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JOINT ECLIPSE MEETING OF THE ROYAL SOCIETY AND THE ROYAL ASTRONOMICAL SOCIETY.

1919, November 6.

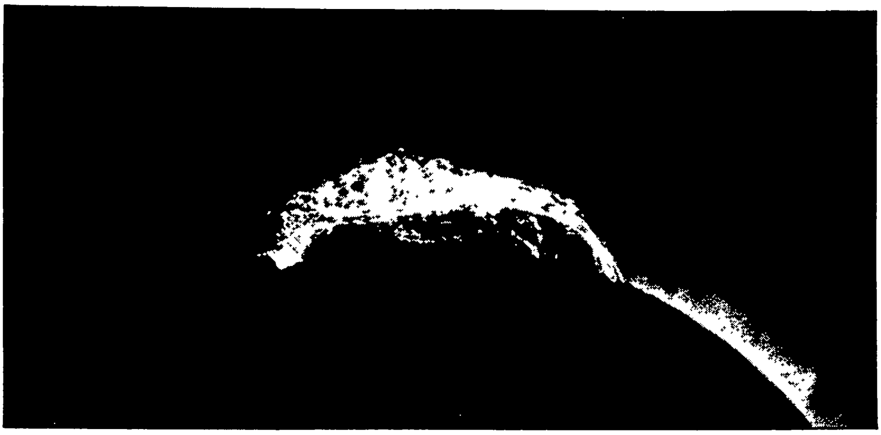
Sir Joseph Thomson, O.M., P.R.S., in the Chair.

The President. I will call on the Astronomer Royal to give us a statement of the result of the Eclipse Expedition of May last.

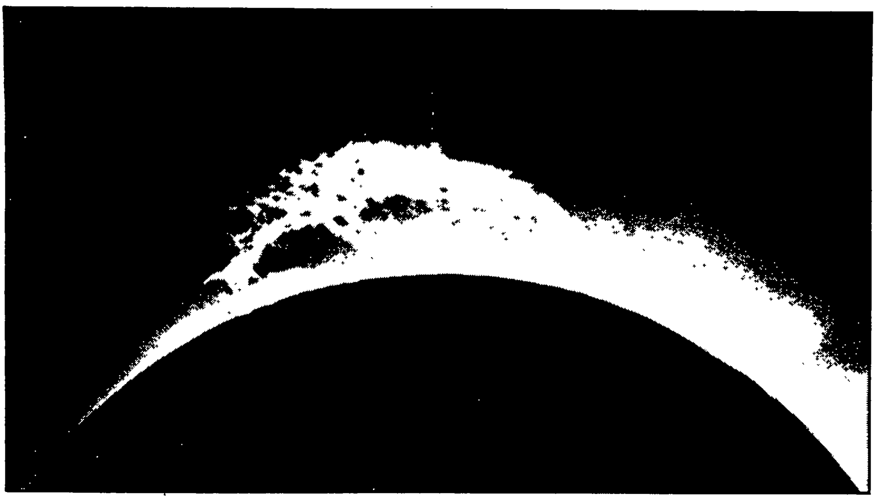
The Astronomer Royal. The purpose of the expedition was to determine whether any displacement is caused to a ray of light by the gravitational field of the Sun, and, if so, the amount of the displacement. Einstein's theory predicted a displacement varying inversely as the distance of the ray from the Sun's centre, amounting to $1''\cdot75$ for a star seen just grazing the Sun. His theory or law of gravitation had already explained the movement of the perihelion of Mercury—long an outstanding problem for dynamical astronomy—and it was desirable to apply a further test to it. Many people considered it quite likely that, even if Einstein's conclusion was not confirmed, we should get half his computed deflection for a beam—this other result being the deflection of a particle moving past the Sun with the velocity of light.

The effect of the predicted gravitational bending of the ray of light is to throw the star away from the Sun. In measuring the positions of the stars on a photograph to test this displacement, difficulties at once arise about the scale of the photograph. The determination of the scale depends largely upon the outer stars on the plate, while the Einstein effect causes its largest discrepancy on the inner stars nearer the Sun, so that it is quite possible to discriminate between the two causes which affect the star's position.

Previous eclipse photographs are generally unsuitable for evidence bearing on the point, as they are either on too large a scale showing too few stars on the plate or else on too small a



Sobral, Brazil. 1919 May 29^d 0^h 1^m 48^s G.M.T.



Principe. 1919 May 29^d 2^h 13^m 28^s G.M.T.

Large Solar Prominence of 1919 May 29.

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scale to provide the delicate test with sufficient accuracy. The plates secured at Sfax in 1905 with one of the astrographic objectives used for the International *Carte du Ciel* seemed of suitable scale. Examination of them failed to give a definite result, but showed that this instrument was well suited to our problem. A study of the conditions of the 1919 eclipse showed that the Sun would be very favourably placed among a group of bright stars—in fact, it would be in the most favourable possible position. A study of the conditions at various points on the path of the eclipse, in which Mr. Hinks helped us, pointed to Sobral, in Brazil, and Principe, an island off the West Coast of Africa, as the most favourable stations, and the eclipse committee decided to send out expeditions to these two places if the war conditions allowed. Prof. Turner, of the Oxford University Observatory, most kindly loaned the object-glass of the Oxford astrographic telescope, and the arrangements for mounting this and the Greenwich objective were pushed forward at Greenwich as hard as the reduced staff permitted. Father Cortie further suggested the use of a 4-inch lens of 19 ft. focal length belonging to the Royal Irish Academy. The instruments were assembled at Greenwich largely under Mr. Davidson's supervision, and all was ready in time for the observers to start from England in March.

The Greenwich party, Dr. Crommelin and Mr. Davidson, reached Brazil in ample time to prepare for the eclipse, and the usual preliminary focussing by photographing stellar fields was carried out. The day of the eclipse opened cloudy, but cleared later, and the observations were carried out with almost complete success. With the astrographic telescope Mr. Davidson secured 15 out of 18 photographs showing the required stellar images. Totality lasted 6 minutes, and the average exposure of the plates was 5 to 6 seconds. Dr. Crommelin with the other lens had 7 successful plates out of 8. The unsuccessful plates were spoiled for this purpose by clouds, but show the remarkable prominence very well.

When the plates were developed the astrographic images were found to be out of focus. This is attributed to the effect of the Sun's heat on the cœlostat mirror. The images were fuzzy and quite different from those on the check-plates secured at night before and after the eclipse. Fortunately the mirror which fed the 4-inch lens was not affected, and the star-images secured with this lens were good and similar to those got by the night-plates. The observers stayed on in Brazil until July to secure the field in the night sky at the altitude of the eclipse epoch and under identical instrumental conditions.

The plates were measured at Greenwich immediately after the observers' return. Each plate was measured twice over by Messrs. Davidson and Furner, and I am satisfied that such faults as lie in the results are in the plates themselves and not in the measures. The figures obtained may be briefly summarized as follows:—The astrographic plates gave $0''.97$ for the displacement at the limb when the scale-value was determined from the plates themselves, and 1″·40 when the scale-value was assumed from the check-plates. But the much better plates gave for the displacement at the limb 1″·98, Einstein's predicted value being 1″·75. Further, for these plates the agreement between individual stars was all that could be expected. The following table gives the deflections observed compared with those predicted by Einstein's theory:—

No. of star.	Displacement in R.A. Observed.	Displacement in R.A. Calculated.	Displacement in Dec. Observed.	Displacement in Dec. Calculated.
	″	″	″	″
11	−0·19	−0·22	+0·16	+0·02
5	−0·29	−0·31	−0·46	−0·43
4	−0·11	−0·10	+0·83	+0·74
3	−0·20	−0·12	+1·00	+0·87
6	−0·10	+0·04	+0·57	+0·40
10	−0 08	+0·09	+0·35	+0·32
2	+0·95	+0·85	−0·27	−0·09

After a careful study of the plates I am prepared to say that there can be no doubt that they confirm Einstein's prediction. A very definite result has been obtained that light is deflected in accordance with Einstein's law of gravitation.

Dr. Crommelin. I have not much to add to what the Astronomer Royal has said, but I should like to say what a great debt we owe to the Brazilian Government for the immense help they gave us. Dr. Morize, the Brazilian national astronomer, gave all possible assistance; he had made a preliminary visit to Sobral a month before, when he made arrangements for our accommodation and also for supplying us with all the labour that we required—porters, bricklayers, and carpenters were all freely put at our service. Members of Dr. Morize's staff helped by supplying us with chronometer errors and meteorological data. We were much indebted to Col. Vicente Saboya, the deputy for Sobral, who put his house at our disposal, with a permanent water-supply—no small boon in a time of drought, and of great importance in the photographic work. Dr. Leocadio Aranjo, our interpreter, gave us invaluable help at every point, clearly explaining to the workmen our complicated demands, and calling the seconds for us at the eclipse. We were also indebted to the Booth Steamship Company for much help in dealing with our heavy baggage. They made arrangements with the local companies to forward it free of charge from Para to Camocim and thence to Sobral.

We should also thank the Sobral municipal authorities, who allowed us to encamp on the race-course and kept the public outside during the eclipse.

With regard to the bad focus of the plates taken with the astrographic during totality, we can only ascribe this to a change of curvature of the cœlostat mirror, due to the Sun's heat; for the focus was good on the stars two days before the eclipse and again on the check-plates taken during July. The small cœlostat used with the 4-inch lens did not suffer from deformation, the images of stars during totality being of the same character as those on the check-plates; this increased the weight of the determination with that instrument. With regard to the July plates, we found that exposure was possible up to 25 minutes before sunrise, when the sky was of about the same brightness as during totality. $39\frac{1}{2}°$ was the greatest altitude of the field on the check-plates, as compared with 44° at the eclipse.

Prof. Eddington. Mr. Cottingham and I left the other observers at Madeira and arrived at Principe on April 23. We were most kindly received at Principe by Sr. Carneiro. He also supplied us with all the labour and materials we needed, and we established our station at Sundy, the headquarters of his plantation, on the N.W. side of the island. The island of Principe is about 10 miles long by 4 miles wide. We soon realised that the prospects of a clear sky at the end of May were not very good. Not even a heavy thunderstorm on the morning of the eclipse, three weeks after the end of the wet season, saved the situation. The sky was completely cloudy at first contact, but about half an hour before totality we began to see glimpses of the Sun's crescent through the clouds. We carried out our programme exactly as arranged, and the sky must have been a little clearer towards the end of totality. Of the 16 plates taken during the five minutes of totality the first 10 showed no stars at all; of the later plates two showed five stars each, from which a result could be obtained. Comparing them with the check-plates secured at Oxford before we went out, we obtained as the final result from the two plates for the value of the displacement at the limb $1''\cdot6 \pm 0''\cdot3$. The p.e. was determined from the residuals of individual stars. This result supports the figures obtained at Sobral.

There was one important difference in our data—we were unable to stay to take check-photographs of the field. As our eclipse took place in the afternoon, we should have had to wait some months longer than the Sobral observers to get the comparison-plates under the same conditions. We, however, took another field of stars for a check and compared our photographs with the Oxford plates of the same field to see whether a similar reduction gave evidence of any displacement corresponding to that found on the eclipse-plates. We got a very small value for the displacement on these check-plates, leading to the conclusion that the larger quantities found on the eclipse-plates could only be due to the presence of the Sun in the field. We also used these check-plates to determine the difference of scale of the photographs at Oxford and Principe, and used that scale for working up the eclipse-plates. This was a great help in making the most of a small amount of material. A difference might have arisen for reasons of temperature changes; but the temperature at Principe is very uniform day and night—in fact, there was not 4° difference during the whole time we were at Principe, and we were there both for the hot and the cold season. Again, in one way we were helped by the clouds that at the time seemed so serious an obstacle; the Sun's rays could not seriously affect the mirror by heating it, as seems to have happened at Sobral.

I will pass now to a few words on the meaning of the result. It points to the larger of the two possible values of the deflection. The simplest interpretation of the bending of the ray is to consider it as an effect of the weight of light. We know that momentum is carried along on the path of a beam of light. Gravity in acting creates momentum in a direction different to that of the path of the ray and so causes it to bend. For the half-effect we have to assume that gravity obeys Newton's law; for the full effect which has been obtained we must assume that gravity obeys the new law proposed by Einstein. This is one of the most crucial tests between Newton's law and the proposed new law. Einstein's law had already indicated a perturbation, causing the orbit of Mercury to revolve. That confirms it for relatively small velocities. Going to the limit, where the speed is that of light, the perturbation is increased in such a way as to double the curvature of the path, and this is now confirmed.

This effect may be taken as proving Einstein's *law* rather than his *theory*. It is not affected by the failure to detect the displacement of Fraunhofer lines on the Sun. If this latter failure is confirmed it will not affect Einstein's law of gravitation, but it will affect the views on which the law was arrived at. The law is right, though the fundamental ideas underlying it may yet be questioned.

The difference of the two laws may be expressed analytically as follows:—Any particle or light-pulse moves so that the integral of ds between two points of its path (in four dimensions) is stationary where

$$ds^2 = -(1-2m/r)^{-1}\,dr^2 - r^2d\theta^2 + (1-2m/r)dt^2 \text{ (Einstein's law).}$$

$$ds^2 = \quad -dr^2 - r^2d\theta^2 + (1-2m/r)dt^2 \text{ (Newton's law).}$$

These expressions are in polar coordinates for a particle of gravitational mass m. I think the second expression may be accepted as corresponding to Newton's law—at any rate, it gives no motion of perihelion of Mercury and the half-deflection of light. What we have established is the necessity for the factor multiplying dr^2.

One further point must be touched upon. Are we to attribute the displacement to the gravitational field and not to refracting matter round the Sun? The refractive index required to produce the result at a distance of 15′ from the Sun would be that given by gases at a pressure of 1/60 to 1/200 of an atmosphere. This is of too great a density considering the depth through which the light would have to pass.

The President. I now call for discussion on this momentous communication. If the results obtained had been only that light was affected by gravitation, it would have been of the greatest importance. Newton did, in fact, suggest this very point in the first query in his 'Optics,' and his suggestion would presumably have led to the half-value. But this result is not an isolated one; it is part of a whole continent of scientific ideas affecting the most fundamental concepts of physics. It is difficult for the audience to weigh fully the meaning of the figures that have been put before us, but the Astronomer Royal and Prof. Eddington have studied the material carefully, and they regard the evidence as decisively in favour of the larger value for the displacement. This is the most important result obtained in connection with the theory of gravitation since Newton's day, and it is fitting that it should be announced at a meeting of the Society so closely connected with him.

The difference between the laws of gravitation of Einstein and Newton come only in special cases. The real interest of Einstein's theory lies not so much in his results as in the method by which he gets them. If his theory is right, it makes us take an entirely new view of gravitation. If it is sustained that Einstein's reasoning holds good—and it has survived two very severe tests in connection with the perihelion of Mercury and the present eclipse,—then it is the result of one of the highest achievements of human thought. The weak point in the theory is the great difficulty in expressing it. It would seem that no one can understand the new law of gravitation without a thorough knowledge of the theory of invariants and of the calculus of variations.

One other point of physical interest arises from this discussion. Light is deflected in passing near large bodies of matter. This involves alterations in the electric and magnetic field. This, again, implies the existence of electric and magnetic forces outside matter—forces at present unknown, though some idea of their nature may be got from the results of this expedition.

I will ask the President of the Royal Astronomical Society to speak.

Prof. Fowler. I should like to emphasise our indebtedness to the Astronomer Royal for the important results obtained by the expeditions. It was he who recognised the specially favourable opportunity for testing the relativity theory which was afforded by the recent eclipse, and who insisted on preparations being made in times of uncertainty, on the chance it might be possible to send out the expeditions. He followed this up by much heavy work at Greenwich. But our thanks are also due to those who undertook the work of the actual expeditions and carried it out so successfully. The conclusion is so important that no effort should be spared in seeking confirmation in other ways. It would now seem to be worth while, for instance, to continue the experiments initiated by Prof. Lindemann in photographing stars near the Sun in daylight by the infra-red rays. Mr. Evershed made elaborate preparations in the summer of 1917 to photograph Regulus in conjunction with the Sun by this method, but he was clouded out, and I have not heard of any further attempts. Good opportunities might not be very frequent, but they would be more so than favourable eclipses. As to the displacement of the Fraunhofer lines to the red, it must be admitted that the results of St. John and Evershed are not so closely accordant as one would desire, but they do agree as to the absence of shifts of the magnitude predicted by the theory. The quantity looked for, I may say, is 100 times the probable error of the measurements which can be made with the large spectrographs now employed in such inquiries. Evershed has also examined the light from the side of the Sun away from the Earth by photographing the spectrum of Venus when on the further side of the Sun. Gravitational displacements, if they exist, should appear in all parts alike; but he finds no displacement on the other side of the Sun, though he does get a slight displacement on the near side. It is very desirable that these spectroscopic tests should be carefully repeated.

Prof. Lindemann. The method to which the President of the Royal Astronomical Society has alluded was evolved in order to test Einstein's theory without being forced to wait for an eclipse. Using dark-red filters, we found it easy to photograph stars in broad daylight fairly close to the Sun. Einstein's theory could be tested, of course, if stars quite close to the limb were photographed. The chief desiderata for this are a clear sky and plates sensitive far into the infra-red. The scattering, of course, decreases with the fourth power of the wave-length, so that any means allowing us to increase the wave-length will be of the utmost advantage.

As to the theory itself, I must confess that the fact that it is always presented from a purely mathematical point of view seems regrettable to a mere physicist. No doubt the present form is correct, and even elegant once one has become familiar with the notation. I cannot believe, however, that a profound physical truth cannot be clothed in simpler language. To put the theory in the form of a curvature in the space-time, manifold seems to me much simpler than the tensor presentation. Cannot Prof. Eddington translate his admirable treatise from the tensor notation into some such form?

In conclusion, I should like to add my congratulations to Prof. Einstein on his good fortune in the observers who tested his prediction, as well as on the brilliant success which rewarded their efforts.

Prof. Newall. May I add my congratulations on the extraordinary success of the observations? I feel that the Einstein effect holds the day, but I do not yet feel that I can give up my freedom of mind in favour of another interpretation of the effects obtained. If Einstein had not existed, or had not predicted a deflection, we might have had a similar experiment made to test the presence of an extended atmosphere round the Sun, and we could have argued from the result back to the hypothesis. Think for one moment of the fine prominence that was seen on the day of the eclipse. It was followed by Evershed for a rotation and a half. Matter was expelled outwards in the course of the movement, as we know from the sequence of photographs secured with the spectro-heliograph at Cambridge on the day of the eclipse. And yet this matter at different high levels goes round the Sun, not with gravitational velocities, but with the same velocities as hold at the lower level of the spots. It is, I know, a cheap criticism to make a qualitative suggestion in opposition to a quantitative result, but I feel that I want to keep an open mind for the existence of matter in a field where gravitational and electric forces are acting simultaneously in unknown relation, under conditions which admit of the existence of an extended corona. It does not carry much weight with me when it is urged that, to explain the deflection in terms of refractive index, a medium having a density $\frac{1}{200}$ or $\frac{1}{40}$ of that of the terrestrial atmosphere would be needed. For we have to do with a possible extended solar atmosphere at very high effective temperature, almost certainly in part controlled by electric as well as gravitational conditions existing in it. And who can say what its refractive properties may be? I prefer to keep an open mind about interpretation. But I wish, in conclusion, to add my heartiest congratulations to the Astronomer Royal and the observers on the observational results obtained.

Prof. Lindemann. Might I say one word more? Have we not evidence from the motion of comets passing near the Sun that matter outside the Sun is distinctly diffuse? The comets suffer no noticeable check in their paths.

Dr. Silberstein. In the first place, I should like also to congratulate the astronomers upon their observational results. But, in spite of what the President said, I believe this result to be essentially an *isolated* fact. There is a deflection of the light rays, but it does not prove Einstein's theory; it cannot be logically deduced from his theory as a gravitational effect in the absence of the spectroscopic result. And, as far as we know from St. John's and Evershed's observations, the predicted shift of the spectrum lines, of an amount exceeding almost 100 times the probable error of the modern spectroscope (as Prof. Fowler has just told us), is not obtained. Prof. Lindemann's attempt to explain the obstinate nullity of this effect by the motion of the luminous gases in the line of vision is not satisfactory; for St. John was well aware of such complicating circumstances, and therefore purposely observed as many as 43 lines at the Sun's centre and almost as many (35) at the limb. Now, we cannot well assume that the gases move everywhere towards or away from us only to suit Einstein's theory, *i.e.* just to compensate his theoretical shift effect. As I have already pointed out on another occasion, if there is no spectral shift, Einstein's coefficient g_{44}, which is entirely responsible for it, must be unity, and if we go back to the differential equations (Einstein's field-equations) we shall see at once that the other coefficient in his well-known approximate solution becomes unity at the same time (the one being $1-\frac{2M}{c^2r}$, the other $1+\frac{2M}{c^2r}$), so that the world element reduces simply to $ds^2=c^2dt^2-(dx^2+dy^2+dz^2)$. And since the equations of motion are always given by $\delta\int ds=0$, we see ultimately that, the spectral shift being absent, Einstein's theory not only does not give such refinements of the planetary motion as is a secular motion of the perihelion, but not even the ordinary Newtonian or Keplerian motion. In short, the orbits of the planets would be straight lines and the planets would move along them uniformly in spite of the Sun's presence. The theory stands or falls on the correctness of the results of Evershed and St. John. It is unscientific to assert for the moment that the deflection, the reality of which I admit is due to gravitation. It is in this sense that I declared it a moment ago to be an isolated fact. The discovery made at the eclipse expedition, beautiful though it is, does not, in these circumstances, prove Einstein's theory. We owe it to that great man [pointing to Newton's portrait] to proceed very carefully in modifying or retouching his Law of Gravitation; this is by no means defending blind conservatism. The spectral shift required is perhaps 100 times, but certainly not less than 40 or 50 times, the error of modern measurement. The solar spectrum can, even in this country, be observed many times a year, and the matter can thus be decided without our having to wait years or centuries for another equally advantageous eclipse. If the shift remains unproved as at present the whole theory collapses, and the phenomenon just observed by the astronomers remains a fact awaiting to be accounted for in a different way.

The Astronomer Royal. In answer to Professor Newall, I would point out that two pairs of stars about equidistant from the Sun, but very differently placed with regard to the denser sections of the corona, show the same displacement, though we should hardly expect such a result from his point of view. Dr. Silberstein is under a misapprehension with regard to my views on repeating this work at future eclipses. I think it most important that this result should be verified at the next two eclipses, only I should hope to avoid the use of mirrors. If necessary, a suitable equatorial mounting should be prepared. The fields are not very favourable, but results can be got with object-glasses similar to those used last May.

Prof. Eddington. One word in reply to Dr. Silberstein. When a result that has been forecasted is obtained, we naturally ask what part of the theory exactly does it confirm. In this case it is Einstein's *law* of gravitation. It is not necessarily his theory that is confirmed, with the underlying assumption that *ds* is a quantity measurable by clock and scale. There still remains the question what the intermediary quantity *ds* is, which must be tested by the Fraunhofer lines.

The President. We must thank the Astronomer Royal and Professor Eddington for bringing this enormously important discovery before us, and for taking such pains to make clear to us exactly where the problem stands.

Photographic Evidence for the Formation of Stars from Nebulæ.

TWENTY-SECOND ANNUAL TRAILL-TAYLOR LECTURE, OCT. 21, 1919, ROYAL PHOTOGRAPHIC SOCIETY.

THE subject-matter of the present discourse, although well known to astronomers, may not be so familiar to photographers. At least it furnishes the opportunity of connecting together, while exhibiting, some of the finest examples of recent celestial photography. The possibilities of photography as an aid to astronomical research were manifested by the photograph of the field of stars, upon the same plate as the comet of 1882, taken at the Cape Observatory, under the direction of Sir David Gill. It is no exaggeration to state that photography has revealed to the astronomer the structure, the dimensions, and the systematic arrangement of the starry firmament, beyond the powers of the largest telescopes. For the action of light upon the photographic plate is cumulative, and hence it is capable of showing what otherwise the eye would never have perceived. The star-clouds of the Milky Way, the detailed structure of the nebulæ, the orderly arrangement of the stars relatively to the Milky Way, could not else have been adequately recognised. Where the telescope shows thousands of stars, the telescope with the adjunct of the photographic plate shows tens of thousands. The naked eye can see about 7000 stars, the photographic plate can reach over 1000 millions. Its aid, too, is all-powerful in sounding the depths of space. For the comparison of photographs of clouds and clusters of stars, taken at an interval of some ten or twenty years, enables the astronomer to utilise the motion of the Sun, Earth, and planets in space to furnish him with a base-line long enough to show the mean parallactic displacement of such distant objects. The outer limits of the clouds of the Milky Way are distant some 30,000 light-