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# ADDRESS

## *Delivered by the President, Professor W. M. Smart, on the Award of the Gold Medal to Professor Antonie Pannekoek*

As announced at the January meeting of the Society, the Council has awarded the Gold Medal to Antonie Pannekoek for his work in astrophysics and on the structure of the galactic system. It is now my privilege to give a resumé of our Medallist's principal achievements during his long and active life.

It is not given to many to start an astronomical career at 16, even on an amateur basis; at this tender age our Medallist began his studies of the Galaxy, a subject which has engaged his attention almost right up to the present time. Of his other early activities mention should be made, in passing, of his photometric studies of the eclipsing binaries $\beta$ Lyrae and Algol (the latter the subject of his Leiden thesis in 1902) and of the light-curve of Polaris, confirming Hertzsprung's earlier discovery of the variability of this star.

In 1899 we find Dr Pannekoek at Leiden Observatory engaged mainly in meridian circle observations, and from 1906 to 1914 in Germany. During this latter period he became interested, through Kugler's works, in ancient astronomy, tangible results following in the form of two publications, one on Babylonian astronomy and the other on the origin of the Saros. During the same period he began his detailed investigation of stellar distribution and the structure of the galactic system, basing his work at first on counts of stars of given magnitudes taken from the available catalogues.

About this time Kapteyn had given a representation of the stellar distribution in the local system in terms of ellipsoids of revolution. In extending this work, dealing with the northern and southern galactic hemispheres separately, Dr Pannekoek paid greater attention to localized distributions and by separate treatment of stars of different spectral classes he showed that the local system was in effect an irregular agglomeration of star-clouds, amongst which the clouds of the B type stars were notably well defined.

In 1919 our Medallist began his association with the University of Amsterdam, and soon he had founded the Astronomical Institute of Amsterdam after the model of the Kapteyn Laboratory at Groningen; although the Institute was at first equipped with simpler forms of measuring instruments, it was only in 1927 that he acquired a microphotometer for the special photometric studies to be referred to later. The first two volumes of the Institute's publications were devoted to the galactic investigations already mentioned, the first volume including a fine set of density-charts; in the second volume a particularly interesting phenomenon was described, a mutual exclusion effect between the bright B and K type stars in clusters.

Later, a photometric survey of the northern Galaxy by means of extra-focal photography was completed in 1933, the plates being taken by Max Wolf at Heidelberg; the southern half of the Galaxy was similarly studied by means of plates taken at Lembang Observatory in Java; this latter work was published only so recently as 1949.

Our Medallist's astrophysical investigations began in 1922, and were to a great extent inspired by Saha's pioneer paper of the previous year. He first applied Saha's ionization formula to the conditions of radiative equilibrium and investigated the heights of formation of the Fraunhofer lines in the solar atmosphere, later extending this work to the stars. In this work he emphasized the importance of the effect of surface gravity on the relative strengths of arc and spark lines, particularly with reference to the method of spectroscopic parallaxes.

In later investigations he introduced surface gravity as a physical parameter in preference to pressure, the latter being unsatisfactory as a characteristic of stellar atmospheres, owing to its immense variation with depth.

Then followed a period when he was engaged in an extensive programme of measuring relative line-intensities in a wide range of spectral types, using plates supplied by various observatories, including Victoria, at which he later spent six months securing, himself, further observational material. These long investigations contributed greatly to the clarification of the problem of spectral classification, especially as regards the differences between giants and dwarfs.

Meanwhile, theoretical researches continued to be prosecuted by Professor Pannekoek. Turning to the detailed structure of stellar atmospheres, he realized that the departure from thermodynamic equilibrium would have a considerable effect on ionization, and in 1926 he succeeded in establishing the appropriate correction to the Saha ionization formula. A member of the Dutch eclipse expedition to Lapland in 1927, he obtained new material, from observations of the flash spectrum, for investigating the heights and intensities of chromospheric lines, confirming Milne's view that the Fraunhofer lines are formed below the chromosphere.

Our Medallist next turned to the problem of line-profiles, developing his well-known numerical solutions of the equation of transfer for different models of stellar atmospheres. Later, he dealt theoretically with the central intensities of Fraunhofer lines, colour temperatures and the applications of limb-darkening.

In 1935, in collaboration with his pupil Vermeij, he published an important investigation on the Stark effect arising from the electric fields of electrons and ions. This work led him to realize that the disturbance of the highest Bohr orbits of the hydrogen atom by nearby charged particles would prevent quantization of the combined system, so merging the highest members of the Balmer series into the continuum. In a later paper he was the first to establish the relation, since confirmed experimentally, between the last visible member of the Balmer series and the electron and ion densities.

In 1942, Professor Pannekoek was dismissed from his University Chair by the Germans then in occupation of Holland. In the intervening years he has been active in completing several investigations—the latest on " Line-intensities in spectra of advanced type " in *Victoria Publications* (1950)—and in particular, he has been able to devote a considerable part of his enforced leisure to the continuation of his historical studies, the fruits of which are now in course of publication and which bear witness to his keen interest in the relations between science on the one hand and the varied human activities of past and modern society on the other.

To the distinguished role of Gold Medallists is now added the name of one whom the Society delights to honour for his many outstanding achievements. I am sure that the Fellows would like me to convey to Professor Pannekoek our sincere wishes for many fruitful years to come and to assure him of a very warm welcome when he comes in April to deliver the George Darwin Lecture and to receive the Gold Medal, our highest recognition of distinguished service to our science.

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# ADDRESS

## *Delivered by the President, Professor W. M. Smart, on The First Half of the Twentieth Century: a Partial Review*

A few weeks ago we entered the second half of the twentieth century; the present occasion thus seems to be a suitable opportunity for reviewing some of the principal achievements in astronomy during the last fifty years, a period within the lifetime of some of us here today. Obviously it would be impossible within the time at my disposal—even if I were qualified in all departments of our rapidly expanding science—to cover more than a small portion of the immense field of observational and theoretical enquiries which have been pursued with almost unbelievable acceleration notably during the last decade or two. A hundred years ago it was still possible for a scientist—and, for that matter, a man devoted to the humanities—to be familiar with, and even an authority in, more than one of the recognized departments of knowledge. These days have gone, and the most that many of us (at least I speak for myself) can hope to achieve is a reasonably intimate acquaintance with one or two of the many sections into which a great science such as ours is now divided.

I think that there is a tendency at the present time to be too much absorbed by the many speculative aspects of astronomy, however fascinating these may be, and to neglect in our general conspectus much of the patient work in fundamental astronomy carried on quietly and almost unobtrusively in many of the observatories scattered over the globe and on which the whole structure of our contemporary knowledge is reared. Much of what I propose to discuss will be concerned with problems and investigations coming within the second category. It might have been anticipated in 1900 that these problems, many of them of an ancient vintage, would have been solved to our general satisfaction by this time; although it is true that a very high degree of observational accuracy has now been attained in many departments, yet this increased precision has frequently pointed the way to new difficulties of interpretation and suggested new problems for investigation.

At the beginning of the century there were serious misgivings as to the accuracy of the adopted value of the solar parallax, defining the astronomical unit of distance. The value of 10″, derived from diurnal observations of Mars first by Flamsteed, then by Bradley and Pound, and also, just about two centuries ago, from the comparison of observations of Mars made by Lacaille at the Cape of Good Hope with similar observations made at northern observatories, was the first relatively accurate determination of this fundamental constant. The two transits of Venus in 1761 and 1769 provided a variety of results lying between 8″·5 and 9″·5 according to the combination of observations made in different parts of the globe. This lack of precision from what appeared to be a perfect geometrical method arises, as we now know, from the effects of the atmosphere of Venus in rendering uncertain the observed times of ingress and egress of the planet on and from the solar disk. In the *Nautical Almanac* for 1801 the round figure of 9″ was used and this remained the official value for a third of a century, being replaced in 1834 by the value of 8″·5776 derived by

Encke from an elaborate discussion of the two eighteenth century transits. The implied accuracy of this last result is very impressive although, it need hardly be stated, entirely illusory. Encke's value, retained for a quarter of a century, was superseded in 1870 by an indirect determination derived by Le Verrier from the observed value of the parallactic inequality of the Moon; this is a perturbation in longitude, arising from the action of the Sun, with a period of a synodic month and an observed amplitude of about 125″. The theoretical expression for the amplitude involves the ratio of the semi-major axes of the lunar and solar orbits; the former being known, the solar parallax is then readily deduced. Le Verrier's value of 8″·95 remained the standard parallax until 1881. It may be interpolated here that a new determination of the lunar parallax made last year from photoelectric timings of the occultation of stars, each observed at a series of stations in the U.S.A., yields a result (regarded at the moment as preliminary) in terms of the lunar distance, showing an increase of 6 km. over the adopted value.

In 1882 the value of 8″·848 for the solar parallax was introduced; this was a weighted mean derived by Newcomb from a variety of determinations, including a re-discussion of the transit of Venus in 1769, observations of Mars in 1862, the parallactic inequality of the Moon, and the observed value of the constant of aberration in association with Foucault's measurement of the velocity of light: it is of interest to note that the last method gave 8″·860 for the solar parallax, a surprisingly accurate result in view of the difficult practical problem of measuring the velocity of light with the physical resources available at the time. Newcomb's value of the solar parallax survived till the end of the century.

Meanwhile astronomers had not been inactive in pursuing more direct investigations. Gill, in 1877, using a heliometer, the most accurate instrument for this kind of work at the time, and adopting the diurnal method, obtained the value of 8″·78 from a series of observations of Mars carried out on the Island of Ascension: this result, although apparently of high accuracy, suffered from the suspicion that the red colour of the planet introduced systematic effects arising from atmospherical dispersion. A few years later a more determined attack was made by Gill in collaboration with four northern observatories, the objects observed being the three minor planets Iris, Sappho and Victoria; the result was 8″·802. New determinations of the solar parallax from the constant of aberration and the parallactic inequality of the Moon were substantially in agreement; against these Newcomb had meanwhile produced two almost identical values, 8″·762 and 8″·759 derived from gravitational considerations. Although a conference of the superintendents of the four principal almanacs decided in favour of Gill's value of 8″·80 to be used as the official solar parallax from 1901 (a value which is still in use), Newcomb's prestige in the realm of celestial mechanics induced a feeling of incertitude in the adopted value which could only be allayed by a new concerted attack with all the increased resources rapidly becoming available.

The discovery of the minor planet Eros in 1898 occurred opportunely; its orbit was found to be greatly elongated so that at its nearest approach to the Earth it is distant only 14 million miles. The opposition of 1900–1 was seen to be eminently favourable and preparations were immediately set on foot to take advantage of this stroke of good fortune. In 1900, eighteen photographic telescopes were in commission; although the photographic method had yet to prove itself in this exacting field of astrometry, it was nevertheless felt that the new technique would offer very distinct advantages over the visual methods hitherto employed. The reduction of the great mass of observations was entrusted to Hinks at Cambridge, and after about ten years of careful and exacting work the final result for the solar parallax was announced to be 8″·806 ± 0″·004, corresponding to a mean solar distance of 92,900,000 miles.

In addition to determining the solar parallax, the observations furnished material for ascertaining the mass of the Moon, or rather the ratio of the lunar and terrestrial masses. The motion of the Earth around the Sun is rather more complicated than that described simply as elliptic; it is the centre of mass of the Earth-Moon system that moves in this way. Accordingly, the observed positions of Eros include an effect, known as the lunar equation, which depends on the Earth's motion relative to the centre of mass and this involves the ratio of the masses of the Earth and Moon, as well as the solar and lunar parallaxes. The observations furnish the numerical value of the lunar equation; with the observed value of the solar parallax and the adopted value of the lunar parallax, the ratio of the masses is then calculated. Hinks found that this ratio $\mu$ (mass of Earth to mass of Moon) was 81·53 ± 0·047—a result evidently of considerable precision as judged by the probable error.

With $\mu$ known it is then possible, from the theory of precession and nutation and the observed value of the luni-solar precession, to deduce the constant of nutation and the mechanical ellipticity of the Earth (a relation between the moments of inertia about the principal axes of figure); the former was found to be 9″·213, in good agreement with the present observed value, and the latter 1/305.

A much more favourable opposition of Eros occurred thirty years later—the minimum geocentric distance of the planet on this occasion was about 16 million miles, just a little more than half that at the 1900 opposition. Telescopes of longer focal length were now available; photographic technique was greatly improved; further, it was realized that a similar opportunity would not occur for a long time. These were all irresistible arguments for undertaking what was hoped to be a final campaign for determining the solar parallax and for clearing up inconsistencies between observed and derived values of the associated constants. The scale of the operations, placed under the direction of Spencer Jones, is indicated by the fact that no fewer than thirty telescopes were engaged in the observational programme, contributing in all a total of 2847 plates.

When the reductions had been considerably advanced what seemed a fatal blow to the entire enterprise was delivered by Vick of Bergedorf in the suggestion that Eros was an unsuitable body for the determination of the solar parallax. Thirty years before, Hinks had some evidence that the brightness of Eros varied in a period of $2^{\mathrm{h}}\ 38^{\mathrm{m}}$, the inference being that the planet was of irregular shape or of varying reflecting power over its surface; in either event the centre of the photographic image would not correspond to the centre of mass of the planet. It may be added that direct observation has recently shown that the former alternative is certainly true. Vick's suggestion, in greater detail, was to the effect that Eros was actually a complex system consisting of at least three discrete bodies and that consequently the positions of the composite images would include unknown fluctuations of a magnitude not to be tolerated in a precise evaluation of the solar parallax. Happily, an exhaustive investigation of the observational material disposed of this suggested complication. Even now we can almost hear the sigh of relief with which the Astronomer Royal felt encouraged to complete the long investigation. The principal result was the value of 8″·790 ± 0″·001 for the solar parallax, corresponding to the Sun's mean geocentric distance of 93,005,000 miles. The lunar equation led to the value of 81·27 for $\mu$ (the ratio of the Earth's mass to the mass of the Moon). With these values and with the adopted value for the constant of precession, the nutation constant was found to be 9″·227, markedly in excess of the observed values which range closely round 9″·210; amongst these latter may be mentioned the value of 9″·213 derived from the Greenwich observations with the Cookson floating telescope in 1911–1931 and the Pulkova result of 9″·211 derived from 45,000 observations made between 1904 and 1941. If the calculations are reversed, with the constant of nutation assumed to have its observed value, the solar parallax comes out to be 8″·841, a wholly inadmissible value.

It is to be remarked that the theory of precession and nutation is built upon an important hypothesis which sometimes we are in danger of forgetting, namely, that of the perfect rigidity of the Earth; consequently, the *calculated* value of the nutation constant is intimately connected with this hypothesis. The departure from rigidity must evidently be small but it may just account for the lack of reconciliation between the derived and observed values of the nutation constant. In recent years, geophysical studies have begun to shed some light on the internal constitution of the Earth. If the Earth has a fluid core, then it is suggested by Jeffreys that the corrections due to non-rigidity will not have the same effect with respect to the nutation in longitude and to the nutation in the obliquity. It is evident that the task of the practical astronomer is certain to be more exacting than ever in this field if he is to contribute reliable information of the kind contemplated.

The first half of our century witnessed an outstanding event in the realm of celestial mechanics—the completion of E. W. Brown's lunar theory. From Newton onwards nearly every mathematical astronomer of note had made some definite contribution to the difficult problem of ascertaining the motion of the Moon with an accuracy commensurate with the increasing precision of observational technique. In 1900 the lunar tables used in the *Nautical Almanac* were those of Hansen, published as far back as 1857, and they remained in force until 1922. During the second decade the *Connaissance des Temps* used the tables, founded on Delaunay's theory, which were published under Radau's direction in 1911. Brown's monumental investigations, originally undertaken on the advice of Sir George Darwin, in honour of whom our Darwin Lectureship was instituted by Sir James Jeans, have their foundation on two remarkable papers by G. W. Hill in 1877, one entitled " Researches in the Lunar Theory " and the other " On the part of the Lunar Perigee which is a function of the mean motions of the Sun and Moon ". Hill's method involved the use of rectangular coordinates referred to axes moving in the ecliptic with the Sun's mean motion and had some points of affinity, in conception at least, with Euler's second lunar theory, based on axes moving in the ecliptic with the Moon's mean motion. Hill's papers were notable for new mathematical devices, particularly the introduction of infinite determinants for the first time into analysis. Hill's papers may be said to have acted as a kind of signpost pointing the way towards a more perfect lunar theory than had yet been devised; but it required a master of consummate mathematical ability to devote his skill and energy to the long journey ahead. Brown attacked the problem in three stages. In the first—the classical problem of three point-masses—he completed the investigation of the Moon's motion under the perturbative action of the Sun. In the second he treated the actions of the planets arising directly in the usual way and indirectly through their effects on the orbital motion of the Sun. The third stage involved the numerical determination of the constants entering into the final solutions; then followed the computation of the tables, the work being frequently lightened by ingenious mathematical devices. The mathematical theory, published in instalments in our *Memoirs* beginning in 1897, was completed in 1908; the tables were published in 1919 and were first used in the *Nautical Almanac* for 1923. One particular result may be noted—the value of $5''\cdot91$ for the Moon's secular acceleration, with which Adams' value, obtained half a century before and about which there was a long and heated controversy, was in excellent agreement. The observed value of the acceleration is about twice that mentioned, the excess being attributed to the effect of the slowing-up of the Earth's rotation through tidal friction.

Brown's distinguished work received well-merited recognition by the award of our Gold Medal in 1907, the seventh award claimed by lunar theory. It needs no great power of divination on my part to suggest that the perfection of Brown's theory will be a stumbling-block to an award on similar grounds for many years to come.

The last fifty years have seen a large addition to the number of satellites, no fewer than nine having been discovered, if we include Phoebe (Saturn's ninth satellite) found by W. H. Pickering in 1898 although its precise status was only determined later. The most recent accessions are Miranda (Uranus V) and Nereid (Neptune II) both discovered at the new McDonald Observatory in 1948 and 1949 respectively. All of the new bodies are small, probably no more than two or three hundred miles in diameter. It is hardly likely that the known total of 30 satellites represents the entire number of these bodies and, if the new great telescopes are given the necessary opportunities, we can anticipate some further additions to the planetary families. A great deal of the interest in the new satellites arises from the large proportion of retrograde orbits which present a problem of great concern, as well as of difficulty, to the cosmogonist. Three of the new attendants of Jupiter, for example, move in retrograde orbits, a circumstance which shatters the conception held fifty years ago that the Jovian system was, in many respects, a replica on a small scale of the solar system itself. A feature of the retrograde satellites which is almost certainly of real significance in cosmogonic speculations is that in the Jovian and Saturnian systems these bodies are at very great distances from their primaries as compared with their orthodox brethren and consequently more susceptible to extraneous gravitational influences. I shall return to this point later.

At the beginning of the century the solar system was bounded by the orbit of Neptune. With the great increase in instrumental power it was not unnatural to speculate on the possibility of extending the planetary system by new discoveries. One of the principal functions which Lowell had postulated for his new observatory at Flagstaff was an organized search for an extra-neptunian planet. To guide the search Lowell investigated, after the manner of Adams and Le Verrier, the discordances between the observed and predicted positions of Uranus, attributing these to the gravitational attraction of an exterior planet. It may be remarked firstly that Neptune was not considered in this connection as this planet had traversed no more than a third of its orbit since its discovery in 1846, and secondly that the discrepancies alluded to were but a small fraction of those at the disposal of Adams and Le Verrier, the largest residual in longitude being just about 2″. In 1915 Lowell completed his theoretical investigation, the main result of which was the specification of two possible and antipodal positions of the hypothetical planet. Then ensued several years of intense photographic observations, culminating in the discovery, by Tombaugh, of a new member of the solar system—later named Pluto—in January 1930, the official date of discovery being, however, deferred till March 13 so as to coincide with the date of Herschel's discovery of Uranus in 1781. The discovery naturally caused considerable interest and even amazement when it became known that the new planet was found within 6° of one of Lowell's predicted positions; it was at once inferred that the achievements of Adams and Le Verrier had been successfully repeated, despite the fact that the telescopic appearance of Pluto gave every indication that its mass was hardly likely to be sufficient to disturb the orbit of Uranus by observable amounts.

The question as to whether Lowell should or should not be acclaimed the theoretical discoverer of the new planet was quickly settled. In the first place, since the discordances between the observed and predicted positions of Uranus were as a rule no greater than the unavoidable errors of observation, it was highly improbable on general grounds that these residuals bore any real relationship to the perturbations arising from the action of Pluto, and when the discordances were examined more carefully they failed to reveal the characteristics of period normally associated with planetary perturbations. Complete support to this conclusion was furnished through E. W. Brown's application of a criterion to the residuals—based on a simple trigonometrical identity which had previously been suggested for the determination of hidden periodicities associated with a continuous series of observations. The result of this very thorough investigation left no doubt that the discovery of Pluto was entirely fortuitous, in the sense of being wholly unrelated to Lowell's mathematical investigation. It may be added that, by applying the criterion to the residuals as used by Adams and Le Verrier, Brown showed conclusively why their predictions were successful, despite the fact that the true elements of Neptune's orbit differed considerably—notably as regards the semi-major axis—from the predicted values.

A few months ago Kuiper, observing with the new 200-inch telescope on Mount Palomar, succeeded in measuring the angular diameter of Pluto; this proved to be no more than 0″·2, giving a linear diameter of 3600 miles. With any reasonable assumption as to density, the planet's mass is unlikely to exceed one-tenth that of the Earth, from which it may be firmly concluded that the new planet is incapable of producing observable effects on the orbit of Uranus.

Pluto's orbit proved to be greatly elongated—the eccentricity is about a quarter, the largest for the major planets—and inclined at an angle of about 17° to the ecliptic. At perihelion, the planet is approximately at the same distance from the Sun as Neptune, and this fact suggested to Lyttleton the possibility of a close encounter between the two planets when, as a result of planetary perturbations over a long interval of time, the apse of Pluto's orbit will lie in or near the plane of Neptune's orbit. A close encounter, it is shown, cannot result in the ejection of Pluto from the solar family although, evidently, its orbit would be greatly altered. But when a close encounter with Triton (Neptune's retrograde satellite, with a mass equal to that estimated by Kuiper for Pluto) is considered, several interesting possibilities are envisaged, amongst which are the following : (i) Triton's orbit may be changed into a direct orbit by Pluto's influence, the latter body retaining its planetary character; (ii) Pluto may be captured in a direct orbit, Triton's orbit remaining retrograde; (iii) under exceptional circumstances Pluto may be captured by Neptune in a direct orbit, with the reversal of Triton's orbit from retrograde to direct; in this event, Neptune will possess two normal satellites revolving in direct orbits. The last process is clearly reversible in time so that in the far distant past it is possible that Neptune had two normal satellites and, through gravitational action between them, one had its motion reversed and the other was expelled from Neptune's control to acquire the status of an independent planet. As I have remarked earlier, the retrograde satellites present a difficult problem to the cosmogonist; but, at any rate, the investigation just described in outline does offer a suggestion as to one mechanism by which retrograde motions may be produced—with what degree of probability, however, it is impossible to estimate.

One of the interesting theoretical problems of celestial mechanics was that generally known by the name of " Lagrange's three particles ". Lagrange's theorem can be briefly stated in the form which concerns us here, namely, that if three point-masses are placed at the vertices of an equilateral triangle and are projected with appropriate velocities, then they will continue to move under their mutual attractions so as to maintain the equilateral configuration. The question as to the stability of these motions was investigated in 1875 by Routh, who showed that for small displacements from the equilateral vertices the stability of the system was assured provided the masses satisfied a particular criterion. The study of this problem, originally an exercise in dynamical theory unrelated to the celestial scene, continued to be of theoretical interest only until the discovery, in 1906, of a minor planet subsequently named Achilles. The new body, whose longitude at the time of discovery was about 56° ahead of Jupiter, was shown by Berberich to be at a heliocentric mean distance almost exactly equal to that of Jupiter; also, the orbital eccentricity was large, of the order of one-seventh, so that its aphelion distance was about $\frac{3}{4}$ of an astronomical unit greater than the radius of the Jovian orbit. Charlier was the first to notice that the new planet, together with the Sun and Jupiter, provided an illustration in the heavens of the celebrated Lagrangian problem; further, the necessary criterion for stability, associated with small departures from the equilateral configuration, was easily satisfied. Altogether thirteen minor planets with mean distances approximately equal to that of Jupiter are known; twelve have now received the names of the principal protagonists in the Graeco-Trojan war described in the *Iliad*; of these, seven are approximately 60° ahead of Jupiter in longitude and five about 60° behind. It is perhaps unfortunate that the Trojans and the Greeks are not separated according to their relative Jovian longitudes; however, the general name of Trojans is applied to all these bodies without discrimination.

The dynamical theory of these small bodies is more difficult than usual owing to the near equality of their mean angular motions round the Sun. To a first approximation the principal oscillation of a Trojan relative to the appropriate equilateral vertex—known as the libration—has a period of about 146 years, its path relative to the vertex being an ellipse with its major axis perpendicular to the heliocentric radius vector of the vertex. Further, the amplitude of the libration is, for at least one of the Trojans, as much as 20°, so that the perturbed planet is only 40° from Jupiter in longitude at one extremity of the libration ellipse. The high eccentricities and inclinations to Jupiter's orbital plane—the latter as large as 20°—add still more analytical difficulties to a complete theory of the motions.

In normal planetary theory the developments are made, for successive approximations, according to powers of the mass $m$ of the disturbing planet; but in Trojan theory the developments have to be made according to $m^{1/2}$ (in this case Jupiter is the disturbing planet, its mass $m$ being $\frac{1}{1045}$ so that $m^{1/2}$ is about $\frac{1}{32}$). The most exhaustive investigations of the Trojans are those of E. W. Brown, which are built up on two assumptions; first, that the difference between the heliocentric longitudes of Jupiter and a Trojan is always strictly non-secular so that the motion in longitude is represented by oscillations about a point at or near the equilateral vertex; and, second, that the range of these oscillations at any time is not so large as to bring the Trojan within the predominant control of Jupiter, the Sun's attraction then playing only a subsidiary role. Brown's theory has been applied numerically by Brouwer and Eckert to the orbits of Achilles and Nestor respectively.

Any full-scale planetary theory is valid for only a limited time, past or future; in the case of the Trojan theory the limit of validity is very much smaller. Accordingly, the nature of the second assumption suggests some speculations as to the origin and eventual fate of the Trojans. If it should prove that their orbits are becoming more stable, it might appear that these bodies were formerly under the gravitational control of Jupiter, in other words, that they were members of the Jovian family of satellites. On the other hand, if the total effect of planetary perturbations, including those of Saturn in particular, is to reduce the minimum difference between the heliocentric longitudes of a Trojan and Jupiter, then we may contemplate for the far distant future an accession to Jupiter's number of satellites, with direct or retrograde motion according to circumstances. We may even take a further leap and suggest that the Jovian retrograde satellites known at present may be the result of this kind of gravitational interplay in the distant past.

Significant developments are now taking place in the field of planetary orbits with a view to the revision of the current theories which have their basis on the classical investigations of Newcomb in particular. For example, the theory of Neptune is known to be inexact principally because of errors in the adopted values of the elements, these errors arising from the insufficient arc of the planet's orbit that has so far come under observation. In addition, there are minor defects in the mathematical theories requiring clarification and, more important, accurate values for the planetary masses have become a prime necessity. A long-term programme has recently been undertaken by the U.S. Naval Observatory, Yale Observatory and the Watson Scientific Computing Laboratory to treat the planetary orbits by the method of mechanical quadrature using an electronic computer. As we heard from Mr Clemence a few months ago, a step in calculation requiring from 20 to 40 hours by a skilled human computer is performed by mechanical means in approximately two minutes. So far, the work on the five outer planets has been completed for the years 1780 to 1940 and it is hoped to extend the calculations back to 1655 (the year of the earliest observations of the eclipses of the Jovian satellites) and forward to 2050. The completion of this programme, estimated to take up to a maximum of 25 years, will leave, it would seem, no further worlds to conquer in this long familiar field of celestial mechanics. It would be interesting to speculate on the reactions, if we could have them, of Newton, Lagrange, Laplace and the other great masters of gravitational theory, to these new developments; but space and time forbid an exercise of this kind.

The unique system of rings of Saturn has, curiously, inspired comparatively few mathematical astronomers to undertake investigations as to their origin, structure and stability; the mathematical problem is, of course, one of very great complexity and even now is not completely resolved. As far back as 1715 it was suggested by Cassini that the rings were composed of small satellites revolving around the planet in circular orbits; but, apparently, little attention was paid to this surmise. However, when the crepe ring was discovered and the ball of the planet seen through it, Cassini's interpretation received more solid support and was finally accepted when Clerk Maxwell published his classical investigation in 1856 which earned him the Adams Prize at Cambridge, founded in honour of J. C. Adams a few years before. The satellite-constitution of the rings received definite confirmation from Keeler's spectroscopic observations near the end of last century. One of the noteworthy features of the ring-system is the broad Cassini division, evidently a gap avoided by the ring-satellites; the vacuity of the division was confirmed by Ainslie's observations in 1917 when the ring-system passed in front of a seventh magnitude star which was seen undimmed in its passage across the division. It may be remarked that the phenomenon of Cassini's division (and Encke's) was not considered in Maxwell's theory.

The mathematical problem of the constitution of the rings with special reference to the divisions was taken up, just after the first world war, by G. R. Goldsbrough and later still by C. G. Pendse. One important result of a general nature is the recognition of the dissipative actions of Saturn's satellites in defining extensive zones of clearance within which the ring-satellites cannot long remain; one consequence is that the rings can no longer be regarded as a stable system but one subject to slow dispersal into the regions traversed by the planet's satellites. Particular results of the mathematical investigations may be briefly mentioned. The Cassini division represents one result of the perturbing action of Mimas, the satellite closest to the globe of the planet; Dione is mainly responsible for the clearance of ring-satellites up to a distance from Saturn in close agreement with the inner boundary of the crepe ring, the whole extent of which is within the dispersive field of action of Rhea and Titan; the latter satellite will, in course of time, cause the slow dispersal of the bright rings. Mimas is responsible for another effect, namely, the clearance of ring-satellites outwards from a certain distance which is almost exactly that given by the outer boundary of the ring-system. A complete mathematical theory, including the gravitational action of the ring-system itself, would no doubt be able to give some indication as to when Saturn will be deprived completely of its incomparable rings; further—a much more difficult problem probably—it might lead to an estimate of the age of the ring-system, taking us back to what is believed to be the catastrophic event described in terms of the disruption of a Saturnian satellite which ventured, with fatal results to its individuality, within the danger-zone prescribed by Roche's limit. From the little I have said on this topic it will be evident, I think, that Saturn's rings still offer a challenge to the mathematical astronomer.

Perhaps the greatest need of astronomy fifty years ago was the rapid and accurate determination of a large number of stellar distances to form a reliable foundation for the deeper exploration of the galactic system and for investigating the physical properties of the stars. The heliometer in the hands, first, of Bessel and, later, of Gill and others had produced a limited number of successful parallax measurements for the nearest stars; but it was evident that the visual methods ceased to be effective for stars further away than a dozen, or perhaps a score, of parsecs. The introduction of photography into this exacting department of astronomy by Pritchard of Oxford in 1886 foreshadowed better things to come. As a result of immense efforts up to 1900 the parallaxes of about three score of stars had been determined, about half of which were within a distance of ten parsecs from the Sun; probably no more than a third of the total had been measured with a precision that would be regarded as tolerable on present-day standards.

It was almost exactly at the opening of the century that the first comprehensive programme of determining stellar parallaxes by photography was inaugurated by Hinks and Russell at Cambridge with the Sheepshanks telescope specially designed for the purpose. This was followed by Schlesinger's long series of measurements in America, so fruitful in technical advances amongst which perhaps the most notable was the introduction of the method of dependences. The measurement of parallaxes is still one of the staple investigations of some of our observatories, Yale Observatory and the Cape Observatory being the most prolific with over 1800 and over 1600 parallaxes to their respective credits.

One of the early results of the new knowledge of stellar distances was the recognition of the immense diversity of luminosity among the stars, from which followed the separation into two classes of giants and dwarfs. Then in 1914 came Adams' discovery of the spectroscopic method of deriving parallaxes—basically a method of deriving absolute magnitudes—which could be applied to any star, belonging to one of several spectral types, sufficiently bright to register a measurable spectrum irrespective of its distance from the Sun.

The measurement of the parallax of a binary star with a well-determined orbit led, by a simple application of Kepler's third law (in its precise form), to the calculation of the mass of the system and, in several cases, with the aid of observations of a different type, to the evaluation of the mass of each component. The binaries for which there was reliable information as regards both parallax and orbit appeared to show that the masses of these systems were close to twice the solar mass. The process could be reversed; by adopting twice the solar mass as the mass of the binary, the parallax could be deduced with an accuracy under the circumstances, which I need not elaborate, comparable with that attainable by direct measurement of the trigonometrical parallax. Eddington's mass-luminosity relationship later introduced much greater precision into this latter method for, with a preliminary value of the parallax obtained as described, the corresponding absolute magnitudes of the two components are easily derived from the observed apparent magnitudes, the mass-luminosity relationship then giving the mass of each component; with the mass of the binary found in this way the parallax can be re-calculated and the procedure repeated *in toto* if necessary. This method of successive approximations applies only to systems for which orbits have been calculated.

In the case of slow-moving binaries for which only a small arc of the orbit has been observed, a statistical process can be applied which, in the skilful hands of H. N. Russell and Charlotte E. Moore, has yielded a large volume of information. Parallaxes determined in this way are known as dynamical parallaxes.

It is not easy to give a reliable estimate of the total number of parallaxes measured up to date. The Yale list (1935) contained entries for 7534 stars for which trigonometrical or spectroscopic parallaxes or both were known and, in addition, 1234 dynamical parallaxes. In the same year the Mount Wilson spectroscopic parallaxes totalled 4179 and Russell and Moore's list of dynamical parallaxes had 166 entries for binaries with reliable orbits and 2363 entries for slow-moving pairs. To summarize the work of the half-century we may say, allowing for poorly determined measures, that in round figures a grand total of 10,000 parallaxes of reasonable accuracy have been obtained by one or other of the methods described.

A deeper penetration into the galactic system is made possible by means of the absolute magnitudes of stars of different spectral types based, of course, on the ascertained parallaxes of the nearer stars. A complication is introduced by the interstellar cloud but I have no time to discuss this further. The observational and theoretical studies of Cepheid variables has enabled us to explore still further afield—to the globular clusters and the nearer extragalactic nebulae—and rougher methods take us to the farthest bounds of the visible universe.

It is probably true to say that the staple occupation of the majority of astronomers in the last century was the determination of stellar positions with the meridian circle. One obvious incentive was the attainment of increased accuracy in these fundamental observations so that the motions of the planets against the stellar background could be ascertained with the greatest possible precision. A second incentive was found in the determination of proper motions—the angular movements to which the stellar velocities relative to the Sun give rise. An observed proper motion consists of two parts, one resulting from the Sun's own motion with respect to the group of stars concerned and the other resulting from the star's individual velocity with respect to certain axes defined by the group. Until the beginning of our century the only important problem associated with proper motions in bulk was that of the solar motion—or rather its direction in the heavens, for it was only when radial velocities became available that the Sun's speed, relative to the group of stars concerned, could be found. The assumption underlying the determination of the direction of the solar motion was the simple one of the random character of the individual velocities as defined; the procedure consisted in averaging the proper motion components of the stars within a particular region of the sky in the expectation that the random motions would cancel out, leaving a reliable value of each component which would then represent the true effect of the solar motion. The combination of such information from different parts of the sky—not necessarily the whole sky—then yielded the direction of the solar motion or, in other words, the coordinates of the solar apex. Although a large number of astronomers of repute analysed the observational data available to them for this purpose, it seems rather remarkable that no test of the fundamental hypothesis relating to the randomness of the stellar motions appears to have been undertaken until 1904, when Kapteyn discovered the phenomenon of the two star-streams, a discovery which may be said to have inaugurated the study of a much wider problem, namely, the dynamics of the galactic system itself.

If the individual motions of the stars in a small part of the sky were simply random in character, the distribution of the observed proper motions in position angle ought to be represented by a particular member of a family of oval curves, its axis pointing in a definite direction depending on the projection of the solar motion on the area of the sky concerned, and its relative dimensions depending on the angular distance of the region from the solar apex. Kapteyn's discovery consisted, first, in noticing that no such simple curve represented the observed distribution, so that the hypothesis of randomness in the original form must perforce be abandoned, and, second, that the observed distribution appeared to be the combination of two oval curves, one pointing to a particular apex and the other to a second apex, these apices being determined from the information supplied by several regions of the sky. Kapteyn's interpretation was in terms of two swarms, or streams, of stars each with its own motion in space.

The immediate developments were due to Eddington who, introducing Maxwell's law as a criterion of randomness for the stars of each stream, devised an effective method for the analysis of the stream motions which he applied in 1910 with great success to the 6000 proper motions of the naked-eye stars of Boss's *Preliminary General Catalogue*, just then published. I mention two of the principal results; first, when the solar motion was allowed for, the streams appeared to move in opposite directions (really a consequence of the definition of the solar motion) lying almost exactly in the galactic plane; second, the populations of the streams were in the ratio 3:2. The first result is expressed in terms of the vertex of star-streaming, towards or away from which the streams are moving; the longitude of the vertex was found to be 347°. Evidently the line joining the vertex to its antipodal point was of the utmost significance in relation to the general structure of the galaxy; but just what it signified in a spheroidal system with the Sun supposed, at that time, to be centrally situated was not elucidated for a decade and a half.

An alternative interpretation of the systematic motions of the stars was offered by Schwarzschild in the ellipsoidal hypothesis, according to which it was postulated that the stars had a greater mobility in the to-and-fro directions defined by the line of vertices than in any perpendicular direction. Analytically, the representations of the two conceptions are very much alike and so far they are practically indistinguishable in their applications to the observations provided that the populations of the two streams are equal. As we have seen the Boss stars gave the relative populations in the ratio 3 : 2; but when the proper motions of faint stars, derived photographically, are examined the disparity virtually disappears. At this point I may add that the proper motions of the *General Catalogue*—about 33,000 in number and including all magnitudes to the ninth—show a significant diminution of the ratio to 6 : 5. It seems now to be reasonably well established that, in the much greater volume of space surveyed for the proper motions of stars to the twelfth or fainter magnitudes, the relative populations of the streams are identical. This is in general accordance with theoretical considerations, for in terms of galactic rotation one stream simply represents those stars whose orbital motions are directed towards the hemisphere of which one vertex is the pole, and the second stream towards the other hemisphere. From this point of view the separation of the stars into two streams may then be regarded as a convenient analytical device to represent the more general conception inherent in the ellipsoidal hypothesis which, in the much more comprehensive problem of galactic dynamics, we assume to be characteristic of the stars in all parts of the galactic system.

There are various arguments for believing that the Galaxy is not in a steady state and in this connection there is one observational result of great significance emerging from nearly every statistical study of systematic motions either according to the two-streams technique or to the ellipsoidal theory. For a system, symmetrical spatially and dynamically, we should expect the direction of the vertex as viewed from the Sun's excentric position to be identical with that of the galactic centre. Now, the distribution of the globular clusters and of faint stars remote from the galactic equator, the direct observations made recently by Stebbins to which I alluded here a year ago, and other considerations, all seem to be unanimous in placing the galactic centre at or near longitude 325°. The observational result referred to is that the longitude of the vertex is nearly 20° higher. Without any further discussion as to the dynamical implications, I venture to take this opportunity of drawing your attention to the most recent determination of the vertex just obtained from an analysis of the 33,000 proper motions of the *General Catalogue* by Dr T. R. Tannahill at the University Observatory, Glasgow; for all the stars considered, the longitude of the vertex turns out to be 343°, thus providing confirmation of the deviation referred to.*

It may be added that the equatorial coordinates of the solar apex are found to be (273°, +35°).

There is one matter of fundamental importance in connection with the subject I have just been discussing and that is the value of the constant of precession. The proper motion of a star recorded in a catalogue is derived from meridian observations separated by as long an interval as possible, and one necessary item of information is a reliable value of the precessional constant. The adopted value of this constant is that derived by Newcomb at the request of the conference of almanac superintendents in 1896. If, then, the value of the constant is accurate, the derived proper motion of a star will be ideally exact although in practice we must recognise that it will be subject to the accidental errors of observation. If, however, the precessional constant requires correction—I omit, for simplicity, consideration of the corrections for certain other factors that enter into the complete problem—then the deduced proper motion will contain a systematic effect. It now transpires, as we shall see in a moment, that the outstanding error in the precessional constant is anything but negligible and, in fact, for a goodly proportion of stars in a standard catalogue it produces inaccuracies in the proper motions which are a substantial fraction of the proper

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* For further details, see p. 194.

motions themselves and are, moreover, of a systematic nature. Each component of proper motion contains, of course, the systematic effect arising from the solar motion, the balance between the observed and parallactic motion being ascribed partly to the star's random motion, the errors of observation and the precessional error mentioned.

Newcomb's investigation was based on a pair of equations for each star, connecting the observed components of proper motion in right ascension and declination with the three components of the solar motion (regarded as unknowns) together with the unknown error of the precessional constant. It is significant of Newcomb's thoroughness of outlook that he contemplated adding to his equations appropriate terms to take account of the possible revolution of the stellar system; this was, of course, long before galactic rotation was anything more than a mere speculation. Newcomb's separate solutions in right ascension and declination were not as accordant as had been hoped—one contributory factor was the neglect of the systematic rotational terms; however, a weighted mean of $0''{\cdot}82$ was taken to represent the correction to the centennial constant of precession. With the new value of the constant the values of the general precession and the luni-solar precession were tabulated at intervals of 25 years from 1725 to 2000—with these, it should be added, are associated secular terms of reliable accuracy. This tabular representation forms the basis for the computed places of the stars—and their proper motions—in the ephemerides.

The foregoing method of deriving the constant of precession, and the associated constants, is now being supplemented by new methods based on planetary orbits. The principal requisite is an accurate theory of the planet concerned, either on traditional lines or by mechanical integration, and this involves an exact knowledge of the individual masses of the planets for evaluating the perturbations produced on the planetary orbit selected. Investigations along these lines have already been undertaken, notably in the U.S.A., giving an additional correction of about $+0''{\cdot}8$ to Newcomb's value of the centennial precessional constant; the accuracy of this result is, however, not particularly high.

As has already been remarked, Newcomb's basic equations are incomplete, for they do not take account of the systematic effects of the galactic rotation on the components of proper motion. A quarter of a century ago, Oort attacked the problem of the precessional constant in a novel way, the data used being the observed proper motions of stars lying in or near the galactic equator. The effects of the solar motion (assumed known) were first removed and the resultant components of proper motion in galactic latitude deduced. Such a component will of course contain the effect arising from the error of the precessional constant and, in addition, that arising from the star's component of velocity perpendicular to the galactic plane. According to the simple rotational theory, these velocity components are accounted random in character so that their effects on the components of proper motion in galactic latitude can be regarded, statistically, as of the same class as accidental errors. An equation then can be derived connecting the precessional error with these components. Oort's original solution showed that the correction to the precessional constant amounted, centennially, to $+1''{\cdot}13$, with a probable error of about one-ninth of this value. The more likely value favoured today is somewhat smaller and not greatly different from that derived from the planetary investigations mentioned earlier.

It is hardly necessary to stress the importance of an accurate determination of the precessional and allied constants in all investigations involving proper motions and particularly in the problem of galactic rotation. The current theory of galactic rotation may be succinctly defined in terms of the two constants A and B introduced by Oort; the former is derived directly from the radial velocities of the distant O and B type stars; the components of proper motion in galactic longitude involve both constants. Apart from the need of increased accuracy in meridian observations over a sufficiently long interval, the principal desideratum is an accurate evaluation of the precessional constant. It might seem that, if we depend only on proper motions as in Newcomb's investigation, we shall have to wait for some considerable time before this essential information is at our command, ready to be applied in elucidating more accurately than is possible at the present time the dynamical characteristics of the stellar system.

In 1947 there appeared the first papers—in *Monthly Notices*, **107**—on a new branch of our science, radio astronomy, which was also the subject of a special article by Dr Hey in the Report of Council at our anniversary meeting two years ago. Although the initial discoveries were made just before the second world war the rapidity of recent progress can be ascribed, in the first instance at least, to the well-known developments in radar technique during the period of hostilities. It must be a peculiar satisfaction to the Society that in the van of discovery in what to some of us is an occult art there are many of our own Fellows. The earlier investigations were concerned mainly with meteors; now the focus of interest seems to have moved to the depths of interstellar and extragalactic space, where new mysteries have been detected offering to our best minds a challenge for their elucidation and interpretation in the years to come.

It has long been known that the principal meteor showers are connected with periodic comets, the inference being that these humble particles of cosmic matter represent the cometary debris dispersed through planetary perturbations. As regards the sporadic meteors the chief problem is that relating to their origin. Are they to be regarded as belonging to the solar system or do they come from interstellar regions? The criterion is one of velocity; in a head-on collision an observed velocity exceeding 72 kilometres per second would undoubtedly point to an interstellar origin; for other kinds of impact there are corresponding critical velocities. Until fairly recently there seemed to be an acute divergence of opinion as to meteoric origins. On one hand Öpik's observations during the Arizona expedition in 1931–33 and the Tartu observations of 1934–38 appeared to show that at least 60 per cent of several hundreds of sporadic meteors had hyperbolic velocities. On the other hand, Whipple's photographic observations and Porter's exhaustive analysis of British meteor data failed to reveal any meteors of interstellar origin; as the new radio observations are entirely confirmatory of the second results, the balance of evidence is now strongly in support of the contention that meteors, of all kinds, are intrinsically members of the solar family.

The radio method of meteor observations is based on the fact that the trail of electrons, produced by a cosmic particle rushing at high speed through the upper reaches of the atmosphere, is capable of reflecting an incident radio-wave giving rise to a short-lived response which can be detected by a suitable receiver. The measurement of meteor velocities was first achieved with radio apparatus by Hey, Parsons and Stewart in 1947; since then a new technique has been

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developed based on the diffraction of radio-waves from the meteor's electronic trail, analogous to the familiar phenomenon of the diffraction of light at a straight edge. It is calculated that for a bright meteor the number of electrons torn from the atmospheric molecules is of the order of a billion per centimetre of the meteor's visible path. Reliable estimates of the sizes of these small bodies appear to show that their diameters are of the order of one-hundredth of a centimetre—not far removed from the size of a grain of sand, the traditional, if speculative, answer to the question : " How big is a meteor ? "

Radio astronomy is independent of climatic conditions and can be pursued as well in daylight as in the long watches of the night. It is perhaps some recompense for the notoriously inclement weather of Manchester, as commonly alleged, that some of the most important developments in radio astronomy are now taking place at Jodrell Bank nearby, under the direction of Dr Lovell of the University Physics Department. Just after the end of the war, daylight observations revealed intense meteor activity, and up to date Lovell and his associates have clearly recognized about a dozen different streams, one of which appears to be associated with Encke's comet. A remarkable fact connected with the daylight streams so far fully investigated is that their orbits all lie within the orbit of Jupiter and are unrelated to any known short-period comet with, of course, the exception of Encke's. The suggestion has been made by Lovell that these streams appear to be related to the orbits of the minor planets, in which event the meteors are but the fine debris of whatever cataclysm produced the minor planets. On the other hand, if meteors are generically part of the debris of comets, the daylight streams may then be the remaining evidence of comets long defunct.

The first suggestion can be discussed in greater detail. When Olbers discovered the second minor planet he suggested that the two small bodies Ceres and Pallas were fragments of a larger planet disrupted in some kind of celestial explosion, a speculative and, to us nowadays, an entirely improbable event. That the asteroids, of which Baade estimates that about 40,000 are within the compass of the great telescopes, have a common origin is still an article of astronomical faith. The idea of an explosion of a large planet can be summarily dismissed, and the fracture of such a planet through too rapid rotation is improbable. As Jeffreys has shown, tidal action as a disruptive agent can also be ruled out. According to Kuiper the only phenomenon that can account for the asteroidal family is a collision of two small planets in the distant past and the successive mutual collisions of the fragments. In support, many of the asteroids are known to be of irregular shape—Eros, as we have seen, is a notable example. A recent estimate by Schuette of Munich puts the total mass of the asteroids at rather more than one quarter that of the Moon; even if we assume that Ceres, Pallas and one or two others have escaped collisions, there is evidently plenty of material left to have originally constituted two or three small planets each with a diameter of the order of perhaps 800 miles. The daylight streams apparently fit into this collision-hypothesis.

We now come to one of the most remarkable discoveries of recent years. It has been known for some time that the Sun emits radio waves, by what mechanism appears to be unresolved at present, and that there is a genera contribution of the same nature from the Galaxy itself with the greatest intensitie from the Milky Way regions, the maximum being in the direction of the galacti centre. One important conclusion seems to be that, even if every star in the Galaxy is an exact replica of the Sun in this respect, the sum-total of the effects is only a negligible fraction of the general galactic emission. In 1945 Hey detected strong radio-emission from a restricted region in Cygnus; more recently, Bolton and Stanley in Australia and Ryle and Smith at Cambridge have found that with increased resolving power the radio waves appeared to come from comparatively minute areas of the sky generally remote from bright stars or from such objects as the dark or luminous nebulae. One notable exception places the seat of emission in the direction of the Crab Nebula, but whether this is actually located in the nebula itself or somewhere in the foreground is still to be settled.

An inference from these striking discoveries is that the emissions emanate from discrete bodies to which the appropriate name of "radio stars" has been given and of which about a hundred have so far been detected. A further deduction appears to follow, namely, that the general galactic emission already noted is an integrated effect from these individual sources.

A few months ago the radio astronomers at Jodrell Bank carried out a series of very remarkable observations— for information on this and other allied matters I am greatly indebted to Dr Lovell. The tower of the great 220-foot paraboloid is vertical and so only zenithal observations are normally possible. Hanbury Brown suggested that if the structure could be tilted 14° from the vertical in the meridian the Great Nebula in Andromeda would come within the sphere of observation. This difficult operation was safely accomplished and with new receiving apparatus of greatly increased sensitivity, radio emissions from the nebula were successfully observed. Our Galaxy is then not unique as regards these new radio phenomena and I do not think that I would be accused of rashness if I suggest that the extragalactic nebulae in general have within them sources, similar to those in our Galaxy, radiating low-frequency electromagnetic waves far beyond the red end of the familiar optical spectrum.

The principal problem to be solved is, of course, concerned with the physical processes operating in the discrete bodies envisaged and producing the type of radiation observed. To the orthodox astronomer one problem relates to the number and distribution of the radio stars. The discovery of such a large number of these objects with apparatus evidently just on the threshold of sensitivity would seem to lead to the conclusion that radio stars are very numerous in our Galaxy. Are they as numerous as the lucid stars and have they a similar galactic concentration? The answers to these questions will be awaited with the greatest interest.

If radio stars are essentially nuclei of matter with masses of the same order of magnitude as those of ordinary stars, their gravitational effects must be included in those that are observable as galactic rotation. Until the advent of these new bodies the best available information indicated, for a first approximation at least, that the galactic system consisted of a nucleus of $10^{11}$ stars of average solar mass together with a spheroidal distribution of nearly equal aggregate mass, the latter including the interstellar clouds. The determination of the relative distribution of lucid stars, radio stars and interstellar matter provides a new problem to investigators of the material and dynamical characteristics of the Galaxy, a problem in which the skill and the patience of the practical astronomer will be called upon perhaps as never before.

Fifty years ago the exploration of the Universe beyond the confines of the solar system had scarcely begun and had certainly made but modest progress. A quarter of a century ago we were still discussing in formal debate whether or not the spiral nebulae should be regarded as constituent members of the galactic system. Today we have before us a vast accession of knowledge in many departments of our science but nevertheless we are still confronted with problems of ever-increasing complexity. If my successor in the presidential chair in 2001 adopts as the subject of his anniversary address "A review of the past half-century"—an occasion at which many of us would give a lot to be present—he will no doubt refer to the little that was really known in 1951 as compared with the immense advances made in his life-time. But will he not have to confess that new and urgent problems, of which we do not have now the smallest inkling, are still rising above the scientific horizon demanding elucidation and interpretation? The history of science would appear to answer: "Yes".