of measured proper motions. The publication in 1888 of the Auwers-Bradley Catalogue containing 3,200 reliable proper motions stimulated these efforts. Still, prior to Kapteyn's ground-breaking studies of stellar motions during the 1890's, no one had succeeded convincingly in relating a distance measure based

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on a few thousand proper motions to the demands of the large survey catalogues, such as the Bonner Durchmusterung, containing hundreds of thousands of stars. A statistically precise relationship between proper motions, parallaxes, and apparent magnitudes was complex, to say the least.

Although Kapteyn preferred the actual distances derived from parallaxes, he recognized, even with improved photographic techniques, that the scope of the sidereal problem demanded a much broader base than that allowed by the earth's orbit. Thus his emphasis on stellar motions was motivated by both practical and theoretical considerations: that by using proper motions understood correctly the base of parallaxes could be extended on the ever-increasing base line of the sun's motion through space, and that for an understanding of the structure of the stellar system knowledge of only the mean distances of groups of stars, rather than the absolute distances of individual stars, was necessary. To complicate matters further, Kapteyn and the Irish astronomer W.H.S. Monck discovered independently in 1892 that there was a direct relationship between proper motion and spectral type.

Within this context, Kapteyn achieved a major synthesis, both in method and conceptually, when he derived the "mean parallax relationship", a statistical law that formed the basis of nearly all his attempts to understand the arrangement of the stars in space. Examining proper motions in the Auwers-Bradley Catalogue, Kapteyn correlated known parallaxes with proper motions and magnitudes, and generalized this correlation among large numbers of stars to form a "mean" distance relationship. Published in 1901, the "mean parallax" relationship not only culminated years of close analysis of stellar motion data, but it also led directly to Kapteyn's luminosity function, which he first published a few months later. In terms of the achievements of classical statistical astronomy, the "mean parallax" formula has been over-shadowed only by the importance of the luminosity and density laws, and the "fundamental equation of stellar statistics".

3. STAR-STREAMING

Also within this context Kapteyn made his great "star-streaming" discovery. During the 1890's, Kapteyn's investigations were predicated on the supposition that stellar motions were the key element in understanding the distribution of the stars. These developments were codified in the so-called velocity law, a relationship, Kapteyn argued, that not only would provide an understanding of the stellar system, but that also would lead to the derivation of the density and luminosity laws. In turn the latter would yield a detailed understanding of the Milky Way system.

About 1895, Kapteyn developed a mathematico-statistical theory that related star-counts, the density function, and a Gaussian probability function of proper motions. In his theory he assumed the traditional view that stellar motions are randomly distributed. Co-authored with his mathematician brother, Willem, the complete discussion of his theory was published in 1900 as No. 5 in the famous series of Groningen Publications. In the introduction he succinctly stated their purpose:

> In what follows, an attempt will be made to deduce from the observations, what, for the sake of brevity, I will call the law of velocities, i.e., the law by which is defined the number of stars having a linear velocity equal to, double, triple, ..., half, a third, ... that of the solar system in space, or shorter: the law by which the frequency of a linear velocity is given as a function of its magnitude. The fundamental hypothesis on which this derivation rests is the following: ... The real motions of the stars are equally frequent in all directions.

The observational evidence supporting the theory was earmarked for No. 6 of the Groningen series. Though it represented the most up-to-date views of the velocity law, the theory turned out to be so wide off the mark that not even a comparison with the observational evidence could be made. The reason for the discrepancy between theory and observation was the invalidity of the fundamental hypothesis of random motions. Although most nineteenth-century astronomers considered this hypothesis as a priori valid, in 1895 Hermann Kobold showed that a random distribution did not represent the observed motions of nearby stars in the Auwers-Bradley Catalogue; soon Kapteyn was to provide an explanation of this startling fact in terms of preferential motions. The anomaly between theory and evidence represented a critical problem for Kapteyn's program, because such a basic discrepancy affected one of his stated aims: the derivation of the density and luminosity laws from the allegedly more fundamental velocity relationship.

When it became clear that the evidence needed to support the theory was not forthcoming, he deduced several hypotheses to explain the alleged discrepancy: (1) preferential stellar motions; (2) incorrect apex value; and (3) incorrect proper motion values. Although he showed theoretically that the last two explanations could account for the failure of his theory, he concluded that both the apex value and proper motions had been calculated correctly.

Of all the numbers of the Groningen Publications, a series forming, in the words of Eddington, "one of the most often consulted works in an astronomical library, No. 6--the one which has never been written--[is] perhaps the most famous of them all...." How could an unwritten document be so significant? Precisely because Kapteyn's failure to harmonize observation with theory reaffirmed the anomalous nature of stellar motions, and put him onto the track that culminated in his discovery of the two star-streams.

Despite the importance of theory in directing Kapteyn's research program, he also claimed to be an inductivist in his scientific methodology. "My studies", wrote Kapteyn to George Ellery Hale in 1915, "have made of me more and more of a statistician and for statistics we must have great masses of data". Kapteyn's procedure was to combine both deductive and inductive approaches. Commenting on the importance of an inductive approach in his letter to Hale, Kapteyn illustrated his point regarding the star-streaming discovery:

> Deduction sets in too soon and too much is still expected from it. To illustrate what I mean take the star-streams as an example. ... Schoenfeld was led, I think by analogy, to consider the question: May there not be a rotating motion of the Milky Way as a whole? He made the necessary computations, but found practically nothing. Other men tried a rotating motion of all the stars in orbit in the Milky Way, not necessarily all with the same period. Some, I believe, tried to adhere to a common direction of motion .... Now all this seems to me too much deductive. We began by making a wild guess, deduce its consequences and see whether it agrees with the observations. How long might we have guessed before we ... came to put the question: Are there two star streams? I blundered along for a long time in the same mistaken way, till one day I swore to go along as inductively as I could. I made drawings showing at a glance the observed data for each point of the sky. There showed very decided deviations from what was to be expected according to existing theory [i.e., random motions]. Considering these deviations as perturbations I tried to isolate these perturbations: I superimposed all the drawings belonging to Zones in which, according to existing theory, there ought to be equiformity and took averages. The result was a figure pretty well in conformity with existing theory. This drawing I then took to represent the undisturbed form and subtraction [of the solar motion] from the individual figures then gave the isolated perturbations. There showed at once a great regularity, which regularity was almost at once seen to consist in a convergence of the lines of symmetry to a single point of the sphere. From this to the recognition of two star streams. Thus the inductive process led in a very short time to a result which others, myself included, had tried in vain to bring out in a more deductive way, for ever so long.

Within a short time after publishing his velocity theory in 1900, Kapteyn rejected his theory and by 1902 had disovered star-streaming. Finding that the stars tended to move in two distinct and diametrically opposite directions, Kapteyn suggested that this phenomenon resulted from two once distinct but now intermingled populations of stars moving relative to one another.

Kapteyn first announced his new theory of stellar motions before the St. Louis World Exhibition in 1904, and again more importantly before the 1905 meeting of the British Association for the Advancement of Science. In both cases Kapteyn argued that without exception all the stars belong to one of the two streams. The over-riding consideration, in Kapteyn's opinion, was not a reevaluation of the reality of the phenomenon, but the necessity to confirm the theory, that is, that there exist two independent streams of stars passing through one another in opposite directions with different mean motions relative to the sun. In this regard, he suggested to his BAAS colleagues that radial-velocity observations might prove to be the most convenient data to test the theory:

> I suspect that the materials for a crucial test of the whole theory by means of these radial velocities are even now on hand in the ledgers of American astronomers--alas not yet in published form. It is this fact which long restrained me from publishing anything about these systematic motions, which, in the main, have been known to me for three years [since 1902].

He had in mind the Lick Observatory people and particularly W.W. Campbell, who had been doing radial-velocity work since about 1900 and therefore possessed the data needed to prove Kapteyn's hypothesis conclusively.

After Kapteyn's discovery, the number of theories and studies of preferential stellar motions increased quickly. Kapteyn, himself, participated relatively little in these developments, since, in the main, studies of the velocity law and star-streaming could not, in Kapteyn's view c. 1905, add directly to a detailed understanding of the arrangement of the stars in space. He remained keenly aware of the newer work, however, particularly the explanations of star-streaming by Schwarzschild and Eddington, returning to these ideas only in his last major paper in 1922.

4. DISTRIBUTION OF STARS

With the failure to derive the velocity law and the subsequent discovery of star-streaming, Kapteyn increasingly turned his attention to a derivation of the luminosity and density laws as conceptual tools to understand the form and structure of the stellar system. This approach was also based on his earlier work on the mean-parallax relationship. Thus, even though his efforts at detailing the nature of the Milky Way system were frustrated by his failed velocity-law studies, he continued to use both the basic data and results to understand the larger system.

In 1898, Kapteyn's contemporary Hugo von Seeliger derived the "fundamental equation of stellar statistics". From 1898 to 1920, Seeliger presented his results utilizing the "fundamental equation". In most of these studies, however, Seeliger had assumed an arbitrary probability function for the luminosity law. In addition to various solutions of the sidereal problem, Seeliger was interested in developing the mathematics that would allow for an analytic solution. On the other hand, while recognizing the importance of the form of this relationship in relating the star counts, the density law, and the luminosity law, Kapteyn realized that an accurate representation of the arrangement of the stars in space would require a precise understanding of the luminosity relationship.

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After deriving the mean-parallax relationship, Kapteyn developed a numerical technique for deriving the luminosity function that would allow one to relate the magnitudes of stars to their motions, and hence their mean distances. Briefly his method entailed placing the catalogued stars in cells corresponding to their apparent magnitude and probable proper motion. The limiting characteristics of the cells corresponded to spherical concentric shells about the sun. Utilizing his "mean parallax" formula, which expressed a dependency between calculated parallaxes, on the one hand, and apparent magnitudes and proper motions, on the other, Kapteyn calculated the mean parallaxes corresponding to each cell. The results, of course, represented only mean parallaxes. In actuality the stars were distributed according to the laws of probability defined by some Gaussian function. The exact shape of the Gaussian curve was derived from a determination of the spread of 58 stars, with precisely known proper motions, apparent magnitudes, and measured parallaxes. Using this computed probability distribution, Kapteyn calculated the spread of parallaxes for the stars within each cell. This resulted in a two-dimensional table in which the catalogued stars were distributed by magnitude class and mean distances. The magnitudes were normalized by conversion to their absolute magnitude using the magnitude-distance relationship.

This technique was first published in 1901 in his classic paper "On the Luminosity of the Fixed Stars". Others, notably Gylden, Schiaparelli, and particularly Seeliger, had noted the importance of using a Gaussian function to represent the luminosities. But Kapteyn alone succeeded in actually deriving such a function. Thus Kapteyn introduced the term "luminosity-curve" into astronomical parlance as "the curve which for every absolute magnitude gives the number of stars per unit of volume". Since his luminosity table expressed distances from the sun to the stars of various magnitudes, it was a simple matter of dividing the numbers of stars within each shell by its volume to calculate the relative density. Thus Kapteyn's procedure also yielded the relative density distribution of stars in the local solar neighborhood. Since stars limited only to magnitude 9.5 were used, the density relationship was tentative at best.

During most of the two decades preceding Shapley's work and the emergence of the "new" astronomy of the 1920's, statistical astronomers generally believed that the density and luminosity laws would be sufficient to explain the arrangement of the stars in space. In addition to the fundamental equations of stellar statistics, Kapteyn's "mean-parallax" formula and his luminosity-curve provided the basic coneptual tools needed in this work. When Kapteyn's luminosity paper appeared in 1901, the possibilities inherent in a rigorous approach to statistical investigations were greatly changed. This was more the beginning, however, than the end. Kapteyn, himself, considered his 1901 paper as providing only a first approximation to the sidereal solution, a problem which, in his opinion, "must be solved by successive approximations".

As a "first attempt" to solve the sidereal problem, however, Kapteyn's research on the general luminosity function made several critical assumptions, that in following years defined key problem areas for statistical astronomy. His 1901 results assumed: (1) negligible light absorption; (2) a sun-centered stellar system; (3) a luminosity-curve uniform throughout the entire stellar system; (4) a luminosity-curve distributed according to Secchi's type I and II stellar spectra; (5) a density relationship independent of galactic longitude and latitude; and (6) true parallaxes of stars distributed about their mean in a Gaussian symmetric form. Let me briefly treat each of these assumptions.

The question of the transparency of space had been discussed by many nineteenth-century astronomers, including William Herschel, F.G.W. Struve, Olbers, and Kapteyn's contemporary Seeliger. Kapteyn recognized that the existence of an interstellar absorbing medium could seriously alter the form of both the luminosity and density functions and thus fundamentally change the parameters describing his stellar system. Although space does not permit detailed analysis of his work on the absorption problem, Kapteyn in 1904 found little evidence for absorption. By 1909, to rationalize away a sun-centered cosmology, he suggested a value of 0.3 magnitudes per kilo-parsec. By 1915, the work of Shapley showed that Kapteyn's value, however, "must be from ten to a hundred times too large ... and the absorption in our immediate region of the stellar system must be entirely negligible." Moreover, Walter S. Adams, also on the Mount Wilson staff, had results that seemed to show hydrogen absorption does not occur in space, but that the stars themselves are responsible for changes in stellar intensity.

Lacking a viable alternative, statistical astronomers had generally assumed that the solar system was centrally located in the universe. To be sure, the ad-hoc nature of this assumption made many feel uneasy; yet, as a workable hypothesis, it was the only really defensible position. As indicated, by 1915 studies tended to confirm the lack of an absorbing medium, which supported a maximum density-function value in the solar neighborhood. Kapteyn's star-streaming work also gave credence to this view, for, as he expressed to Hale in 1915, "... the stream velocity increases with decreasing distance from the sun. The result seems to me to be well established. One of the somewhat startling consequences is, that we have to admit that our solar system must be in or near the centre of the universe, or at least to some local centre."

Kapteyn's third assumption, the uniformity of the luminosity-curve, was theoretically independent of the question of interstellar absorption. It had been derived for the local solar neighborhood where, it was argued, absorption (even if present) was essentially negligible. Thus Kapteyn adopted the view that the luminosity-curve is the same for different distances from the sun. Hence, regardless of galactic position or distance from the sun, all regions of space exhibit the same distribution of luminosities within the same unit volume. Since the derivation of his luminosity function required empirical knowledge of parallaxes and proper motions, it could only be determined for the local region of space.

Kapteyn had noted the importance of spectral type in his 1901 studies of the stellar system. But with the revolutionizing developments in spectral classification early in the century, Kapteyn continually emphasized the importance of close spectral studies. Writing to Adams in 1912, Kapteyn noted: "In my mind, the most important problem in sidereal astronomy would be: the study of the arrangement of stars in space (including star streams) separately for stars of different spectral type." Thus, in writing to Hale a few years later in 1916, Kapteyn again noted: "... we can find the distributions in space of nearly all the Helium stars [, and] ... that there is a gradual transition in every direction from the Helium stars to the other types.... All this finished I will have to come to the A stars, which in the main I find to behave like the Helium stars. If I finish them too I think I may hope to solve the many riddles that remain for the rest."

Though the Herschels, Struve, and others had noted the dependency of the number of stars on galactic latitude, it was Seeliger who, in the 1880's, first rigorously demonstrated this fact. In later studies Kapteyn recognized Seeliger's work on this point, and noted that although the luminosity-curve is independent of galactic latitude, since it was not an absolute measure, but a distribution function, the density law is a function of latitude. A dependent relationship between density and galactic longitude was not rigorously confirmed until 1917, and thus was generally not taken into account in these early studies.

Finally, Kapteyn continued to assert throughout his investigations the reliability of the dispersion of the measured parallaxes about their mean. In 1920 he wrote in his classic paper on the so-called "Kapteyn Universe": "It has been shown in G.P. 11 [1901] that widely differing assumptions as to the dispersion law lead to results that differ but little .... Therefore, we have not deemed it necessary to derive this law anew, but have adopted the one found and tabulated in G.P. 8 [1901]." This was an important point since the dispersion determines the parameters of the mean-parallax formula.

Between 1904 and 1906, Kapteyn had proposed his Plan of Selected Areas as a coordinated international effort to collect the kind of data needed to resolve these assumptions and verify his star-streaming hypothesis. Although his Plan was not completed until 1920, Kapteyn continued to examine these problems closely in a whole series of important research papers.

5. KAPTEYN'S UNIVERSE

By 1914, the basic concerns of statistical astronomy had been further refined, and focused on the following problems: the relationship between spectral type and stellar distribution; the relation between mean-parallax and factors as proper motions and apparent magnitudes; the nature of the velocity law and star-streaming in general; the analytical form of the star-count function, including particularly its maximum magnitude value; the relation between stellar distribution and galactic latitude and longitude; the mathematical form of the luminosity function, and the absolute magnitude at which it obtains a maximum; and the analytic form of the density law.

After 1914, Kapteyn and others continued vigorously both to refine the empirical support needed to quantify these questions and to investigate the mathematico-statistical basis of the sidereal problem. Provisional answers to these questions came together conceptually in two classic papers Kapteyn published in 1920 and 1922, the year of Kapteyn's death. Briefly, the 1920 paper, co-authored with van Rhijn, described a transparent, ellipsoidal stellar system in which star density at low galactic latitudes diminishes in all directions with increasing distance from the sun. Star-density at 600 parsecs was about 60 percent of that near the sun; at 1,600 parsecs about 20 percent; at 4,000 parsecs only 5 percent; and at its perimeter, about 9,000 parsecs from the sun, star-density was less than 1 percent of that in the solar region. At high galactic latitudes, Kapteyn's results were closer to the actual state of things. Although his 1920 system was nearly sun-centered, he was not, despite increasingly stronger evidence from others' research, willing to relinquish this assertion easily.

Two years later, in 1922, Kapteyn developed a dynamical theory of the stellar system, in which he attempted to explain stellar distributions and motions in terms of gravitational forces. The force exerted by the Milky Way system was calculated at various distances perpendicular to its plane. From this calculation an estimate of the total mass density per volume was derived for the vicinity of the sun. Within the plane of the stellar system, he assumed a general rotation about the polar axis with the two star-streams accounting for the motion. Centrifugal forces plus random motions were balanced by the gravitational field.

In actuality, the 1920 model represented Kapteyn's lifetime achievements dealing with the sidereal problem; the 1922 theory was a provisional attempt to relate Kapteyn's (1920) model of the distribution of the stars with his earlier discovery of star-streaming. Taken together, Kapteyn's 1920/22 theory of the stellar system came to be known as the "Kapteyn Universe".

6. CONCLUSION

Obviously, Kapteyn was not solely responsible for the emergence of statistical astronomy prior to the "new" astronomy of the 1920's. But Kapteyn, and to a lesser degree Seeliger, continued as leaders in what promised to become an extremely fruitful research endeavor. With the beginning of their statistical studies in the 1890's until the early 1920's, Kapteyn defined, clarified, and devised many of the major research problems dominating statistical astronomy.

Not only problems of substance were explored, but new methods were developed all of which provided grist for the mills of many statistical astronomers. In these endeavors, the role of statistical theory increasingly came into prominence. In a sense these scientists considered themselves as some sort of latter-day Kepler. Just as they thought Kepler had derived empirically three planetary laws, so too the statistical astronomers believed they were seeking stellar laws, as true of the galactic system as Kepler's are about the planetary system. Furthermore, they were utterly convinced that an understanding of these relationships would yield universal laws of nature, not just statistical relationships. After the derivation of the luminosity function in 1920, Kapteyn expressed it this way: "It is difficult to avoid the conclusion that we have here to do with a law of nature, a law which plays a dominant part in the most diverse natural phenomena."

Not only was Kapteyn a supremely gifted scientist, but he managed, as nearly all leaders do, to stimulate international cooperation. We have already noted Kapteyn's involvement with Gill's Cape Photographic Durchmusterung. His Plan of Selected Areas was perhaps the first truly multi-national astronomical effort. His long association with numerous astronomers, and particularly with the Mount Wilson Observatory, only further high-lights Kapteyn's abilities to achieve great success with limited resources. Perhaps the clearest indication of Kapteyn's success was noted, shortly after Kapteyn received the Bruce Medal in 1913, when George Ellery Hale, in reference to Kapteyn's stellar studies, wrote: "You must not suppose for a moment that there was any mistake made in awarding you the Bruce Medal. In my opinion, no astronomical work of the past generation has been more significant or important than your own, and it is a compliment to the other men who have received the Medal to claim them with you."

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DISCUSSION

The Chairman, M.A. Hoskin: : Dr. Clube has prepared a Discussion Contribution.

S.V.M. Clube: History has been less than fair to Kapteyn. Thus, it is well known that his analysis of proper motions led to the discovery of the two star streams and that he then embarked on a major programme to delineate the so-called Kapteyn Universe. His discovery was followed, however, by Schwarzschild's suggestion that the proper-motion data could be equally well represented by a velocity ellipsoid. This proposal was enthusiastically endorsed by Eddington (1914), but any mathematical convenience arising from the idea that a single population of stars was experiencing forces that perturbed them along a preferred axis (now recognized as the galactic centre-anticentre line), was not originally seen as denying the physical reality of Kapteyn's two streams - one of which (Stream I) moved relative to the other (Stream II) at around 35 km s^{-1} away from what is now recognized as the galactic centre direction. As it turned out, however, Shapley's discoveries around 1918 led to the sensational collapse of the Kapteyn Universe, and it now looks as though the ensuing loss of confidence in Kapteyn's programme of research led also to an (irrational) decline in interest in the two streams. Whatever the exact sequence of events, the velocity ellipsoid as represented by Stream I alone (generally an intrinsically brighter and younger population) soon graduated to become a primary observational base for Lindblad's and Oort's development of galactic rotation theory, whilst Stream II (generally a fainter and older population) was relegated to the historical dustbin. This is unfortunate, since modern surveys of nearby stars tend to demonstrate the validity of Kapteyn's division. Stream II for example has virtually no representatives among the local young population: see the space-motion studies of A stars by Eggen (1963) and Me dwarfs by Upgren (1976). These latter, comprising Stream I, have motions in the mean like that of the nearby gas and thus approximate closely to the currently adopted local standard of rest. The older and more widely dispersed M dwarfs on the other hand are representative of Stream II (cf. Clube 1978; also these proceedings), and such stars with well-mixed orbits evidently define a physically preferable but entirely different l.s.r. By continuing to overlook Stream II, we not only unbalance our understanding of galactic dynamics but deny Kapteyn's discovery its proper place in history.

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The Chairman: Are there any comments to Dr. Clube's contribution?

H. van Woerden: Is it not true that the average motion of older, well-mixed stars is influenced by the radial density gradient in the Galaxy?

Clube: You are referring to the so-called Stromberg drift. The relative motion of Kapteyn's star-streams is orthogonal to that. All I am suggesting at the moment is that we have a clash of information given by the nearby young stars and the nearby old stars. And I would have thought it is more reasonable to be suspicious of the young stars rather than the old ones.

A. Blaauw: If there is a problem about these streams that both are nearby, should one not look at larger distances, where local effects are more smeared out and one has a better overall view? If I remember well, the star streaming or ellipsoidal distribution is shown just as well by the faint stars as by the bright ones, or even better.

Clube: I agree with your comment. I believe also that we ought to look at the behaviour of the more distant stars a great deal more carefully than has been done. Tomorrow I will describe some more recent observations that I think lead one to suspect that we may have misunderstood the more distant material as well as the nearby stars.

The Chairman: We shall now discuss Dr. Paul's paper.

H. C. van de Hulst: The word "sidereal problem", which you used many times, is not common in modern literature. Is it derived from the older literature, or is it a word you coined yourself?

Paul: The phrase "sidereal problem" appears about 1900. Kapteyn may have used it first in his 1904 survey paper published in Science. He there states that he considers the arrangement of the stars in space the central problem in astronomy, and calls it the sidereal problem. He does not indicate who coined the term, and I have been unable to find that out, but it is very prominent in the English-speaking literature.

M. Schmidt: As you indicated, Kapteyn adopted a gaussian distribution of the logarithm of the parallax around the mean value for given magnitude, proper motion, etc. Do you know whether this gaussian distribution was based on observations, or was it an assumption?

Paul: Kapteyn derives the distribution with a complex, numerical technique from the raw data and it drops out in a Gaussian form.

Schmidt: I wonder whether any of those present here that are senior to me knows the answer to this question?

Clube: I hesitate to claim to be more senior, but I would point out that this matter is discussed at length by Eddington in his 1914 book on "Stellar Movements and the Structure of the Universe". It is very interesting that in the end he clearly sits on the fence, and would not choose between the two-stream hypothesis - which would amount to some non-Gaussian distribution - and the Schwarzschild ellipsoidal distribution; that is, he regarded the issue as unresolved by the observations in 1914.

Schmidt: However, I think that the ellipsoidal distribution of velocities is not identical at all to the question that I am asking about the assumption of a gaussian distribution of log π around its mean value.

Clube: It is clear that you needed somebody more senior than you yourself.

Hoskin invites further discussion, after Clube has answered Schmidt.
CFD

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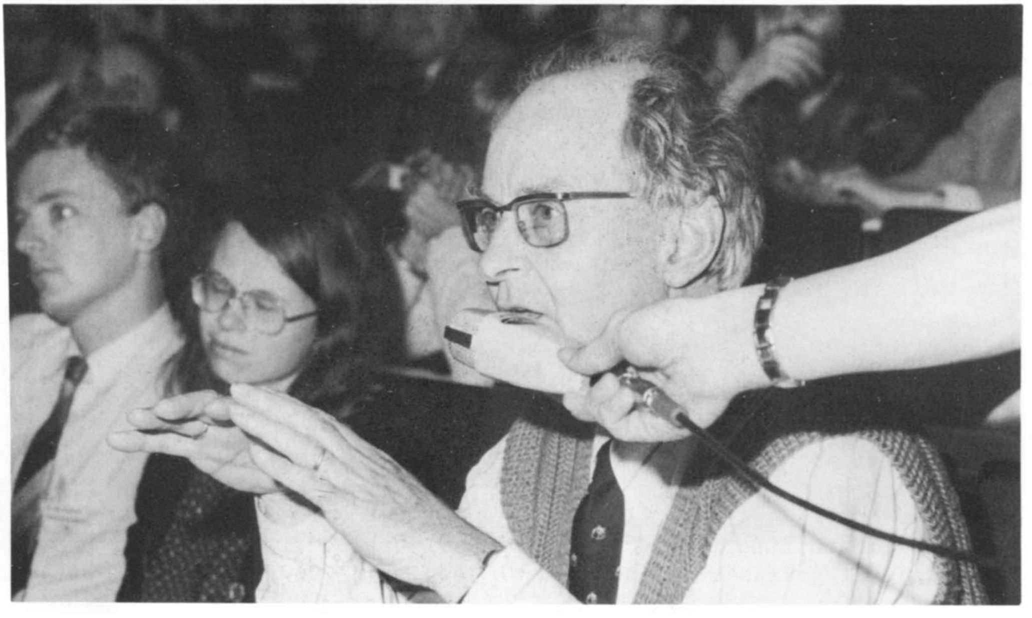
R.H. Sanders: What in your opinion was the essential observation that overthrew the Kapteyn Universe?

Paul: I think you are all familiar with the debate about the globular clusters. One of the key elements was that Shapley had determined that the short-period Cepheids were giant stars. Kapteyn and Van Rhijn had assumed that they were dwarfs. A lot of the argument hinges on that particular point. If they are dwarfs, they are much closer - in fact, Kapteyn argues that they should be 8-10 times closer to the Sun rather than in the expanded system that Shapley argues for. There are other elements in the issue.

A. Blaauw: I always thought that the crucial point had been the influence of interstellar absorption. Was not the main point at that time, whether its influence was so large that one should discard the Kapteyn density distribution in the plane? I know that Kapteyn himself looked into this question very carefully; and in fact Van Rhijn's thesis (1915) investigated the possible effects of interstellar absorption by checking for reddening, and the answer was negative.

Paul: Certainly the absorption question was a major thing. Kapteyn vacillates somewhat on this. He has a 1904 paper responding to the American astronomer Comstock, in which he concludes that there is essentially no absorption. In 1909 he comes up with some absorption. In 1915 he is back down to no absorption essentially. The tradeoff is



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that, if there is no absorption and the luminosity function is derived for the solar neighbourhood, the parameters defining the system are relatively small (about 9000 pc diameter), but the problem is that you have to accept the central location of the Sun. Alternatively, if one allows absorption - and indeed Kapteyn asks: if we assume a certain amount of absorption, what will the system look like? - then for large distances the density function increases enormously. So the tradeoff for absorption is that you get a very extended system, and Kapteyn finds that an unacceptable hypothesis.

R.G. Carlberg: Did Kapteyn have any opinion on the nebulae, the external galaxies?

Paul: People who assumed the small galactic system, in general asserted the Island Universe theory. Seeliger does argue that the spiral nebulae are island universes. I believe Kapteyn does as well. Robert, do you know?

R.W. Smith: He changes. At the start of the century he goes along with the old notion that there are no visible external galaxies. Later on I think he makes the spirals external galaxies.

B.F. Burke: Shapley's model of the Galaxy was well-known by the time Kapteyn's final papers on the "Kapteyn Universe" were published. What was Kapteyn's opinion of Shapley's model?

Paul, flanked by Clube and Hoskin, invites Smith to comment. CFD

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Paul: The so-called Kapteyn Universe was published in 1920. The Universe presented in 1922 is substantially the same, with the Sun moved off about 650 pc from the centre, but its shape is almost identical. The 1920 Universe itself is not much different from earlier models that Kapteyn had presented in 1908 and 1914. So Kapteyn's model was already very well known when Shapley published his work. It appears that Kapteyn did not have much regard for Shapley's work, partly on the grounds of the luminosity of the Cepheids.

O. Gingerich: Most of us think of the Shapley model as it was given by H.S. Plaskett in his Halley lecture of 1935. However, at the time when Shapley presented his model, he linked together a large number of star clouds into a Galaxy that was quite different from our concept of today. So his model was not so very incompatible with Kapteyn's. He accepted Kapteyn's Universe as being the local star cloud that was swallowed up in his much grander system. Hence the problems of absorption and scale did not immediately enter the argument at that time. The two men were talking on two relatively different grounds, and in their meetings at Mount Wilson Kapteyn always seemed relatively uninterested in what Shapley was doing -- at least that is what Shapley told me several times.

Paul: In response to that: Shapley went to the IAU meeting in Rome in 1922, and on his return from Rome he stopped in Groningen or Leiden to speak with the Dutch face to face about this particular issue. In the private correspondence there is evidence of mutual respect, but there was a serious theoretical disagreement here at that time. Seeliger's model is in some respects very similar to that of Kapteyn; and one of Seeliger's students, Hans Kienle, becomes a very early advocate of Shapley's model. In 1924 Kienle edits a Festschrift for Seeliger, and in that book he publishes a paper by Shapley on the globular clusters. So we already see the next generation of people, including Van Rhijn, beginning to recognize that the Kapteyn- or the Seeliger Universe refers simply to the local solar neighbourhood.

J.H. Oort: About the attitude of Van Rhijn and Kapteyn to Shapley's work: if I remember correctly, it was mainly - in the beginning at least - a doubt about the distance scale, whether Shapley had got the distances or the absolute magnitudes of the globular clusters correctly. There was some reason to feel uncertain about that. The other point concerns the absorption. The reasons why Kapteyn and Van Rhijn decided to neglect the absorption was, in a way, a very sound one: they used Shapley's data in fairly high galactic latitudes to indicate that the absorption per kpc was negligibly small. This was correct in all latitudes above 10 or 15 degrees, and so one can say that the Kapteyn system was essentially correct for all directions that were not exactly in the plane of the Galaxy. But Kapteyn and Van Rhijn did not realize sufficiently at that time - and quite understandably so - that one had to go exactly in the galactic plane to find the real extent of the Galaxy, and there of course the absorption was all-important.

The Chairman: Thank you, Dr. Oort. The historians are especially happy to have you with us today. In spite of the many hands being raised, we must now close this discussion.