13. DISCUSSION

B. J. Bok. I would now like to invite discussion on the papers that we have heard this afternoon.

W. J. Luyten. There now appears to be a possibility that the Bruce telescope may be mounted again in its old mounting at the Boyden Station. If this should materialize I would like to suggest that it be used to repeat some of the best old long-exposure plates. Conservatively we should be able to find at least 50 of these, showing stars and galaxies down to $m = 18$ pg. These plates are now sixty years old and by repeating them and measuring motions on them relative to galaxies we would have an excellent chance to learn what some of the difficulties are that we are going to encounter in the Lick program. The old plates were taken without a grating, hence we can only measure stars of roughly the same magnitude as the faint galaxies. Moreover, because the scale of the Bruce plates is that of the Carte-du-Ciel (1 mm = 1′) such a program would bridge the gap between the Pulkovo and the Lick programs. Also, the star images on the old Bruce plates are rather fuzzy and therefore resemble more closely the images of the galaxies, which may be of advantage.

S. Vasilevskis. I disagree that fuzzy images help.

W. Fricke. At Heidelberg we have the 40 cm Bruce telescope and we have plates dating from 1900, whose images are also not of the best quality. These plates are usable to determine proper motions of nearby stars, also many red stars can be found. However, I agree with Dr Vasilevskis that one cannot expect the same accuracy as one does from modern astrographs. But I also agree that the tests suggested by Dr Luyten would be valuable.

S. Vasilevskis. In principle I am in favor of Dr Luyten's proposals. I still maintain, however, that fuzzy star images will not be of advantage. The images of galaxies on the Lick plates are measured with errors 1·5 times larger than those for star images. But since there are about 70 galaxies in the reference frame on a plate, the fuzziness of the images of galaxies has virtually no effect upon the accuracy with which the position of a star can be obtained. On the other hand, there is only one single image of each star to be measured, and the error resulting from the fuzziness of a star image will therefore in its full amount affect the determined position of the star.

W. J. Luyten. Images near the center of a good Bruce plate are of better quality than those I have seen near the edge of a Lick plate, yet still within the limits to which measures will be made. What about changes in the lens system and in the emulsion between epochs? The Bruce plates would be valuable for investigating these problems, because such studies carried out now might disclose difficulties which could arise later in the Lick program.

D. Brouwer. The Bruce telescope also may not be the same now as it was sixty years ago. The lens has been dismounted and may therefore not perform in the same way as it originally did. I do not necessarily think it important to have the Bruce restored.

K. Aa. Strand. I would not discourage anyone from carrying out any such program. The sixty-year old Bruce plates should be useful. In 1970, we can meet together again and review the results of all these programs and then decide which one gave the best results. At present all these programs should be considered important and valuable.

J. L. Greenstein. The following remarks refer to Dr Luyten's presentation concerning faint blue stars. Spectroscopic surveys of stars selected in the galactic polar regions only for blueness of color have shown that at a mean apparent magnitude of 15, approximately one-third of the 90 stars observed are white dwarfs. A total of 30 white dwarfs of unknown parallax have recently been found. Only 5 stars out of a total of 180 faint blue and white dwarf stars have been 'normal' B or A stars: these mainly lie between magnitude 11 and 13. The balance

of the stars are (1) 'horizontal-branch' B and A stars of mean spectroscopic absolute magnitude 0 to +3, and (2) hot sub-dwarfs from −1 to +5. Other old stars like nuclei of planetary nebulae range from −1 to +7, and old novae from +3 to +8. In no case is a single direct parallax determination available. Consideration of luminosity of these faint blue stars suggests that, except for the white dwarfs, the mean annual proper motions will be small. But unfortunately no absolute proper motions of the blue stars exist; the majority of the proper motions of even known white dwarfs are still determined from blink methods only. On the assumption that the tangential motions are 60 km/sec, proper motions of the halo and hot sub-dwarfs are only 0″·001 to 0″·010. Even if galactic rotation is the source of dispersion of tangential motion, the proper motions are only 0″·005 to 0″·060; thus the blue stars of magnitude 20 at the galactic pole, if they are in the range $M_v = 0$ to +5, will have $\mu < 0''{\cdot}006$. Thus, the very faint blue stars (near 20 mag.), if they are not all white dwarfs, may provide a slow-moving reference frame. However, decisions as to their nature, which is completely unknown, since spectra stop at 17 mag., will depend on absolute motions, probably with respect to the extragalactic nebulae.

B. J. Bok. In the papers presented this afternoon we have heard from Professor Oort and others about the related problems of A, Oort's constant, and R_0, the distance to the galactic center. It is most important that A should be firmly fixed for the regions near the Sun, for the quantity AR_0 is reasonably well established (approximate value 150 when R_0 is measured in kiloparsecs).

Today we heard that the trend is toward smaller values of A than were suggested a few years ago. Professor Oort seems to support a reduction from $A = 20$ to $A = 17{\cdot}5$. We should, however, bear in mind that during the past decade there have been various suggestions of much smaller values of A. I am thinking here in terms of the OB studies by Whitford and associates at the Washburn Observatory, of Weaver's results, and of the recent value of A found by H. L. Johnson and Svolopoulos from studies of galactic clusters. Over the years I have personally rather favored the Victoria value of the 1930s, $A = 15{\cdot}5$, and the trend seems to be in this direction. An important consequence of a smaller value of A would be that the distance from the Sun to the galactic center might be as great as 10 000 parsecs, certainly larger than Baade's often-quoted value. It would be interesting to hear Professor Oort's comments on the values of A of the order of 15 and less that have been suggested—and also those of Dr Weaver and Dr Whitford, who are both present today. We shall first hear from Dr Schmidt.

M. Schmidt. In trying to reach a decision on the most likely values of A, B and R_0, the following considerations may be mentioned in addition to those given by Professor Oort.

1. The ratio $-B/A$ over the range $R_0 = 6$ to 14 kpc in M 31 is 0·6 or larger.

2. The value of AR_0 as determined from distant stars, is given by Münch as around 200 km/sec. From the OB star material discussed by Thackeray one gets a value around 135 km/sec.

3. The value of R_0 as determined by Thackeray from the same material is 8·9 kpc. Two stars in Münch and Münch's list lead to 9·2 kpc.

4. Fricke has shown that if the rate of escape of stars from the Galaxy is low, the local circular velocity has to be 276 km/sec or more.

5. The relation between A, B, R_0 and the density gradient in the plane of the Galaxy can be studied through the construction of trial models of the distribution of mass. The model I published in 1956 had a large local density gradient. The escape velocity was found to be about equal to the sum of the local circular velocity and the cut-off at 63 km/sec for high-velocity stars moving in the direction of rotation. I now feel that the local density gradient

found in that model is too large. As a consequence, stars near the 63 km/sec limit do not have the escape velocity.

H. F. Weaver. We are preparing a large volume of data on about 100 clusters, and on OB stars and Cepheids, from which we hope to make a determination of the constants A and B from stellar data.

With regard to Professor Oort's presentation, I wish to point out that his values for A and B are local values, inferred from the ω versus R curve. The ratio h^2/k^2, which can be related to the A and B constants, can be determined by various methods, as, for example, the method employing space motions or that employing proper motions. There is a significant discrepancy in the numerical values of h^2/k^2 resulting from these two methods. A value of 0·40 is found from the space motions. Space motions can be determined only for nearby stars; this value is therefore a 'local' value as it should be in keeping with the definition of the velocity ellipsoid. On the other hand, by investigating proper-motion dispersions in different directions, we find a value of 0·24. The average distance of the stars involved in this determination is greater than one kpc. For such distant stars, there are three effects contributing to the proper motions: solar motion; random motions of the stars; and differential galactic rotation. The theory utilized in the usual proper-motion method of deriving the value of h^2/k^2 involves only the first two of these. When a 'two-effect' theory is used to discuss 'three-effect' motions, an erroneous value of h^2/k^2 will result. Thus the larger value of h^2/k^2, obtained from space motions, is likely to be the more nearly correct one. The larger value of h^2/k^2 implies the smaller value of A.

J. H. Oort. We can estimate what improvement in the galactic rotation constants one might hope to obtain from absolute proper motion surveys, such as the Lick program: from a sufficient number of plates taken in the right directions, ω_c should be obtainable with an accuracy of $\pm 0''{\cdot}0002$ or $\pm 0{\cdot}9$ km/sec/kpc. Therefore this would be a significant new determination.

B. J. Bok. Would Dr Whitford comment on his work concerning galactic rotation?

A. E. Whitford. I believe that the Wisconsin results concerning galactic rotation should be viewed with reserve on account of doubts regarding the velocities in various parts of Wilson's catalog.

B. Lindblad. The formula for h^2/k^2 implies a vertex deviation equal to zero, which is generally not the case. One can, however, use a more general theory which includes the vertex deviation, and such recent revisions favor a value of B closer to 10.

I. R. King. I have considered the effects of deviations from a Gaussian curve in the velocity distribution and of irregularities in the shape of the velocity ellipsoid. No comprehensive investigation has yet been made, but preliminary studies show that effects of this kind appear to be significant.

R. P. Kraft. Analysis of Cepheids in galactic clusters leads to a value of A of approximately 15 under the assumption that the slope of the period luminosity relation in our Galaxy is the same as that in the SMC. This result agrees with that of Johnson and Svolopoulos; but the two results are not independent, because the Cepheids used here have been calibrated by means of photometric distances of galactic clusters.

H. F. Weaver. But if one uses a zero point derived from proper motions and not from clusters, one still gets a value of A between 14 and 15, which constitutes a confirmation independent of galactic clusters.

B. J. Bok. Dr Blaauw has drawn attention to the key position that the h and χ Persei cluster and the Scorpio-Centaurus moving cluster hold in the calibration of absolute magnitudes of

super-giants and of blue giants. Recent work at Radcliffe Observatory (Thackeray and Feast) and at Stromlo (H. M. Johnson, Aller and Faulkner, Buscombe) shows that the super-giants in the Magellanic Clouds have normal spectra and that the emission nebulae in the Clouds show the same sorts of relative abundances of the chemical elements as do those of our Galaxy. Would it not be desirable to use the blue super-giants in the Clouds (especially those of the Large Cloud) for calibration purposes? The extreme permissible range of distance modulus for the Large Magellanic Cloud seems to be 18·7 to 19·2 and, if, as seems likely, this range can be halved in the next few years, we should have by far the most reliable absolute magnitudes for super-giants from the data of the Large Magellanic Cloud.

A. Blaauw. This method would be very good indeed, but it will still require an accurate independent determination of the distance moduli of the Magellanic Clouds.

B. J. Bok. We hope to have a determination good to $\pm 0{\cdot}2$ mag. within a few years.

A. Blaauw. We should nevertheless continue to improve calibrations based on moving clusters.

M. W. Feast. We are engaged in a study concerning the use of the Magellanic Clouds for absolute magnitude calibrations. This work can be continued only after a thorough investigation has been made of the possibility of basic differences between stars in the Magellanic Clouds and in our Galaxy.

F. J. Kerr. A major problem in all 21-cm studies is the lack of an independent distance scale. It has been necessary to introduce certain assumptions regarding galactic rotation and overall symmetry of the Galaxy. The higher resolution now obtainable will enable us to measure the width of the hydrogen layer in any particular direction or region. And although it will still be necessary to make certain assumptions, this will give some independence to the 21-cm distance scale. We can try to study the circle of zero radial velocity through the Sun and see how far we can trace it through the Galaxy.

A. Blaauw. If we want to have proper motions in the Scorpio-Centaurus region with the necessary precision, how accurately can we hope to obtain such absolute proper motions, considering the large area covered by the association which makes it necessary to combine meridian observations made from the northern and from the southern hemispheres? In such a procedure, systematic errors and possible discontinuities in the various series of meridian observations may cause a serious problem.

We can, of course, hope to obtain proper motions with respect to extragalactic nebulae for these stars as a result of the Lick program. But how and with what precision will it be possible to extend within the Lick system a reliable tie-in to these stars in low galactic latitudes?

S. Vasilevskis. There are two ways to accomplish this, both requiring proper-motion determinations on each side of the zone of avoidance.

1. By means of overlapping plates the Lick proper-motion system can be extended across the zone of avoidance. Such a procedure, if properly carried out, should result in negligible closing errors where the systems for the two hemispheres are joined.

2. By including a sufficient number of AGK stars in the Lick program one can establish and extend across the zone of avoidance the relation between the proper motion systems of the AGK and of the Lick program.

Galaxies in 'windows' of the zone of avoidance can provide additional reference and check.

H. K. Eichhorn. Programs such as the Lick proper-motion survey will provide absolute proper motions of lesser quality of a large number of stars which can then be used as reference

stars for determinations of accurate absolute proper motions of important individual stars by means of long-focus refractors.

M. W. Ovenden. I would like to comment on the use of the moving cluster method of distance determinations. Without a knowledge of the distances of the stars, it cannot be proved that the motions of a given selected group of stars do fulfill the criteria for a moving cluster. The method therefore makes the hypothesis that, if the observed motions of the selected stars show (approximately) the features that would be exhibited by a moving cluster, then the selection of stars does fulfill exactly the criteria for moving clusterhood, the discrepancies being due to errors of observation. This would be a reasonable hypothesis only if there were some special property in a set of stars having a common space motion—some large *a priori* probability of a group of stars forming a moving cluster. There seems no good reason for believing this to be so, *in general.* Hence I believe that the moving cluster method of distance determination should be used only with the greatest caution, especially for widely-dispersed groups of stars.

R. M. Petrie. I wish to make the following remarks concerning the calibration of the absolute magnitudes of B stars:

1. The study by Bertiau of the Scorpio-Centaurus group makes possible a direct comparison between the MK and the DAO absolute magnitude systems. From 16 stars such a comparison gives:

$$\mathrm{MK} - \mathrm{DAO} = -0^{\mathrm{m}}\cdot 41 \pm 0\cdot 13$$

2. New radial-velocity determinations show many stars to have variable radial velocity. Of 36 stars observed by Buscombe and Morris, 20 have variable radial velocity. Of a total of 77 stars, radial-velocity values cannot be adopted for more than 51 stars. The average residual without sign is 8 km/sec.

3. The new determinations of proper motions and of radial velocities suggest that the conventional moving cluster treatment may not be legitimate: (*a*) if it *were* legitimate, then the new radial velocities would give a correction of about $-0\cdot 2$ to the MK absolute magnitudes as deduced by Bertiau; (*b*) if it *is not* legitimate, then the standard solution for the mean parallax results in a correction of $+0\cdot 4$ to the MK values.

4. Additional radial-velocity observations are essential to clarify the nature of the motions and to give a reliable space motion, if one is at all meaningful. The great sensitivity of the method to the adopted value of the space motion is not a very good feature, particularly since addition or removal of a few stars makes a significant difference in the resulting space motion. Addition of 16 stars, for instance, changes S from 25·9 km/sec to 21·9 km/sec.

A. Blaauw. For the Hyades and for Scorpio-Centaurus it can be shown that any irregularities can be explained on the basis of errors in the observed proper motions and radial velocities. Such discrepancies therefore do not provide a basis for an objection against the use of the moving cluster method, but merely demonstrate the need for more and better quality observations.

B. J. Bok. Dr Kerr has recently made the suggestion that the interstellar hydrogen in the Galaxy near the Sun has an outward motion from the center of the Galaxy of the order of 7 kilometers per second relative to the older stars—which should on the average show no such outward motion. Small residual effects might be shown by the very youngest stars and associations (O to B) and Dr Pismish has actually pointed to the possible existence of such an effect. I see no evidence of any such effect in Edmondson's tabulations. Would Dr Kerr and Dr Edmondson care to comment on this?

F. J. Kerr. Are the motions of Edmondson's groups of stars really representative of large-scale motions or could they be due to local streaming?

F. K. Edmondson. My table shows that the X_0 - component of solar motion is practically constant.

W. Fricke. There is an angle of about 5° between the directions of the centroids of motion of the high-velocity and of the low-velocity stars, respectively. If the centroid of the high-velocity stars coincides with the direction of circular velocity, then the low-velocity stars move on the average in an orbit which is directed inwards by 5° from the tangential direction.

K. L. Franklin. I plan to convert the Wilson catalog to galactic co-ordinates and to the local standard of rest and I invite comments concerning the catalog material. It is my intention to compare the motions of OB stars with the 21-cm observations of the motions of neutral hydrogen.

V. Osvalds. The results from a recent investigation of *Mira* variables suggest that:

1. For all known *Mira* variables with periods from the shortest up to 225 days, radial velocities and proper motions should be determined in order to see whether the still persisting bump in the absolute magnitude-period curve is real.

2. Spectra of *Mira* variables with periods from about 275 days to 325 days should be examined to see whether there is a more definite division between the shorter- and longer-period *Mira* variables. The velocities and their dispersions are different for these two groups, but the division cannot be established very accurately from these data alone.

B. J. Bok. Thank you very much for having been present.