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J. C. KAPTEYN AND THE EARLY TWENTIETH-CENTURY UNIVERSE

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Introduction

With only a few recent exceptions, scholarly literature in the history of astronomy has generally emphasized planetary astronomy to the neglect of developments in stellar astronomy.[1] Within the broad field of stellar astronomy, the sub-discipline of 'statistical astronomy' has been among those areas that have fallen victim to this tendency.[2] Yet this sub-discipline has been of great importance, for during the first several decades of this century astronomers generally believed that a statistical approach to analysing the aggregate of stars would eventually lead to an accurate understanding of the architecture of the stellar universe. Although William Herschel initiated this program in the late eighteenth century, it was not until a century later that it began to achieve promise with the work of two prominent astronomers, J. C. Kapteyn (1851-1922) and Hugo von Seeliger (1849-1924). From about 1890 until roughly 1920, Kapteyn and Seeliger shaped astronomers' views of the stellar universe.

To understand the stellar universe through the use of statistical techniques, referred to as the "sidereal problem" in much of the technical literature, was considered the major research program of stellar astronomy during much of the period considered here. It is the purpose of this paper to examine Kapteyn's contributions to this field of astronomy during the three decades prior to the "astronomical revolution" of the 1920s. Kapteyn was motivated principally by his life-long desire to understand how the stars were distributed and arranged in space. Many of the methods he developed to investigate the complexities of the Milky Way system have become fundamental. The techniques he used and the provisional models of the stellar system he obtained provided astronomers with tools and concepts needed to explore with increasing success the Milky Way system. All of his efforts, including those directed towards his organization of star catalogues, and his work in the field of international diplomacy, were motivated by this one great, astronomical project. In what follows, we will focus principally on two developments: (1) Kapteyn's studies of systematic stellar motions, and (2) his analysis of the sidereal problem.

Equally significant for the development of astronomy, Kapteyn's interests in the Milky Way directly shaped the contributions of several generations of his students. Willem de Sitter, H. A. Weersman, Pieter van Rhijn, and through van Rhijn, Jan Oort, Bart Bok and others, made fundamental advances in modern astronomy. As Bok has recently reminisced in an oral interview for the American Institute of Physics: "In astronomy, I think we [the Dutch] all derived from Kapteyn. It all started with Kapteyn. Oort was Kapteyn's student. I was Oort's student. I was van Rhijn's student; van Rhijn was Kapteyn's student. Pannekoek was a great friend of Kapteyn's. So the rise of Dutch astronomy was entirely Kapteyn."[3] But Kapteyn's influence extended far beyond his native Holland. Early in his career he collaborated with David Gill in the production

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of the *Cape photographic Durchmusterung*. After Kapteyn had become a close colleague of George Ellery Hale and a Research Associate at the Mount Wilson facilities in 1908, Hale, relying on Kapteyn's advice, began employing a number of Dutch and European astronomers, including Adriaan van Maanen, Arnold Kohlschutter, van Rhijn, and Kapteyn's Danish future son-in-law, Ejnar Hertzsprung. More generally, Kapteyn's astronomical colleagues world-wide found his enthusiasm and penetrating insights infectious. Besides Gill and Hale, those whose work he influenced included Arthur S. Eddington, Karl Schwarzschild, Frank Dyson, Edward C. Pickering, and Walter S. Adams.

Early Developments

In its most general form, the problem that Kapteyn, Seeliger, and their colleagues attempted to solve was to find how the stars are distributed through space as a function of absolute magnitude, spectral type, and distance from the solar reference. In their studies of the architecture of the Milky Way system, statistical astronomers had increasingly recognized the importance of three fundamental relationships: (1) the luminosity function—describing the spread of stellar brightness by magnitude class (and spectral type), (2) the density function—expressing the absolute numbers of stars per unit volume, and (3) the velocity function—a probability function yielding the actual numbers of stars as a function of their space velocities and magnitudes.

Kapteyn's attack on the sidereal problem focused directly on these three relationships. His work, however, may be divided roughly into two periods, in which he conceptualized the problem in somewhat different terms. Up until about 1904, Kapteyn used primarily proper motion and parallax data for understanding the arrangement of the stars in space. This early work culminated in his 1902 discovery of "star-streaming", first publicly announced in 1904. The discovery of star-streaming, according to which stars appear to move in two distinct and diametrically opposite directions, represented a sort of watershed in his thinking. Not only did he confirm the existence of preferential stellar motions within a dynamic Milky Way, a discovery Eddington later considered one of the five most important events in astronomical history during the preceding 100 years, but Kapteyn began to realize the inadequacy of stellar motion studies alone to solve the sidereal problem. After this discovery he emphasized stellar luminosity and density studies vis-à-vis the stellar velocity relationship as a means towards unravelling the complex arrangement of the stars in space. While the direction of his research shifted after the discovery of star-streaming, however, his earlier motion studies still remained an ingredient of his later luminosity and density work.

After graduating from Utrecht with a physics doctorate in 1875, Kapteyn obtained a position as observer at the Leiden Observatory. Having been trained mostly in the rarified atmosphere of mathematics and physics, Kapteyn immediately set about learning the practical necessities of his newly adopted profession. His abilities were soon recognized, and early in 1878 Kapteyn was selected, from among four highly qualified candidates, to fill the newly instituted chair of astronomy and theoretical mechanics at the University of Groningen.[4] For his inaugural address to the University body, Kapteyn chose the topic "The parallax of the fixed stars", which suggests that he already regarded stellar distances as requisite knowledge for an understanding of the sidereal problem. Recognizing early the importance of a broad-based set of stellar data, Kapteyn eagerly studied the extant star catalogues; but even the great *Durchmusterung* catalogues of Argelander and Schoenfeld were limited to magnitudes and position, and so lacked the data on stellar motion that Kapteyn believed so important.

Kapteyn also recognized that he neither possessed the financial resources nor was located in the proper geographical climate to undertake successful observational studies. Yet if he was to explore the nature of the Milky Way, it would be necessary to acquire massive amounts of the right kind of data. As it happened, during this period David Gill, the director of the observatory at the Cape of Good Hope and the current leader in practical astronomy, was attempting to fill the hiatus created by Schoenfeld's delimitation of the southern *Bonner Durchmusterung* to $-22°$ declination. Recognizing the importance of Gill's work, Kapteyn offered to help him measure the numerous photographic plates and catalogue virtually countless stars. In this collaboration with Gill, Kapteyn recognized a real opportunity for mutual benefit: Gill would provide the raw observational data, and Kapteyn would reduce the material for Gill and, in turn, use it in his investigation of the sidereal problem. In offering his assistance to Gill, Kapteyn wrote in 1886: "I think my enthusiasm for the matter will be equal to (say) six or seven years of such work."[5] In the end, it took nearly thirteen years of Kapteyn's constant attention before the *Cape photographic Durchmusterung* was completed in three volumes published between 1896 and 1900. Thus began Kapteyn's international cooperation in astronomical research. By the time the *Cape* project had gone to press, Kapteyn's laboratory had become institutionalized, and named the "Astronomical Laboratory at Groningen". Later, after Kapteyn's 1906 "Plan of Selected Areas", according to which a number of observatories would coordinate their observational work of selected stellar regions, had been adopted, Kapteyn's astronomical laboratory provided worldwide the resources for reduction and analysis of the data. His American colleague, Frederick H. Seares, later wrote that "Kapteyn presented the unique figure of an astronomer without a telescope. More accurately, all the telescopes of the world were his".[6]

Having acquired both extensive experience and the astronomical data, during the 1890s Kapteyn produced the first of a series of papers on the nature of stellar motion, all with the purpose of eventually solving the sidereal problem. Of course, these studies were not developed within a vacuum, but relied on the earlier work of numerous nineteenth-century astronomers. Briefly, an understanding of the arrangement of the stars in space had been a major problem since the late eighteenth century when William Herschel enunciated his project, the discovery of the "construction of the heavens". A central, theoretical concern was over the kind of data that could be used accurately to measure stellar distances. In the absence of other methods, Herschel had suggested that distance was proportional to stellar brightness by the principle that "faintness means farness". He built his cosmology on this principle, even though he was aware, at least by the early years of the nineteenth century, of the contradiction implied by the existence of binary and multiple star systems with members of different luminosities. This anomaly motivated many others throughout the nineteenth century to seek an alternative and rigorously defensible measure of stellar distances.

Knowledge of stellar motions had begun with Edmond Halley's discovery in 1718 of the motion in latitude of three stars. In 1783, Herschel became the first to use known proper motions to derive the 'solar apex', or direction towards which the solar system is moving. By this time, some astronomers were beginning to suspect that large proper motion was a more reliable guide to the nearness of a star than apparent brightness, and this principle underlay the campaign of Bessel to use the "flying star", 61 Cygni, for the first successful measurement of trigonometric parallax.[7] Later in the century astronomers such as Friedrich Argelander and George Airy used Gauss's least-squares techniques to identify random fluctuations in stellar motions, and eventually concluded that random irregularities were due, not to observational errors, but rather to the *peculiar* or individual motions of stars relative to other stars.[8] They recognized the systematic nature of proper motion data, and emphasized the need to reduce the data to yield peculiar motions. As a result, during the last half of the century there came into widespread use the assumption of randomness among the real motions of stars.

This assumption was coupled with the belief that proper motion data could be used as a measure of stellar distances. Bessel's measurement of the parallax of 61 Cygni in 1838 was followed by other successes, and by the 1870s astronomers, like the American T. H. Safford, were arguing that "...the stars' distances are inversely proportional *upon the whole* to their [the stars'] proper motions".[9] Many more stellar distances could be determined if proper motion data could be used directly in this way, for proper motion data were available relatively abundantly: for each known trigonometric parallax there were scores of measured proper motions.

After 1875, the assumptions that stellar distances were inversely proportional to proper motions and that stellar motions were random increasingly influenced the direction of attack on the sidereal problem. This is not to suggest that there was complete unanimity among astronomers. In particular, Maxwell Hall, Hugo Gyldén, and Eduard Schoenfeld had independently proposed dynamical models of the stellar system in which random stellar motions were rejected and replaced by inter-stellar forces. Thus, in the words of Hall, "the Sun and stars are ... subject to the same law of Force, and revolve in immense orbits round the same centre".[10] But non-dynamicists, such as Argelander, Airy, and Safford, accepted the hypothesis of random motions applied to large groups of stars.[11]

Though the dynamic models were perhaps intellectually more appealing, the sheer immensity of the sidereal problem prevented them from providing a conceptual basis needed to understand the arrangement of the stars in space. In contrast, non-dynamicists increasingly found empirical evidence correlating stellar motions with parallaxes. The publication in 1888 of Auwers's new reduction of Bradley's star catalogue, which listed 3,200 reliable proper motions, further stimulated these efforts. Nevertheless, prior to Kapteyn's studies of stellar motions during the 1890s, no one had succeeded convincingly in relating a distance measure based 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 precise relationship between proper motions, parallaxes, and apparent magnitudes, even if only statistically based, might well be complex.

Although Kapteyn generally preferred to use the distances directly derived from measured (trigonometric) parallaxes, he recognized that the solution of the sidereal problem, even with improved photographic techniques, demanded a much broader base than that allowed simply by the Earth's orbit. His emphasis on stellar motions was therefore motivated by both practical and theoretical considerations. From a practical point of view, by correlating proper motions with stellar parallaxes, the base of parallaxes could be extended on the ever-increasing base line of the Sun's motion through space. From a theoretical point of view, for an understanding of the structure of the stellar system knowledge of only the mean distances of groups of stars, rather than the actual 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 correlation between proper motion and spectral type.[12]

Drawing on these various developments, Kapteyn examined the proper motions in Auwers's catalogue and obtained a relationship correlating known (trigonometric) parallaxes with proper motions and magnitudes. Generalizing his results over large numbers of stars, he derived a statistical function that expressed 'mean' distances. Published in 1901, the 'mean parallax' relationship not only culminated years of careful analysis of stellar motion data, but also led directly to Kapteyn's luminosity function, which he obtained within a few months. Thus his analysis yielded a method for determining distances to stars, and it allowed him conceptually to recast his solution to the sidereal problem in terms of the luminosity function, rather than the velocity function. While there were some minor emendations to his results, it was widely believed that his mean parallaxes were substantially correct. In terms of the contributions of classical statistical astronomy, the 'mean parallax' formula has been exceeded in importance only by the luminosity and density laws, and by a relationship called the "fundamental equation of stellar statistics".[13]

Methodology and the Discovery of Star-Streaming

During the 1890s, Kapteyn's investigations were based on the supposition that stellar motions were the key to understanding the distribution of the stars. His developments were later expressed in the so-called velocity law, which was supposed to relate the velocities of stars to their brightnesses. This relationship, Kapteyn argued, would not only provide an understanding of the stellar system, but would also lead to the derivation of the density and luminosity laws. The latter, it was hoped, would in turn lead to a detailed understanding of the Milky Way system. In the final analysis, however, conclusive observational evidence for his velocity relationship was entirely lacking. In the words of Eddington, of all the numbers of the famous *Groningen publications*, a series forming "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...".[14] How could an unwritten work be so significant?

Speaking before the Amsterdam Academy of Sciences in 1895, and again in

1897, Kapteyn announced the creation of a mathematico-statistical theory expressed in the form of integral equations that related star-counts, the density function, and a Gaussian probability function of proper motions.[15] A necessary component of his theory was the standard nineteenth-century assumption that stellar motions are randomly distributed. With his mathematician brother, Willem Kapteyn, professor at the University of Utrecht, as co-author, the complete discussion of his velocity equations was published in 1900 as no. 5 in the *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.[16]

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 their velocity law, the theory, in Eddington's words, "turned out to be so wide of the mark that not even the beginnings of a comparison [with the observational evidence] could be made".[17] 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 valid *a priori*, already in 1895 Hermann Kobold showed conclusively that a random distribution did not represent the observed motions of nearby stars in Auwers's catalogue.[18] Soon Kapteyn was to provide an explanation of this startling fact in terms of preferential motions. Meanwhile, the anomaly between theory and evidence represented a critical problem for Kapteyn's program, because such a basic discrepancy directly affected one of his stated aims: the derivation of the density and luminosity laws from the allegedly more fundamental velocity relationship.

Despite the importance of theory in directing Kapteyn's research program, he claimed to be an inductivist in his scientific methodology. By this he meant primarily that one must always search for a greater data base, and that it was from this, with a properly formed conceptual framework, that hidden relationships would emerge. In reality, however, he organized the assembled data according to preconceived ideas. His velocity theory is a good case in point. Indeed, when it became clear that the evidence needed to support the theory was not forthcoming, he proposed several hypotheses to explain the alleged discrepancy: (1) the apex value was incorrect, (2) the proper motion values were incorrect, and (3) there were preferential stellar motions. Although he showed theoretically that the first two explanations could account for the failure of his theory, in the end he concluded that both the apex value and the proper motions had been calculated correctly, and he was therefore forced to conclude that stellar motions are not randomly distributed.[19] 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.

Kapteyn continually emphasized the need for data, data, and more data, thus reaffirming his inductive proclivities that eventually led to his discovery. "My studies", wrote Kapteyn to Hale in September 1915, "have made of me more and more of a statistician and for statistics we must have great masses of data, of course".[20] Kapteyn's view of proper scientific methodology was to combine both deductive and inductive approaches. Commenting on the importance of an inductive (though non-Baconian) approach in his letter to Hale, Kapteyn illustrated his point with his discovery of star-streaming:

> I also believe ... that we neglect the 'Art of discovery' too much. My impression is that we are still not sufficiently imbued with the sense of the absolute necessity of proceeding by induction. 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. These 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 from the individual figures then gave the isolated perturbations. These 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 2 star streams "il n'y a qu'un pas".
>
> 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.[21]

By 1902 Kapteyn had rejected his original velocity theory and had discovered star-streaming. Finding that the stars tend 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 at the St Louis

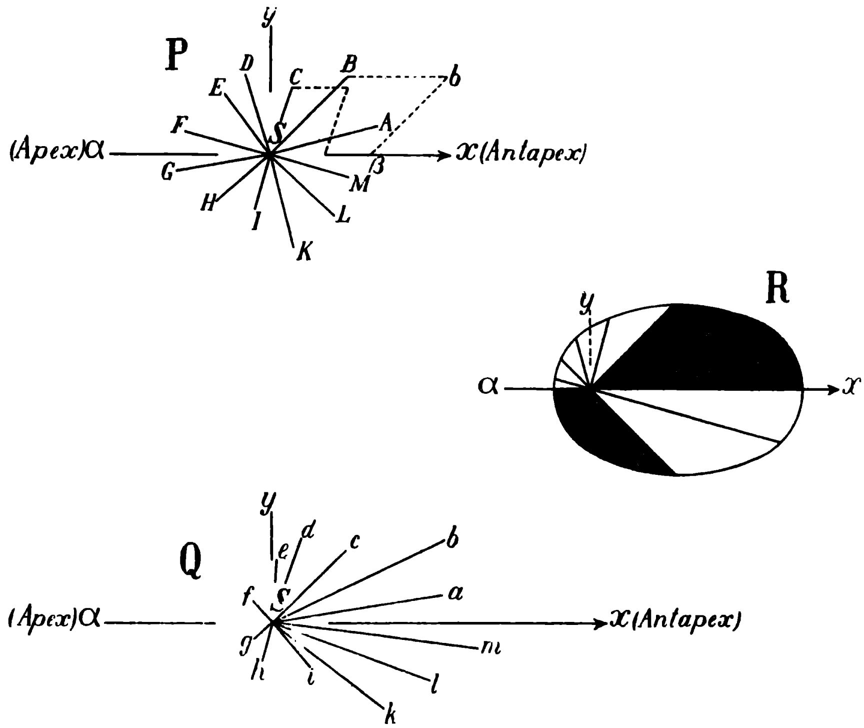

FIG. 1. *P* represents the distribution of the peculiar proper motions for a group of stars located at *S*. The directions of motion are randomly distributed, while the length of each radial vector represents the magnitude of motion. When the observer is in motion towards the apex, the observed motions of stars as in *P* becomes *Q*. In *R* the radial vectors making angles between 0° and −60° and between 60° and 180° have been blackened. (From J. C. Kapteyn, "Star-streaming", *Report of the British Association for the Advancement of Science*, 1905, Section A, 257-65, p. 258.)

World Exhibition in 1904, and again, more importantly, at the 1905 meeting of the British Association for the Advancement of Science in Cape Town (see Figure 1). At both meetings, Kapteyn emphasized that not the "slightest doubt" could be entertained concerning the reality of star-streaming. In fact, Kapteyn declared emphatically, "*all* the stars, without exception, belong to one of the two streams".[22] His over-riding concern, at this point, was not to reaffirm the phenomenon, but the necessity to *confirm* the theory, 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 by which 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].[23]

He had in mind the Lick Observatory staff in California and particularly W. W. Campbell, who had been measuring radial velocities since about 1900 and possessed the data needed to confirm Kapteyn's hypothesis conclusively; but, unfortunately from Kapteyn's point-of-view, Campbell was less interested in verifying someone else's hypothesis (such as "star-streaming") than in doing his own original work. Accordingly, during a long tenure as Research Associate at the Mount Wilson Observatory from 1908 until his death in 1922, Kapteyn continually encouraged Hale and his associates there to further their radial velocity work with the 60-inch and later the 100-inch telescopes, in part to validate star-streaming.[24]

Kapteyn's discovery greatly stimulated interest in 'preferential' stellar motions. He himself participated relatively little in these later developments, however, since star-streaming *per se* could not, in his view, add directly to a detailed understanding of the architecture of the stellar system. Yet he remained keenly aware of the newer work, particularly the theoretical explanations of star-streaming offered by Karl Schwarzschild and Arthur S. Eddington. Occasionally, later in his career, he would even describe the sidereal problem as "the study of the arrangement of stars in space and their *systematic motions*".[25]

While his own active research in star-streaming was diminishing, Kapteyn's investigations into the peculiar, proper, and radial motions of stars continued unabated.[26] In this regard, his research took two directions: study of the 'velocity law', and of proper motions. While it is true that Kapteyn continued to work on a theory of the space velocities of stars, where the frequency of velocities is a function of galactic latitude, magnitudes, spectra and proper motions, he would write as late as 1915 that work "on the velocity-law has been making hardly any progress".[27] His research interest in proper motions and parallaxes, however, remained intense. After all, parallaxes and proper motions were vital components of his theory of mean parallaxes, the basis to his luminosity studies.[28]

During the war years, Kapteyn and Hale exchanged many letters discussing scientific method and proper approaches to scientific work. While Kapteyn's research was generally empirical, his approach often possessed a noticeable mathematical flavour. Certainly he recognised that for many types of investigations his inductive "method" was only partially useful. In his September 1915 letter to Hale, in which he explained how he had discovered star-streaming, Kapteyn noted:

> From the standpoint of a finished theory [of star-streaming], the [inductive] method must be conceded to be very little rigorous. I have a notion that it will be so in most cases. Therefore it has to be supplemented by a deductive treatment of the problem: given that there are two streams, show rigorously that the observations are well represented. I think that it is on account of the mathematical rigor that the latter problem (the deductive one) appeals to so many even of the very best men....[29]

Among the "very best men", Kapteyn included Schwarzschild and Eddington, both of whom were mathematically rigorous in their work on star-streaming.

Kapteyn concluded this letter to Hale by noting that "the inductive part of the investigation, with all its defects, is incomparably the more important part of research. The deductive problem can be solved by any well skilled mathematician". Here Kapteyn was confusing mathematical deduction with hypothesis formation and the deducing of tentative claims based on the assumptions and principles of the relevant body of knowledge. On precisely this point, Hale responded to Kapteyn's "most interesting letter of September 23", and gently corrected Kapteyn while attempting to convince him of the fundamental significance of "framing hypotheses":

> The account of your discovery of the two star streams is a splendid illustration of the value of the inductive method, which doubtless serves best in a large class of investigations. And yet I cannot help feeling that in many other cases a combination of deduction and induction is more likely to be successful. In fact, I constantly find myself instinctively framing hypotheses as guides to research, ... and therefore endeavouring to construct multiple hypotheses to account for obscure phenomenon. Each hypothesis suggests the application of a series of criteria, and it usually becomes possible to eliminate some of them very soon.... In my experience, therefore, deductive methods are almost invariably applied. Frequently they are the merest guesses, without a substantial theoretical basis. But each suggests experiments or tests which would hardly occur to me otherwise, and thus almost any hypothesis may prove useful.[30]

Continuing, Hale showed that Kapteyn actually framed hypotheses, and in fact had done so in both his star-streaming discovery and his research on stellar velocities and movements:

> Is it not true that you also employ deductive methods in many instances? Sometimes they may enter only tacitly, but nevertheless play an important part in your investigations. In fact, on a basis of pure induction your imagination would have little opportunity to serve as a guide....Your new result regarding the star streams is of the greatest interest and importance, especially as it offers a much more satisfactory picture of the star system than the separate streams afforded.... Is it even possible that you would not have undertaken the investigation which led to the discovery of the star streams if you had not been started in this direction by an (incorrect) hypothesis?[31]

For our purposes, it is perhaps most important to note that Kapteyn eventually agreed with Hale that hypotheses are the essential guiding heuristics needed to make sense of disparate phenomena. In response to Hale's reply, Kapteyn wrote: "After all I think that our difference is more a difference of degree. One cannot pass from the observations to the laws underlying them, than by making certain jumps, certain hypotheses and applying these to the observations in hand, and modifying them till they fit."[32]

Distribution of Stars

Frustrated in his hope of deriving the velocity law because of the phenomenon of star-streaming, Kapteyn, while using his original data and his earlier work with the mean-parallax relationship, increasingly turned his attention to a more direct approach to understanding the structure of the sidereal universe. Using the basic data he had worked with for so many years, Kapteyn eventually obtained solutions for the luminosity and density functions, which describe, respectively, the spread of stellar brightness from the faintest stars (in absolute magnitude) to the brightest, and the density distribution of stars in terms of distances from the solar region. Thus, even though his efforts at detailing the Milky Way were temporarily thwarted by the failure of his velocity theory, he continued to use both the observational data and the results of his earlier work in order to understand the larger sidereal system. Indeed, at the very end of his career, he returned to these early efforts and developed a dynamical model of the stellar system, in which he combined both his stellar motion studies and the more fruitful results from his luminosity and density research.

In 1898, Kapteyn's contemporary Hugo von Seeliger derived what the Swedish astronomer C. V. L. Charlier later christened "the fundamental equation of stellar statistics". Seelinger developed the mathematics and generally used the most current data available that would allow for a purely *analytic* solution of the sidereal problem.[33] The "fundamental equation"

$$A(m) = \int \varphi(M)\, dM \int D(r)\, dr$$

related, in the form of integral equations, the following functions:

$A(m)$: counts of stars given by magnitude class m,

$\varphi(M)$: luminosity function with absolute magnitude M,

$D(r)$: density function with distance r from the Sun.

In his early studies, Seeliger was forced to assume an arbitrary probability function for the luminosity distribution, since one had not yet been empirically obtained. Though the work was extremely tedious, counts of stars by magnitude class were directly available. Using analytic forms for the star-counts and luminosity function, Seeliger succeeded in solving his integral equations, yielding the third, or density, function.[34] Generally speaking, he left to others the precise determination of the empirical form of the luminosity function. Nevertheless, Seeliger's fundamental equations provided the mathematical description Kapteyn later needed to solve the sidereal problem once the necessary data had become available.

While Seeliger was the first theoretical astronomer to provide a solution (in 1898) to the sidereal problem, his analysis was mathematically very abstract and it was not based rigorously on all the available observational data. Kapteyn had also been working on the sidereal problem, but he was in greater command of all the empirical data, particularly proper motions and parallaxes. Both Seeliger and Kapteyn recognized the essential importance of relating the star-counts, the density distribution, and the luminosity functions; they further realized that an accurate representation of the arrangement of the stars in space ($D(r)$ in Seeliger's terms) would require an exact understanding of the luminosity relationship. Kapteyn had already concluded during the 1890s that a precise description of stellar distances could be determined from data obtained from stellar motions and stellar brightnesses. By 1901 he had developed a *numerical* technique for obtaining the luminosity function that related the magnitudes of stars to their motions, and hence their mean distances. Kapteyn's solution, however, was more clearly comprehensible and far less abstract than Seeliger's. Partly for this reason, Kapteyn's work became more widely known than the highly original investigations of Seeliger.[35]

Kapteyn's procedure was first published in 1901 in his classic paper "On the luminosity of the fixed stars",[36] and all the subsequent analyses of the sidereal

TABLE 1 (this and the following tables are from J. C. Kapteyn, "On the luminosity of the fixed stars", *Groningen publications*, no. 11 (1902), 3-32, pp. 8, 10, 11).

μ	Mag. / Mean μ	1.5—2.5	2.5—3.5	3.5—4.5	4.5—5.5	5.5—6.5	6.5—7.5	7.5—8.5	8.5—9.5
		2.1	3.1	4.1	5.1	6.1	7.1	8.1	9.1
0′.000—0″.009	0″.005	6	5	22	90	343	1504	9010	38257
.010— .019	.015	4	15	52	194	638	1896	7313.	28184
.020— .029	.025	1	10	41	177	595	1910	5882	21225
.030— .039	.035	3	16	27	188	542	1910	4768	15809
.040— .049	.045	3	12	27	93	461	1490	3877	12045
.050— .059	.055	5	13	22	86	357	1249	3342	9184
.060— .069	.065	1	4	25	77	252	963	2540	6775
.070— .079	.075	2	2	18	71	247	752	1827	4968
.080— .089	.085		5	25	45	209	692	1337	3614
.090— .099	.095	1	9	17	54	200	646	802	2409
.100— .149	.125	5	10	57	152	424	963	1649	4373
.150— .199	.175	5	9	34	79	181	420	1070	2033
.200— .299	.25	5	11	32	73	200	315	670	919
.300— .399	.35	1	3	17	43	105	75	178	422
.400— .499	.45		5	12	18	34	121	133	196
.500— .599	.55	1	3	8	7	24	38	53	60
.600— .699	.65	2		2	7	5	24	32	36
.700— .799	.75			9	5	10	17	27	26
.800— .899	.85	1		3	2		12	13	16
0.900— 0.999	0.95				2		6	9	10
1.000— 1.199	1.1		1	3	5		7.5	9	10
1.200— 1.399	1.3			3	2	5	12.0	9	7.6
1.400— 1.599	1.5				2		4.5	4.4	6.0
1.600— 1.799	1.7						1.5	4 4	1.5
1.800— 1.999	1.9			2	2		1.5	0.0	1.5
2.000— 2.999	2.5		1				6.0	8.9	6.0
3.000— 3.999	3.5					5	3 0	4.5	6.0
4.000— 4.999	4.5				2		1.5		3.0
5.000— 5.999	5.5					5		4.5	
6.000— 6.999	6.5						1.5		4.5
7.000— 7.999	7.5								
Total		46	134	458	1476	4842	15 042	44 576	150 607

problem that he published during the remaining years of his life followed the method outlined in this work. Briefly, his method entailed placing the catalogued stars in cells corresponding to their apparent magnitude and probable proper motion (see Table 1). (Only stars with Secchi spectral types I and II were used, because these could be more easily derived from available star counts.) Utilizing his 'mean parallax' formula, which expressed a dependency between calculated parallaxes on the one hand, and apparent magnitudes and proper

TABLE 2.

Limits of π			fraction of the whole	Number.
0″00000	and	0″00100	0.001	0
00100	,,	00158	.004	2
00158	,,	00251	.028	13
00251	,,	00398	.097	45
00398	,,	00631	.209	96
00631	,,	0100	.275	127
0100	,,	0158	.226	104
0158	,,	0251	.116	54
0251	,,	0398	.0358	16.5
0398	,,	0631	.0068	3.1
> 0″0631			.0009	0.4
			1.000	461

motions on the other, Kapteyn next calculated the mean parallaxes corresponding to each cell. The stars in each cell are not, however, all found at the same parallax distance, but rather they are distributed about some mean as expressed by the 'mean parallax' formula. Thus, for example, for the 461 stars with p.m. = 0″.045 and app.mag. = 6.1, the mean parallax is 0″.0102. These results represent only *mean parallaxes*. In actuality the stars would be distributed according to the laws of probability defined for purposes of analysis by some Gaussian function. The exact shape of the Gaussian curve was derived from a determination of the spread of 58 particular 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. Thus, for example, the 461 stars are distributed about their mean as shown in Table 2. The limiting characteristics of the distributed stars correspond to spherical concentric shells about the Sun. The first shell represents a distance of ten parsecs (Kapteyn's unit distance); all subsequent shells represent distances corresponding to an increase of one apparent magnitude. Since the stars in each cell are distributed across all shells, Kapteyn, after deriving similar results for each cell of Table 1, produced a two-dimensional table (see Table 3) in which the catalogued stars were distributed by magnitude class and mean distances. The magnitudes were normalized by conversion to their absolute magnitude by means of the magnitude-distance relationship. This could directly yield the spread of magnitudes or luminosities (as total illuminating power) of all stars. Finally, Kapteyn reduced his tabular representation to an analytic form.

TABLE 3.

π	Mag. / $\bar{\pi}$	—0.9	0.1	1.1	2.1	3.1	4.1	5.1	6.1	7.1	8.1	9.1	Vol.
0″.00000—0″.00100					0.4	0.5	3.0	18.0	84.0	440.0	3016.0	15363.8	
.00100— .00158	0″.00118				1.0	2.0	8.0	34.0	152.0	667.0	3658.0	16197.0	3 140 000
.00158— .00251	.00187			1	1.9	2.7	15.2	76.0	306.0	1216.0	5419.0	21953.0	788 000
.00251— .00398	.00296				2.9	7.9	29.1	141.0	531.0	1942.0	6912.0	25578.0	198 000
.00398— .00631	.00469				3.8	14.9	48.7	211.0	770.8	2642.0	7880.0	26044.0	49 700
.00631— .0100	.00743	1	1	1	5.1	20.2	64.7	255.5	901.0	2899.7	7352.0	20977.0	12 500
.0100 — .0158	.0118				6.1	23.7	74.6	248.8	833.5	2403.2	5278.5	14067.5	3 140
.0158 — .0251	.0187				7.3	21.8	74.5	209.4	614.3	1556.9	3003.9	6789.5	788
.0251 — .0398	.0296			2	6.5	17.6	59.2	142.4	367.5	780.5	1338.0	2568.4	198
.0398 — .0631	.0469		1	2	5.2	11.5	40.1	78.8	177.2	322.5	496.4	792.1	49.7
.0631 — .0100	.0743		1	1	3.3	6.4	23.3	37.7	69.1	114.6	157.3	207.9	12.5
.0100 — .158	.118		1	1	1.6	3.0	11.4	15.3	22.9	39.2	45.8	50.0	3.14
>.158	.204	1	1	2	0.9	1.8	6.2	8.1	12.7	18.4	19.6	18.8	1.05
		2	5	10	46.0	134.0	458.0	1476.0	4842.0	15042.0	44576.5	150607.0	

Others, such as Hugo Gyldén, G. V. Schiaparelli, and particularly Seeliger, had noted the importance of using a Gaussian function to represent the luminosities, but Kapteyn alone succeeded in actually deriving a workable, and defensible, function. Kapteyn thus 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".[37] 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 as a by-product, Kapteyn's procedure also yielded the density of stars in the local solar neighbourhood. Since only stars brighter than magnitude 9.5 were used, the density relationship was tentative at best. Nevertheless, a first, provisional solution was now available.

Statistical Astronomy c. 1900

During most of the two decades preceding Shapley's work and with it the emergence of the 'new' astronomy of the 1920s, statistical astronomers generally believed that the density and luminosity laws alone would be sufficient for understanding fully the arrangement of the stars in space. When Kapteyn's luminosity paper appeared in 1901, the possibilities inherent in a rigorous approach to statistical investigations were immediately improved. This was more the beginning, however, than the end. Kapteyn himself considered his 1901 paper as providing only a first approximation to the sidereal question, a problem which, in his opinion, "must be solved by successive approximations". Indeed, the 1901 solution only considered the density distribution as a distance from the solar neighbourhood, entirely ignoring both galactic latitude and longitude.

It has frequently been observed that the significance of a research program lies not so much in the number of questions that are answered, as in the number of areas for additional research that are opened up as a result of the work. As a "first attempt" at the solution to the sidereal problem, Kapteyn's study of the general luminosity function in fact made several critical assumptions that in following years opened extremely fruitful research areas for statistical astronomers by defining key problems. His 1901 work had assumed: (1) negligible light absorption; (2) a Sun-centred stellar system; (3) a luminosity curve uniform throughout the entire stellar system (i.e., independent of distance and direction from the Sun); (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.

The question of the transparency of space had been discussed by many nineteenth-century astronomers, including William Herschel, W. Olbers, F. G. W. Struve, and Kapteyn's contemporary Seeliger. Kapteyn recognized that the existence of an interstellar absorbing medium could seriously alter the form of the luminosity curve and thus fundamentally change the parameters describing his stellar system. During his vacation months from 1908 until the outbreak of the First World War, while Kapteyn held a Research Associate position at Mount Wilson, he himself remained actively involved in this research, and he encouraged others on the Mount Wilson staff to study the problem. By 1915, Kapteyn had submitted three papers dealing with it.

Between the time of his earlier star-streaming work and 1915, Kapteyn's view on the question of absorption changed enormously. He and others began to realize that the problem involved not only general spatial absorption, but also selective absorption (dependency on wavelength), 'scattering in space' of stellar light, and photographic film absorption. During most of his early work on the sidereal problem, Kapteyn had made the (reasonable) assumption that stellar absorption was negligible, for without additional empirical justification any other assumption would have been entirely *ad hoc*. Under this assumption, his derived density distribution function gave him a maximum value in the solar region, justifying a Sun-centred model. The question, of course, was whether in fact the density distribution was real or merely apparent. And the answer to this question depended in large measure on the existence of interstellar absorption.[38]

Kapteyn's interest in the absorption question was heightened when G. C. Comstock examined the problem in a somewhat superficial analysis published in 1904, using techniques and data derived from Kapteyn's 1901 work. Comstock used the outmoded assumption of equal intrinsic stellar brightness, which, when coupled with Kapteyn's results for stellar distances, led him to assert an absorbing coefficient of 17mag/kpc. Kapteyn was quick to point out that Comstock's results led to entirely erroneous density distributions within as little as one kiloparsec of the Sun. But, spurred on by Comstock's work, Kapteyn analysed the general absorption question, and found that, for various absorption coefficient values, a nearly constant stellar density distribution could be obtained with an absorption of only 1.6mag/kpc. This result had the advantage that it rationalized away a Sun-centred cosmology, implying a 'uniform' universe without a recognizable centre.[39]

His 1904 results were tentative at best, and Kapteyn admitted as much. Other methods, therefore, would have to be utilized. Since reliable, empirical results remained scanty, within the next few years a critical assessment of instruments and techniques led Kapteyn and others again to question their absorption coefficient results. Writing to the Mount Wilson astronomer Harold D. Babcock early in 1909, Kapteyn suggested:

> As the silver grain of the film must cause at least some scattering of light, it seems almost necessary, that the transmitted light must be somewhat reddish—on the same ground on which the scattering in space must give a reddish colour to the stars. If so, then the effect must be much diminished and may even be reversed.... If you had hundreds of bright and faint stars, you could even probably determine from your plates, both the amount of space—and of film—absorption.[40]

He therefore proposed to search for reddening of light caused by a scattering dependent on wavelength. Since the eye is less sensitive to blue light than is a photographic plate, Kapteyn suggested comparing the magnitudes of stars obtained visually with those obtained photographically. The result revealed an absorption of 0.3mag/kpc.[41]

Kapteyn was not able to return to serious research into absorption until his summer tenure at Mount Wilson in 1913. Though his results during these few summer months were preliminary, Kapteyn predicted that researchers would find significant general absorption, and not primarily selective absorption owing to interstellar material. Walter S. Adams, a leading spectroscopist and second-in-command at Mount Wilson, and Frederick H. Seares, also a permanent staff observer at Mount Wilson, continued to study the general spatial absorption predicted by Kapteyn, and soon obtained encouraging results. As Hale wrote to Kapteyn: "With the new material now available I hope you will be in a position to complete your paper on the general question of space absorption, as the 'psychological moment' for its publication seems to have arrived."[42] It soon became clear to both Hale and Kapteyn, however, that the 'psychological moment' for discussing *general* absorption had, contrariwise, not yet arrived. Replying to Hale's letter, Kapteyn emphasized the importance of wavelength-dependent absorption vis-à-vis general absorption: "I feel quite elated about the turn that the absorption of space work is taking, in particular if it really turns out that there is an influence on the hydrogen [spectral] lines. It is almost too beautiful to be true."[43] The most spectacular result to emerge from the work of the Mount Wilson astronomers indicated that, regardless of whether there exists interstellar absorption or not, there is an empirically verified absorption of starlight in passing through interstellar material. Writing to Kapteyn in May 1914, Hale summarized the current position of the Mount Wilson research: "It does not now seem probable that the hydrogen absorption occurs in space, as the lines appear to be displaced equally with other stellar lines. This ... indicates that the star itself is responsible for the changes of relative intensity."[44]

While Kapteyn continued slowly to relinquish his earlier view of substantial spatial absorption, he still hoped conclusive evidence would eventually appear. In writing to Hale earlier in 1914, Kapteyn remarked: "If we could but find some lines ... altogether due to gaseous matter in space! It should mean I think almost a revelation in astronomy."[45] Unfortunately, this was to remain an elusive quest for another decade, when empirical evidence of line absorption was at last discovered. The definitive work on general absorption was done by Robert Trumpler in 1929-30. Until then, other approaches to the various absorption questions were tried, and astronomers at Mount Wilson remained intensely interested in the question.

Harlow Shapley obtained an appointment from Hale as an assistant at the Mount Wilson Solar Observatory just prior to receiving his Ph.D. from Princeton in 1913.[46] Soon after Shapley arrived at the Pasadena facilities in the spring of 1914, Hale set him the task of investigating globular clusters. After carrying out extensive studies of the stars in many of the clusters, Shapley concluded that the clusters must be between 10 and 100 kiloparsecs from the solar region. At these distances, if interstellar absorption existed, even in the small amounts proposed by Kapteyn, then all the cluster stars should be reddened. In fact, the colours of stars ranged from red to blue, hardly an encouraging result for Kapteyn's program. Interstellar space, concluded Shapley, must be transparent: Kapteyn's earlier value "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".[47] By late 1915, Kapteyn personally wrote to Shapley acknowledging the latter's cluster results as both "fine and surprising". But Kapteyn also expressed his disappointment that an empirically verifiable interstellar absorption had not yet been obtained.[48]

Lacking a plausible alternative, statistical astronomers had generally assumed that the solar system was centrally located in the universe. To be sure, the nature of this assertion made many feel increasingly uneasy; yet, as a working hypothesis, it was simple and reasonably defensible. As we have indicated, by 1915 studies had confirmed the absence of an absorbing medium, and the results supported a maximum density distribution within the immediate solar neighbourhood. In the September 1915 letter to Hale quoted earlier, in which Kapteyn discoursed on method and on star-streaming, he had concluded that the direction and velocity of the star-streams also gave credence to the privileged solar position:

> ...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.
>
> Twenty years ago this would have made me very sceptical ... Now it is not so. — Seeliger, Schwarzschild, Eddington and myself have found that the number of stars is greater near the sun. I have sometimes felt uneasy in my mind about this result, because in its derivation the consideration of the scattering of light in space has been neglected. Still it appears more and more that the scattering must be too small, and also somewhat different in character from what would explain the change in apparent density. The change is therefore pretty surely real. Here then we have another, really very strong indication that the position of our solar system is nearly central.[49]

His letter to Hale only confirmed privately what he had, for all intents and purposes, already committed himself to publicly as early as 1901: that the local solar neighbourhood seemed centrally located within the larger sidereal system. While he included the usual qualification that the density distribution remained provisional, even his 1908 solution of the sidereal problem confirmed the earlier general solution (1901) of a Sun-centred stellar system. As Kapteyn expressed it to Hale in 1908: "I am just now head over ears working out the problem of the thinning out of the stars as we recede from the solar system. In [Groningen] Publ. 11 I think I have arrived at what I consider a fairly trustworthy determination of the luminosity 'curve', but, as mentioned there, the law of the densities here derived could only be considered as a very provisional and rough one."[50] Following the results of 1901, Kapteyn's 1908 work had also been on the basis of negligible absorption.

It could be assumed that all regions of space, regardless of galactic position or distance from the Sun, exhibit precisely the same *distribution* of luminosities within the same unit volume. Kapteyn's third assumption, that "the luminosity-curve is the same for different distances from the sun", was therefore theoretically independent of the question of interstellar absorption.[51] However, since the derivation of his luminosity function required empirical knowledge of parallaxes and proper motions, as well as of stellar luminosities, the function could actually be determined only for the local solar neighbourhood.

While Kapteyn devoted a considerable part of his time to analysing the luminosity function, the precise shape of the luminosity curve continued to remain a difficult problem. Nevertheless, he hoped additional data could be obtained that would, in particular, yield more accurate information for stars of increasingly fainter magnitudes. Already as early as 1909, for example, he argued vehemently on theoretical and practical grounds for the construction of the 100-inch telescope needed for solution of the sidereal problem:

> [E]very inch of aperture gained promises a great advance in astronomical possibilities.... For if we can determine both the frequency law [luminosity law] and the absorption of space, the determination of the real arrangement of stars in space very probably will not present any very serious obstacles.[52]

Kapteyn had already noted the importance of spectral type in his 1901 studies of the stellar system. But the revolutionizing developments in spectral classification by A. J. Cannon, H. N. Russell, and Hertzsprung early in the century, encouraged Kapteyn to emphasize the importance of detailed spectral studies. Writing to Adams in 1912 concerning the crucial importance of the 100-inch telescope, Kapteyn noted:

> Now that we begin to know something of the luminosity curve of stars of one particular class of spectrum—I have already derived such a curve for the Helium stars and will shortly try to do so for the A stars....
>
> Before we can really ... attack the question of the arrangement of stars separately for the different spectra, such a knowledge [of the colours] seems absolutely necessary.
>
> 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....[53]

Writing to Hale a few years later, Kapteyn outlined the research he and others were undertaking concerning this critical question. "[W]e can find the distributions in space of nearly all the Helium stars [and] ... 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."[54]

Stars are distributed not only by spectral type, but also by galactic position. Though the Herschels, Struve, and many others had noted the dependency of the number of stars (stellar density) on galactic latitude, it was Seeliger who, in the 1880s, first rigorously demonstrated this fact.[55] In later studies Kapteyn recognized Seeliger's work on this point, and noted that the luminosity curve, being a distribution function and not an absolute measure, is independent of both galactic longitude and latitude. On the other hand, the density function is, in the final analysis, an absolute measure of the numbers of stars per unit volume of space. Therefore, it would depend not only on the distance from the solar region (assuming the stellar system is Sun-centred), but also on galactic longitude and latitude. But a dependence relationship between density and galactic longitude was not rigorously confirmed until 1917, and so it had little effect on these early developments.[56] After 1918, with the emergence of Shapley's cosmology, and its increasing acceptance by astronomers after 1920, the question of the relation of density distribution to longitude no longer remained of immediate importance.

Finally, while Kapteyn remained sensitive to the validity of the dispersion of the measured parallaxes about their mean, he continued to assert the reliability of his earlier results. In 1920 he summed up his views on this question when 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]."[57] This was an important point since the dispersion determines the parameters of the mean-parallax formula.

Kapteyn's Universe

During the first decade of the twentieth century, there emerged three major centres for classical statistical astronomy: Groningen, with Kapteyn, Pieter van Rhijn and their Dutch students; Munich, with Seeliger and his 'school', including most notably Karl Schwarzschild and Hans Kienle; and Lund, with Charlier and his Swedish students. In England, Eddington and Frank Dyson also made notable contributions. With the gradual acceptance of relativity, however, and after Eddington published in 1914 a masterly summary of (statistical) astronomy in his *Stellar movements and the structure of the universe*, the leaders of astronomy in England ploughed new and fertile ground. Mostly it was left to the founders of statistical astronomy, the Dutch, the Germans, and later the Swedes, to refine the empirical support needed to explore the questions and investigate the mathematico-statistical basis of the sidereal problem.

Kapteyn's entire career was dominated—'obsessed' would be a better word—by his quest for a solution to the sidereal problem. He was directly involved in numerous mammoth projects for the collection of raw data, such as Gill's *Cape* survey, his own *Plan*, and later Pickering's photographic survey of the northern hemisphere.[58] His private correspondence, more so than his published work, indicates that he almost despaired of securing the data necessary to realize his goals. We could cite many examples from his letters, but one, written to Hale, will suffice: "In the study of the general system, however, we are still so little advanced and the data are so scanty that there are mostly not even sufficient for drawing up a good program."[59] This letter was written in 1918! Kapteyn was 67 years old, and had been searching for nearly forty years, ever since he gave his inaugural address to the University of Groningen in 1878.

His desire for a definitive solution of the sidereal problem was so compelling that he continued to be directly involved in data reduction and analysis, in order to obtain the needed empirical material. Indeed, the vast bulk of his professional time was actually spent doing this sort of measuring work. Gill, Hale, Adams, and many others continually cautioned Kapteyn, however, that his talents would be better used if he would focus on synthesis and remove himself from "mere" cataloguing. As Hale expressed it in 1912:

> [A] man of your ability ought not to be compelled to devote time and

> attention to such a piece of routine [as measuring and data reduction]. The more opportunity you have for thought on the larger phases of astronomical work, the more will astronomy benefit through the extraordinary range of your imaginative power. Routine work may not do you any harm, but it will certainly prevent you from dwelling on the larger theoretical aspects of the subject, and insofar as it does this it will handicap you.[60]

By about 1915, in fact, Kapteyn was increasingly turning his attention to these larger concerns. But he preferred to rely on the older, tried and true methods that had served him well for so many years. The time had finally arrived, he was convinced, for the "grand synthesis".

By this time, the basic concerns of statistical astronomy, as understood by Kapteyn, Seeliger, Charlier, and the main-line of research, had been further refined, and were focused on the following problems: (1) the relationship between spectral type and stellar distribution; (2) the relation between mean parallax, proper motions, and apparent magnitudes; (3) the nature of the velocity law and star-streaming in general; (4) the analytical form of the star-count function, including particularly its maximum magnitude value; (5) the relation between stellar distribution and galactic latitude and longitude; (6) the mathematical form of the luminosity function, and the absolute magnitude at which it obtains a maximum; and (7) the analytic form of the density law. As far as Dutch astronomy was concerned, provisional answers to many of these questions came together conceptually in Kapteyn's last papers, published between 1920 and 1922. In 1917, with the assistance of his colleague van Rhijn, Kapteyn had begun his final onslaught on the sidereal problem. Before his death in July 1922, he would obtain some plausible results.

Throughout his entire career, Kapteyn always recognized the absolute necessity of reliable data of the right sort, if even a provisional solution was to be achieved. Of course, the quality and accuracy of the empirical data had increased immeasurably since publication of his first stellar theory. Whereas in 1901 reliable data were available only for stars brighter than sixth magnitude, by the late 'teens the data had been extended to the twelfth magnitude, and began to fail altogether only beyond the fourteenth. In their now classic monograph on the stellar universe, in which they detailed their new theory, Kapteyn and van Rhijn noted:

> Now that, after so many years of preparation, our data seem at last to be sufficient for the purpose ... of making possible an elaborate treatment of the arrangement of the stars in space, ... we have been unable to restrain our curiosity and have resolved to carry through completely a small part of the work.[61]

Using Kapteyn's tabular method (1901), they derived from the newer data both the density law and the luminosity curve. The latter ranged over no fewer than eighteen magnitudes, and gave an approximation to the luminosities that was "astonishingly close". From their tabular results, the authors derived an analytic form for the new luminosity curve. Not only did the curve as a whole approximate to the empirical data, but most significantly, in their view, both the maximum of the curve and its dispersion matched recently obtained data

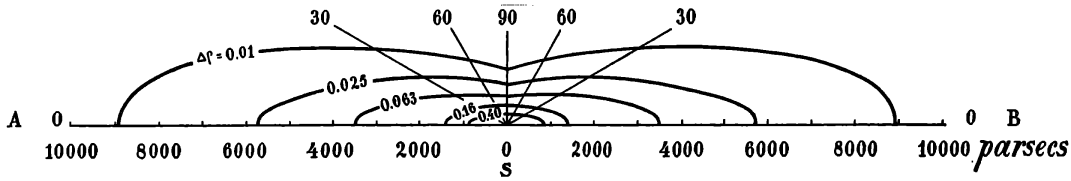

FIG. 2. Kapteyn's 1920 stellar system. The curves represent lines of equal density distribution perpendicular to the plane *AB* of the Galaxy. The numbers 0, 30, 60 and 90 represent galactic latitudes in degrees. Density numbers are relative, with the density of the Sun (assumed to be at the centre) taken as unity. (From J. C. Kapteyn and P. van Rhijn, "On the distribution of the stars in space especially in the high galactic latitudes", *Astrophysical journal*, lii (1920), 23-38, p. 37.)

extremely well. Thus it was not necessary, as it had been in 1901, to extrapolate the curve in order to find the position of the Gaussian maximum. These results they labelled the "first solution" of their grand synthesis.

Although the luminosity curve was derived only for the region within a local solar neighbourhood of radius 630 parsecs, within which it was assumed to be absolutely valid, it was obviously necessary to assume its *universal* applicability in order to deal with the *entire* stellar universe. Beyond 630 parsecs the paucity of directly observed data, such as parallaxes and proper motions, compelled a different approach. Here Kapteyn and van Rhijn employed Schwarzschild's version of the "fundamental equation of stellar statistics", in which Schwarzschild accounted for both the luminosity curve and the star-count function using second-order approximations. Kapteyn and van Rhijn's work also yielded nearly identical second-order results. Since their solutions were in accord with the mathematical work of Seeliger and Schwarzschild, Kapteyn and van Rhijn felt confident that the new density law correctly described the actual state of affairs beyond the local solar region. The Seeliger-Schwarzschild mathematics was particularly useful at those distances for which neither the luminosity nor the density functions could be derived using Kapteyn's tabular, or numerical, approach of 1901. Since the star-counts were empirically known and the luminosity curve now derived anew, the "fundamental equation" analytically yielded the density function.[62] Triumphantly they concluded that with "astonishing approximation" their results confirmed the mathematical results of Schwarzschild: "[T]his must give a first insight into the arrangement of the stars of the *whole* stellar system in space."[63] For purposes of their discussion, the derivation beyond 630 parsecs they called the "second solution".

By combining both the first and the second solutions, Kapteyn and van Rhijn described their most comprehensive model of the stellar universe (see Figure 2). For distances within 100 parsecs the second solution gave unacceptable values, while the first solution gave values in accord with empirical observations. Between 100 and 630 parsecs the two solutions agreed surprisingly well and, consequently, the mean of the two was chosen. Beyond 630 parsecs only the second solution was valid. In the 1920 paper, they described a transparent, ellipsoidal stellar system in which star density at low galactic latitudes diminishes in all directions with increasing distance from the Sun-centred system. Overall the dimensions of the system were 2,400 parsecs toward the galactic poles and 18,000 parsecs in the galactic plane. At 600 parsecs star-density was about 60% of that near the Sun; at 1,600 parsecs about 20%; at 4,000 parsecs

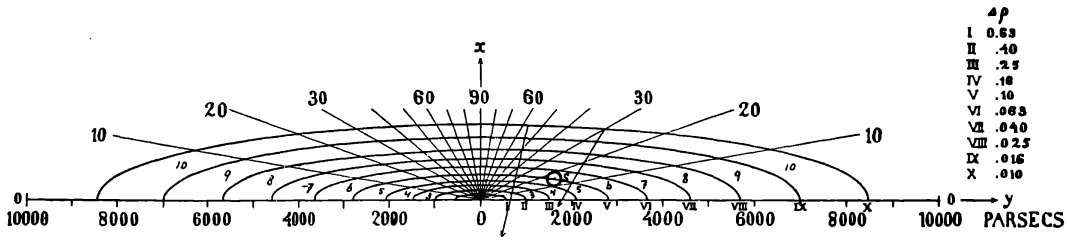

FIG. 3. The "Kapteyn Universe". The ellipsoids of revolution represent lines of equal density surfaces distributed perpendicularly to the plane of the Galaxy. Marginal numbers represent galactic latitudes; roman numerals represent (relative) densities with the centre of the system taken as unity. Note the eccentric location of the Sun. (From J. C. Kapteyn, "First attempt at a theory of the arrangement and motion of the sidereal system", *Astrophysical journal*, lv (1922), 302-28, p. 304.)

only 5%; and at its perimeter, about 9,000 parsecs from the Sun, star-density was less than 1 percent of the solar region. At high galactic latitudes, Kapteyn's results closely represented the observational data. While Kapteyn and van Rhijn still admitted their solution to be no more than "provisional", many others considered their results nearly complete. Additional empirical data would refine only details, and not their general results. R. J. Trumpler and H. F. Weaver in their fundamental book *Statistical astronomy* (1953) were to hail Kapteyn's 1920 theory of the stellar system as "the final achievement of a lifetime of masterly statistical investigations".[64]

As his 1920 system was nearly Sun-centred, Kapteyn was not willing to relinquish the heliocentric assertion readily, despite increasingly stronger evidence from the research of others. The major studies to the contrary came from his nemesis Shapley, who until recently had also been on the Mount Wilson staff. Kapteyn had earlier written Shapley that because the Milky Way appeared to be similar in all directions, he could not accept Shapley's assertion of an eccentric position for the Sun.[65] It was not only Shapley's rejection of the idea of a Sun-centred system that disturbed Kapteyn, but also Shapley's challenge to the reality of Kapteyn's luminosity curves:

> I was much interested in this 'second approximation' [1920]. There are many points I would be glad to discuss with you, particularly the matter of symmetrical luminosity curves. I cannot convince myself that they exist except as the reflection of the combination of many diverse factors.[66]

Between 1920 and Kapteyn's death in 1922, there was a protracted conflict, often face-to-face and certainly 'fact'-for-'fact', between Kapteyn's smaller Sun-centred universe and Shapley's much larger eccentric system. The result of their dispute was the virtual demise of Kapteyn's life-long program, and the increasing acceptance of Shapley's "Big Galaxy", as Shapley liked to call it.[67]

Kapteyn's 1920 model was the culmination of his life's work on the sidereal problem. All the solutions he had provided since his earliest 1901 paper, had followed the same general approach. This model, like the others that had preceded it, is entirely static, and it does not account for large-scale motion of the stars, as represented in his important discovery of star-streaming. Perhaps somewhat anti-climactically, Kapteyn developed in 1922 a dynamical theory of the stellar system, in which he attempted to explain stellar motions in terms of gravitational forces, in the context of his earlier static, density distribution system. In order to account for the shape of his (1920) galactic system through dynamical considerations, Kapteyn assumed that within the plane of the stellar system there is 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. The flattening of the system was accounted for by a sufficient velocity of rotation. Since the two star-streams interpenetrate at a relative velocity of 40 km/sec, the Sun, suggested Kapteyn, must be about 650 parsecs from the centre of the system in order to account for a linear velocity of 20 km/sec. As regards his earlier Sun-centred system, Kapteyn now concluded that "it seems infinitely improbable that the sun is at the center". In this way, as a minor and perhaps even symbolic concession to Shapley, Kapteyn placed the Sun asymmetrically within his new system (see Figure 3).[68]

Combined, Kapteyn's 1920/22 theory of the stellar system came to be known as the "Kapteyn Universe", a term coined by James Jeans, the brilliant English mathematician-turned-astrophysicist.[69] In actuality, however, the 1920 model represented Kapteyn's lifetime achievements in dealing with the sidereal problem, while 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.

Conclusion

Obviously, Kapteyn was not solely responsible for the emergence of statistical astronomy prior to the 'new' astronomy of the 1920s; his contemporary Hugo von Seeliger must be given shared credit for establishing statistical astronomy as a major research field. After the pioneering studies of Kapteyn and Seeliger between 1880 and 1910, others, particularly van Rhijn, Schwarzschild, Eddington and Charlier, began to make significant contributions as well. But Kapteyn, and to a lesser degree Seeliger, continued as leaders in what promised to become an extremely fruitful research field. From the beginning of his statistical studies in the 1890s until the early 1920s, Kapteyn defined and clarified many of the major research problems dominating statistical astronomy.

Not only were problems of substance explored, but new methods were developed, all of which provided grist for the mills of the numerous statistical astronomers. Accordingly, after about 1900 research in statistical astronomy was further characterized by investigations of the correct analytical form of the various relationships, particularly the laws of density and luminosity, in addition to that of velocity, but also the mean-parallax and star-count functions. In this work, they were utterly convinced that an understanding of these relationships would yield universal laws; not just statistical relationships, but laws of nature. 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."[70]

As to Kapteyn, not only was he a gifted scientist, but he managed, as nearly all leaders do, to stimulate international cooperation. We have briefly noted Kapteyn's involvement with Gill's *Cape photographic Durchmusterung*, and 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 highlights Kapteyn's abilities to achieve great success with limited resources. Perhaps the clearest statement of Kapteyn's status in the astronomical community came shortly after Kapteyn received the Bruce Medal from the Astronomical Society of the Pacific in 1913. Referring to Kapteyn's stellar studies, George Ellery Hale 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."[71]

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1. See, for example, M. A. Hoskin, *Stellar astronomy: Historical studies* (Chalfont St Giles, Bucks, 1982); R. W. Smith, *The expanding universe: Astronomy's 'Great Debate', 1900-1931* (Cambridge, 1982); and R. Berendzen, R. Hart and D. Seeley, *Man discovers the galaxies* (New York, 1976). Both Smith and, to a lesser degree, Berendzen have dealt with some issues involving Kapteyn.
2. For a brief survey of Kapteyn's work, see my "Kapteyn and statistical astronomy", in H. van Woerden *et al.*, *The Milky Way Galaxy: IAU symposium no. 106* (Dortrecht, 1984), no. 3. A thorough, blow-by-blow analysis of most of the technical issues dealt with in this paper, including a discussion of the immense amount of primary technical literature, may be found in my *Seeliger, Kapteyn and the rise of statistical astronomy* (unpublished dissertation, Bloomington, Ind., 1976). For a more accessible examination of Seeliger's contributions to our understanding of the stellar universe during this period, see my paper, "H.v. Seeliger and modern stellar astronomy", *Journal for the history of astronomy* (forthcoming).
3. "Bart Bok interview", American Institute of Physics (15 May 1978), 24.
4. Among the unsuccessful candidates was Hugo von Seeliger, Kapteyn's lifelong friendly rival; see J. Schuller tot Peursum-Meijer, "De sterrenkunde voor Kapteyn (1614-1878)", in A. Blaauw *et al.*, *Sterrenkijken Bekeken* (Groningen, 1983), 7-31, p. 28.
5. Kapteyn to D. Gill, 16 December 1885, reprinted in *Cape photographic Durchmusterung*, i (*Annals of the Cape Observatory*, iii (1896)), p. xiii (original letter lost). Also see Gill to Kapteyn, 9 January 1885 and 22 January 1886 (Kapteyn Astronomical Laboratory Archives; hereafter: KAL Archives) in which Gill gladly accepts Kapteyn's generous offer of assistance.

6. F. H. Seares, "J. C. Kapteyn", *Publications of the Astronomical Society of the Pacific* [hereafter: *PASP*], xxxiv (1922), 233-53, p. 233.
7. I am indebted to Michael Hoskin for bringing this information to my attention, and for clarifying the 61 Cygni episode. See Hoskin, *Stellar astronomy* (ref. 1), 5-21, espec. p. 9.
8. G. B. Airy, "On the movement of the solar system", *Memoirs of the Royal Astronomical Society*, xxviii (1860), 143-71; and F. Argelander, "Ueber die eigene Bewegung des Sonnensystems", *Astronomische Nachrichten*, xvi (1839), cols 43-56.
9. T. H. Safford, "On the solar motion in space and the stellar distances", *Proceedings of the American Academy of Arts and Sciences*, xi (1876), 52-61, p. 55 (second paper).
10. M. Hall, "The sidereal system", *Memoirs of the Royal Astronomical Society*, xliii (1876), 157-97, p. 157.
11. T. H. Safford, "On certain groups of stars with common proper motions", *Monthly notices of the Royal Astronomical Society* [hereafter: *MNRAS*], xxxviii (1878), 295-7, p. 295. It was generally admitted, however, that in some cases smaller groups of stars did exhibit preferential motions, a tendency at the time called "star-drifting".
12. J. C. Kapteyn, "Over de verdeeling van de sterren in de ruimte", *Verslagen en Mededeelingen der Koninklijke Akademie van Wetenschappen te Amsterdam Wis- en Natuurkunde Afdeeling*, ix (1892), 418-21; and W.H.S. Monck, "The Sun's motion in space", *PASP*, iv (1892), 75-77.
13. For a survey of the various forms proposed for the 'mean parallax' formula, see R. A. Robb, "The correlation between absolute magnitude, linear tangential velocity, distance, apparent magnitude and proper motion", *MNRAS*, xcvii (1936), 67-75.
14. A. S. Eddington, "J. C. Kapteyn", *The observatory*, xlv (1922), 261-5, p. 265.
15. J. C. Kapteyn, "Over de verdeeling der kosmische snelheden", *Verslagen der Zittingen van de Wis- en Natuurkundige Afdeeling der Koniklijke Akademie van Wetenschappen te Amsterdam*, iv (1896), 4-18, and "Over de verdeeling der kosmische snelheden", *ibid.*, vi (1898), 51-60.
16. J. C. Kapteyn and W. Kapteyn, "On the distribution of cosmic velocities. Part I: Theory", *Groningen publications*, v (1900).
17. Eddington, "J. C. Kapteyn" (ref. 14), 264.
18. While the first indication that a random distribution did not represent the observed motions of the nearby stars was noted by H. Kobold in "Über die Bewegung im Fixsternsystem", *Astronomische Nachrichten*, cxxv (1890), cols 65-72, col. 72, his full analysis was first presented in "Untersuchungen der Eigenbewegung des Auwers-Bradley Catalogs nach der Bessel'schen Methode", *Abhandlungen der Kaiserlicher Leopoldinisch-Carolinischen Deutschen Akademie der Naturforscher*, lxiv (1895), 213-365, and subsequent publications.
19. Kapteyn, "Over de verdeeling der kosmische snelheden" (ref. 15), 57-58, and *idem*, "The determination of the apex of the solar motion", *Proceedings of the Royal Academy of Amsterdam*, ii (1900), 353-62.
20. Kapteyn to Hale, 23 September 1915 (Hale Microfilm Collection of the Mount Wilson and Las Campanas Observatories, Pasadena, California; hereafter: Hale Microfilm).
21. *Ibid.*
22. J. C. Kapteyn, "Star-streaming", *Report of the British Association for the Advancement of Science*, 1905, Section A, 257-65, p. 264. Also see his earlier paper "Statistical methods in stellar astronomy", *International Congress of Arts and Sciences, St Louis*, iv (1904), 396-425, espec. pp. 412-22.
23. Kapteyn, "Star-streaming", 264.
24. Kapteyn to Walter S. Adams, 10 December 1910, and Kapteyn to Hale, "Notes", 17 March 1918 (Hale Microfilm).
25. Kapteyn to Hale, "Notes", 17 March 1918 (italics added); also see Kapteyn to W. S. Adams, 11 November 1912 (Hale Microfilm). For the subsequent theoretical studies of star-streaming, see A. S. Eddington, "The systematic motions of the stars", *MNRAS*, lxvii (1906), 34-63; and Karl Schwarzschild, "Über die Eigenbewegung der Fixsterne", *Nachrichten von der K. Gesellschaft der Wissenschaften zu Göttingen*, (1907), 614-32. Kapteyn first became aware of Eddington's work from Gill; see Gill to Kapteyn, 10 November 1906 (KAL Archives).
26. Kapteyn's interest in star-streaming always remained very strong. Indeed, in his final attempt at a model of the sidereal system, he explained star-streaming in terms of a gravitational attraction within a rotating system of stars.
27. Kapteyn to Hale, 13 May 1915, and 6 August 1915 (Hale Microfilm).
28. Indeed, of the dozen papers Kapteyn published after 1915, six deal with proper motions and

parallaxes. For a short time, there was some question about the validity of Kapteyn's parallaxes. Around 1917 Adams and Stromberg, at Mount Wilson, claimed that Kapteyn's proper motions were erroneous. They based their work on the incorrect assumption that there were no giant and dwarf stars, thus suggesting that the distances (mean parallaxes) were much closer. On this episode, see Kapteyn to Hale, 23 May 1918, 6 October 1918, 7 October 1918, and 1 December 1918, and Hale to Kapteyn, 17 August 1918; and Shapley to Hale, 12 September 1920 (Hale Microfilm).

29. Kapteyn to Hale, 23 September 1915 (Hale Microfilm).

30. Hale to Kapteyn, 4 November 1915 (Hale Microfilm). In their ensuing correspondence on scientific method, Hale gently recommended that Kapteyn read a number of philosophical works on the subject; Hale to Kapteyn, 22 September 1915, 18 October 1915, and 9 March 1916 (Hale Microfilm).

31. Hale to Kapteyn, 4 November 1915 (Hale Microfilm).

32. Kapteyn to Hale, 26 March 1916 (Hale Microfilm). In his letter, Kapteyn noted that he had spoken extensively on the matter of method with "our philosopher Heymans and with Ehrenfest, Lorentz's successor".

33. Seeliger presented the detailed results of his investigations in a variety of major papers, published first in 1898, and again in 1909, 1911 and 1912, and finally in 1920, when he presented his last definitive analysis.

34. Seeliger's first ground-breaking study appeared as "Betrachungen über die räumliche Verteilung der Fixsterne", *Abhandlungen der Mathematisch-Physikalischen Klasse der K. Bayerischen Akademie der Wissenschaften zu München*, xix (1898), 565-629. For additional references see *supra*, ref. 2. For a derivation and solution of Seeliger's "fundamental equation", see my *Seeliger, Kapteyn and the rise of statistical astronomy* (ref. 2), 458-67.

35. See Shapley to H. Kienle, 11 October 1922 (Shapley Archives). Shapley noted that the mathematical abstractness of Seeliger's work hindered many astronomers from understanding Seeliger's contributions better. Also see Kienle to Shapley, 21 September 1922 (Shapley Archives).

36. J. C. Kapteyn, "On the luminosity of the fixed stars", *Koninklijke Akademie van Wetenschappen te Amsterdam. Proceedings of the section of sciences*, iii (1901), 658-89; reprinted with slight corrections in *Groningen publications*, no. 11 (1902), 3-32.

37. *Ibid.*, 670; 14.

38. For a useful analysis of the absorption question, see D. Seeley and R. Berendzen, "The development of research in interstellar absorption, c. 1900-1930", *Journal for the history of astronomy*, iii (1972), 52-64, 75-86, espec. pp. 75-79.

39. G. C. Comstock, "Provisional results of an examination of the proper motions of certain faint stars", *Astronomical journal*, xxiv (1904), 43-49; and J. C. Kapteyn, "Remarks on the determination of the number and mean parallax of stars of different magnitude and the absorption of light in space", *Astronomical journal*, xxiv (1904), 115-23.

40. Kapteyn to Harold D. Babcock, 8 November 1909 (KAL Archives).

41. J. C. Kapteyn, "On the absorption of light in space", *Astrophysical journal*, xxx (1909), 284-317.

42. Hale to Kapteyn, 6 January 1914 (Hale Microfilm). At first the tentative results of Adams and Seares were not published, but communicated by Hale to Kapteyn (who was living in Holland) so that the latter could examine the material and offer his opinion of the results; Kapteyn to Hale, 11 September 1913, and Hale to Kapteyn, 6 January 1914 (Hale Microfilm).

43. Kapteyn to Hale, 12 February 1914 (Hale Microfilm).

44. Hale to Kapteyn, 29 May 1914 (Hale Microfilm). On the recognition of selective absorption as significant, see also Kapteyn to Hale, 6 April 1914, and Hale to Kapteyn, 11 May 1914 (Hale Microfilm).

45. Kapteyn to Hale, 29 March 1914 (Hale Microfilm).

46. Shapley to Hale, 14 November 1912; Hale to Shapley, 26 December 1912 (Hale Microfilm).

47. Shapley to F. Moulton, 7 January 1916 (Shapley Archives). For a review of Shapley's cluster colour studies work, see his "Studies based on the colors and magnitudes in stellar clusters. First part: The general problem of clusters; Second part: Thirteen hundred stars in the Hercules cluster (Messier 13)", *Astrophysical journal*, xl (1917), 118-40.

48. Kapteyn to H. Shapley, 17 August 1915 (Shapley Archives). For similar remarks about Shapley's innovative work, also see Kapteyn to Hale, 7 March 1915 (Hale Microfilm).

49. Kapteyn to Hale, 23 September 1915 (Hale Microfilm).

50. Kapteyn to Hale, 18 February 1908 (Hale Microfilm). J. C. Kapteyn, "On the number of stars

of determined magnitude and determined galactic latitudes", *Groningen publications*, xviii (1908); and *idem*, "On the mean star-density at different distances from the solar system", *Koninklijke Akademie van Wetenschappen te Amsterdam. Proceedings of the section of sciences*, x (1908), 626-35.

51. See Kapteyn, "On the luminosity of the fixed stars" (ref. 36), 670-4, espec. p. 670.
52. Kapteyn to Hale, 21 September 1909 (Hale Microfilm).
53. Kapteyn to Adams, 11 November 1912 (Hale Microfilm).
54. Kapteyn to Hale, 26 March 1916 (Hale Microfilm).
55. H.v. Seeliger, "Die Vertheilung der Sterne auf der nordlichen Halbkugel nach der Bonner Durchmusterung", *Sitzungsberichte der Mathematisch-Physikalischen Klasse der K. Bayerischen Akademie der Wissenschaften zu München*, xiv (1884), 521-48; and *idem*, "Über die Vertheilung der Sterne auf der sudlichen Halbkugel nach Schoenfeld's Durchmusterung", *ibid.*, xvi (1886), 220-51.
56. H. Nort, "The Harvard map of the sky and the Milky Way", *Recherches astronomiques*, vii (1917), 1-117.
57. J. C. Kapteyn and P. van Rhijn, "On the distribution of the stars in space especially in the high galactic latitudes", *Contributions of the Mount Wilson Observatory*, no. 188 (1920), reprinted in *Astrophysical journal*, lii (1920), 23-38, p. 29.
58. J. C. Kapteyn and E. C. Pickering, "Durchmusterung of selected areas between $\delta = 0$ and $\delta = +90°$, Systematic plan", *Annals of the Harvard College Observatory*, ci (1918).
59. Kapteyn to Hale, 17 March 1918 (Hale Microfilm).
60. Hale to Kapteyn, 3 December 1912. Also see Kapteyn's response to Hale, 31 December 1912 (Hale Microfilm), in which Kapteyn generally concurs with Hale. For Gill's concern, see Gill to Kapteyn, 27 March 1907, and Gill to Pickering, 25 October 1912 (KAL Archives), in which Gill chides Pickering for soliciting Kapteyn's assistance. In correspondence with Hale as early as 1905, Kapteyn expressed his devotion to the theoretical side of these questions, but lamented the fact of the paucity of relevant data; see Kapteyn to Hale, 7 May 1905 (Hale Microfilm).
61. Kapteyn and van Rhijn, "On the distribution of the stars in space especially in the high galactic latitudes" (ref. 57), 23-24.
62. See K. Schwarzschild, "Über die Integralgleichungen der Stellarstatistik", *Astronomische Nachrichten*, clxxxv (1910), cols 81-88, espec. cols 85-86.
63. Kapteyn and van Rhijn, "On the distribution of the stars in space especially in the high galactic latitudes" (ref. 57), 34-35.
64. R. J. Trumpler and H. F. Weaver, *Statistical astronomy* (Berkeley, Calif., 1953), 438. Also see G. E. Hale, "Professor Kapteyn's investigations", *Mount Wilson Observatory reports*, no. 19 (1920), 254-5.
65. Kapteyn to Shapley, 15 June 1919 (Shapley Archives). Shapley's assertion of an eccentric Sun was based on the assumption that globular clusters are centrally distributed about the Galaxy.
66. Shapley to Kapteyn, 7 March 1920 (Shapley Archives).
67. For a complete analysis of the Dutch reaction to Shapley's cosmology (spearheaded by Kapteyn and van Rhijn), see my paper "The death of a research programme: Kapteyn and the Dutch astronomical community", *Journal for the history of astronomy*, xii (1981), 77-94.
68. J. C. Kapteyn, "First attempt at a theory of the arrangement and motion of the sidereal systems", *Astrophysical journal*, lv (1922), 302-28. Kapteyn had been working on the gravitational distribution of forces since at least early 1921; see Kapteyn to Hale, 4 April 1921, 30 July 1921, and 3 January 1922 (Hale Microfilm).
69. The results of Kapteyn's latter work had been communicated at the Edinburgh meetings of the BAAS in September 1921. Since Jeans was present, it was known to him prior to publication. Working independently, though relying on much the same data, he reached basically the same conclusions as Kapteyn. See Jeans to Kapteyn, 28 December 1921 (University of Groningen Archives), and J. H. Jeans, "The motions of the stars in a Kapteyn-Universe", *MNRAS*, lxxxii (1922), 122-32. For a discussion of Jeans's support of the "Kapteyn Universe", within his force studies, and implications for the "island universe" theory, see Smith, *Expanding universe* (ref. 1), 104-5.
70. Kapteyn and van Rhijn, "On the distribution of the stars in space especially in the high galactic latitudes" (ref. 57), 33.
71. Hale to Kapteyn, 18 February 1913 (Hale Microfilm).