MEETING OF THE ROYAL ASTRONOMICAL SOCIETY

Friday, 1960 April 8 at $16^h\ 30^m$

Professor R. O. REDMAN, *President*,
in the Chair

Secretaries: H. BONDI
M. W. OVENDEN

The Minutes of the previous meeting were read, confirmed and signed. *The President* announced with regret the death of Dr. Beno Gutenberg and Fellows stood silently in his memory.

The President. Today we welcome Dr. C. S. Gum who is on his way back to Australia. Perhaps he would like to say something about his work there.

Dr. C. S. Gum. I am returning to the Chatterton Astronomy Department of the School of Physics, University of Sydney. The equipment there includes a conventional 40-inch reflector and we are building a large intensity interferometer to extend the work on star diameters initially carried out by Hanbury-Brown and Twiss at Manchester.

The President. I wish all speakers were as modest. Now Dr. R. d'E. Atkinson is going to give an account of his work on the design and tests of a mirror transit circle axis.

Dr. R. d'E. Atkinson. In 1946 I outlined a proposed "Mirror Transit Circle", namely a Transit Circle whose moving telescope is replaced by a moving plane mirror with its normal sweeping out the meridian. The two collimators now act also as fixed observing-telescopes. The advantages appear considerable, but the mirror must be very well mounted in the axis. I have now designed and made an axis and mirror, to test whether adequate stability is obtainable.

As proposed originally, there are two flats on the edge of the mirror, perpendicular to its main face and parallel to each other; auto-collimation on a flat, through the appropriate pivot, can then completely determine how the normal to the main face wanders in R.A., as the axis is rotated. No similar tests on the optic axis of a telescope are possible; the collimation of an ordinary T.C. is determined only when the telescope is horizontal (or, more rarely, vertical) and if flexure changes it at other pointings, this is unobserved. A Mirror T.C. thus has a definite advantage here.

The requirements for mirror-stability are, however, severe. The probable error of a star-place observed at one transit is about $\pm 0''\cdot 18$, and fidget of the mirror ought to add very little to this; allowing a factor of two for the reflection, its own p.e. should thus be well below $\pm 0''\cdot 09$, say about $\pm 0''\cdot 025$. Since, now, the three constraints needed to locate the mirror cannot have lever-arms much exceeding 20 cm., any random (*i.e.* non-elastic) movement at one constraint should have a p.e. not greater than $\pm 0\cdot 025/(2)^{\frac{1}{2}}$ microns, or about $\lambda/30$ in optical terms. I decided therefore in 1946 that the weight of the mirror should be counterpoised and that the constraints (steel, not lead) should be nearly flat and co-planar, so that the areas in elastic contact can be large.

If the mirror is counterpoised, the counterpoises can also neutralize its flexure. My $12\frac{1}{2}$-inch quartz mirror therefore has six invar seatings cemented on to its back, about 8·3 inches from its centre, and these carry ball-joints on the ends of steel rods; the other ends of these have ball-joints mounted (three and three) in one of two triangles, each with a ball-joint at its centre, by which it is attached to a counterpoise-arm. Each arm is mounted in gimbals coaxial with the axis, and the two counterpoises are also coaxial, inside the axis, and have central holes for the pivot-telescope beams.

The first slide shows this counterpoise system, mounted in a dummy axis and carrying a dummy mirror of the correct size and weight. The mirror is also counterpoised edgeways, by the parallel-link system shown, and is thus in complete neutral equilibrium however the axis is rolled along. Moreover, there is no couple in declination, either on the mirror or on the axis.

The second slide shows the interference-fringes on two of the nearly-finished constraints, when a glass proof-plane was laid on all three at once. (The third was similar.) The fringes move only very slightly if the pressure is increased by 1 kg. Each pad is 9 mm in diameter, machined integrally with the axis, chromium plated, and lapped by hand.

The axis is machined out of a solid steel cylinder, 36 inches long and 18·4 inches in diameter. Its weight is carried by two counterpoised ball-races (except for a small load on the pivots), and if the axis were bisected each half would balance in its own ball-race; since the weakest point is certainly at the centre, this balancing should nullify the leading term in the flexure.

Auto-collimation through one pivot was observed with a telescope of 76 inches focal length, having a single-screw micrometer with a pitch of 1/100 inch. This was read to 1/1000 rev., and the six readings taken at each axis-position were averaged to a fourth place directly; the unit is thus a millionth of an inch, or 0·00272 seconds. The axis was advanced in 30° steps, through a full revolution plus an overlap (up to a revolution) for drift of the telescope, readings being taken in x only; then the micrometer was rotated 90° and a similar set was observed in y. If ψ is the angle between the main mirror and the axis, then except for a small and determinable constant, we have $\psi = -x \sin\theta + y \cos\theta$, where θ is the nadir-distance of the main normal. We thus get twelve ψ-values, for the twelve steps of rotation. On October 7, the mean of the twelve was -1131 (*i.e.* $-3''\cdot08$) and if $\psi + 1131 = R(7)$ the sum (without regard to sign) of the twelve R-values was 121; on October 9, the mean was -1137, and if $\psi + 1137 = R(9)$ the sum of the R is 132. The last slide shows $R(7)$ and $R(9)$; they are evidently very similar, and the sum of the twelve $R(9)$–$R(7)$ is only 59. The corresponding probable error of the change in mean tilt, from -1131 to -1137, is $\pm 0\cdot 8453 \times 59/(12\sqrt{11}) = \pm 1\cdot 25$, or $\pm 0''\cdot 0034$. $(R(9)+R(7))/2$ is the combined effect of pivot-errors and axis-flexure; its p.e., for any one θ-value, is $\pm 0''\cdot 0034 \times \sqrt{12}/2 = \pm 0''\cdot 006$, very considerably smaller than what had been demanded. Indeed, comparison with the Cooke T.C., allowing for the total time and total number of observations involved, suggests that almost everything here must really be pure errors of observation and not fidget of the mirror at all; but even if it were all fidget it would be small enough.

I should particularly like to thank the Astronomer Royal for freeing me from almost all other duties during this work, and for supporting it in every way I asked.

The President. One of the difficulties with transit circles is that they are elastic and nothing stays still and therefore it is necessary to have small flexures and to check them continuously.

Mr. C. A. Murray. The great advantage of the mirror transit circle is that, as Dr. Atkinson has remarked, observations on the side mirror enable the deviation of the normal to the main mirror from the true instrumental meridian plane to be determined directly, and, if considered necessary, for each star transit.

In the case of a conventional transit circle the locus of the optical axis has to be inferred from pivot error measures made at isolated epochs, on the assumption that axis flexure is symmetrical. Furthermore there is no reason to suppose that the pivot errors themselves remain constant with time.

The President. Can you observe both above and below the pole?

Dr. Atkinson. The entire meridian can be observed in one or the other of the two telescopes, and there is a region 5° each side of the zenith where both can be used without appreciable vignetting. I do not think there is any advantage in trying to go further.

The President. When do you hope to carry out the first experiment?

Dr. Atkinson. As soon as the Admiralty allows!

Dr. A. Hunter. Your instrument gives a beam of about 8 inches. Can really large transit circles, say 12–14 inches, be made in the same way?

Dr. Atkinson. Yes, I think so, but more than six supports for the mirror would probably be needed. But I am not sure if there would be any advantage as the present instrument ought to go to about $9^{m}\cdot5$.

The President. I am sure we all join the Astronomer Royal in congratulating Dr. Atkinson and we thank him for his interesting account of it. Now I invite Prof. W. H. McCrea to discuss his joint paper with Mr. D. McNally on the formation of Population I stars, Part II.—The formation of molecular hydrogen in interstellar matter.

Prof. W. H. McCrea. When we first make hydrogen gas at school we are given a number of reasons for believing that we have *molecular* hydrogen, and we learn why it is difficult to get free atomic hydrogen. On the other hand, when we first hear that interstellar matter consists mostly of hydrogen gas we are quite prepared to believe that it is in atomic form. However, this belief requires to be justified. At a typical point in interstellar space, the mean life of a H_2 molecule before photo-dissociation is probably about 10^6 years; but the rate of direct recombination is so small that even this rate of dissociation would in fact prevent there being an appreciable proportion of H_2 unless some other recombination-process is operative.

Van de Hulst pointed out in 1948 that probably the most effective process for the production of H_2 in interstellar space is a surface reaction on interstellar grains. This is a type of reaction that is well known to occur in the laboratory. Its possible astrophysical importance has been studied by several other authors. In particular, Kahn has pointed out the significance of H_2 molecules produced in this way for the cooling of interstellar clouds after mutual collisions.

Our first purpose is to calculate the reaction-rate and the consequent abundance of H_2 molecules, taking as careful account as possible of such features as the distribution of grain-sizes, etc. The results have to be expressed in terms of a parameter π that describes the unknown efficiency of the surface reaction ($\pi = 1$ for maximum efficiency). I need not give the tabulated results here, but I could mention as typical that with Dunham's value for the radiation-field in a cloud of hydrogen of 10 atoms/cm^3 at a kinetic temperature of 100° K, we should have about 0·5 H_2 mol./cm^3 if π is approximately unity.

The next purpose is to see what checks there are upon such predictions. These are: (*a*) The cooling already studied by Kahn. If $\pi \sim 1$, we get a little more H_2 than he did but not enough to make much difference to the resulting temperature. (*b*) The interesting feature that this type of process gives a maximum possible density of *free atoms*. The actual figure I get with Dunham's radiation-field and kinetic temperature 100° K is 73 atoms/cm^3. This may possibly explain why observations seem never to have revealed a density of more than about 100 atoms/cm^3 even in regions where much higher densities could reasonably be expected. (*c*) Some comparison with the case of interstellar *CH*, which is discussed in the paper. All these considerations give quite strong support to the possibility that H_2 is made in the manner discussed and that the value of π is fairly near to unity.

If this is so, then the significant result is that if the density of interstellar matter is increased to a total of about 1000 atoms/cm^3 then it will nearly all be in the form of H_2. This is probably very important in the problem of forming stars from interstellar matter.

Dr. F. D. Kahn. Can you say something about possible mechanical effects arising from the change of specific heat when atomic hydrogen is converted into molecular form?

Prof. McCrea. The really important point is that the system can collapse so readily.

Dr. D. Sciama. This would not help the first generation stars because there was no dust for them.

Prof. McCrea. Surely it is not known whether dust was present initially or not?

Dr. M. J. Seaton. Previous attempts have been made along similar lines but they seemed to show that the process was not sufficiently powerful. What is the essentially new feature in your work?

Prof. McCrea. We have considered a much larger range of densities.

Dr. Seaton. Is there a critical point at some stage in the condensation?

Prof. McCrea. Provided one has densities somewhat higher than in a typical cloud condensation can occur.

Dr. R. J. Tayler. What is the time constant of the equilibrium equation?

Prof. McCrea. In the paper we put the longest at about 10^6 years.

The President. It would be nice to have an observational measure of molecular hydrogen. This may be possible from an artificial satellite using forbidden lines in the infra-red.

Prof. C. W. Allen. You mentioned 73 as the maximum number density of hydrogen molecules. This figure depends both on the number of dust grains and on the radiation density, and there may be an overall maximum if ionization is an important factor.

Prof. McCrea. I do not think there is any question of ionization in the present case.

The President. Thank you, Prof. McCrea, for discussing this important subject of the formation of stars from interstellar gas. Observational evidence is very poor at present but there are possibilities of getting more from artificial satellites. Dr. R. V. Willstrop will now tell us about his observations of absolute measures of stellar radiation.

Dr. R. V. Willstrop. This paper is concerned with the measurement, in c.g.s. units, of the intensity of radiation of certain wavelengths received outside the earth's atmosphere from stars of known magnitude and colour index.

Theoretical astronomers are interested either in the total radiation of stars, which cannot be measured from anywhere within the Earth's atmosphere, or in the intensity in much narrower regions, perhaps less than 1 A wide, but most stellar photometry is still carried out by measuring regions of the visible and near ultra-violet and infra-red spectrum about 1000 A wide. Measurements made in regions of different width do not give identical results; when measured in broad bands hot and cool stars are apparently brighter than stars with intermediate temperatures even if they are equally bright in a narrow band at the same equivalent wavelength. This "band-width effect" may be much reduced by measuring narrower regions. Woolley has shown that, for a black body, the average intensity of a region 300 A wide centred at 5000 A is proportional to the intensity at the centre of the region within 0·1 per cent over nearly the whole of the observed range of stellar temperatures. Nevertheless, broad-band measures are not likely to be abandoned, if only because broad spectral regions must be measured if the faintest, most distant stars are to be compared with nearer stars with as little random and systematic error as possible.

The aim of this work was to provide a "conversion table" to allow one to find the flux of radiation within fairly narrow wavelength bands, as received outside the Earth's atmosphere, from any star of known magnitude V and colour index $B-V$ on H. L. Johnson's *U.B.V.* system.

At the Cambridge Observatories a spectrometer with adjustable exit slits was used to measure selected regions of stellar spectra 200 A wide. It was found that measurements made in regions centred at 5410 A and at 4390 A agreed well with V and B magnitudes respectively. By making many observations the uncertainty in these wavelengths was reduced to about ± 5 A, but the agreement of individual narrow-band measurements with Johnson's broad-band magnitudes was almost always limited by the poor photometric conditions at Cambridge. No narrow region matched the broad-band ultra-violet magnitudes, because of the strong features in that region of stellar spectra.

At the Royal Observatory, Cape of Good Hope, an artificial star, composed of a calibrated lamp and filter, was compared with bright stars, using a 4-inch telescope with a d.c. photometer and four interference filters selected using the Cambridge results as a guide. One filter was chosen in the visual region of the spectrum, and three in the blue. The equivalent wavelengths of these filters were: y, 5390 A; b_1, 4571 A; b_2, 4401 A; b_3, 4311 A. b_1 and b_3 were also used because b_2 was very transparent at 4340 A and it was feared that measurements made with it would be affected by $H\gamma$. The atmospheric absorption was determined frequently by measuring standard stars at different altitudes. Care was taken to make allowance for small systematic errors, not exceeding 1 per cent altogether. The radiation received outside the Earth's atmosphere from a star of magnitude $V=0{\cdot}00$ and colour index $B-V=+0{\cdot}60$ was found to be $(3{\cdot}85 \pm 0{\cdot}08) \times 10^{-7}$ ergs cm^{-2} sec^{-1} (100 A)$^{-1}$ at 5390 A. This is not quite independent of the colour index of the star, because although the filter y had its peak transmission at 5410 A it had an equivalent wavelength of 5390 A, where hot stars are brighter and cool stars fainter than at 5410 A. The flux received from a star of magnitude $B=0{\cdot}00$ and colour index $B-V=+0{\cdot}60$ was found to be $(6{\cdot}62\pm0{\cdot}13)\times10^{-7}$ ergs $cm^{-2}\,sec^{-1}\,(100\text{ A})^{-1}$, but this also depends slightly on the colour index of the star, because of the "band-width effect" mentioned earlier. The result for the visual region differs by only $2\frac{1}{2}$ per cent from a recent determination based on a measurement of the solar radiation by Dunkelman and Skolnik and on the apparent magnitude of the Sun, $m_{pv}=-26{\cdot}73$, measured by Stebbins and Kron.

The narrow-band magnitudes and colour indices for the 221 stars measured in the course of this work have been compared with broad-band measures. The best agreement is found with the latest Cape measures, and with H. L. Johnson's results. If a small number of stars, including the Wolf-Rayet star γ Vel and some faint stars which I could not measure sufficiently accurately, are ignored, the r.m.s. difference between these broad and narrow-band visual magnitudes is only $\pm0^{m}{\cdot}014$ when a linear correction in $B-V$ is applied. Most of this difference is accounted for by the internal errors of each series, estimated to be $\pm0^{m}{\cdot}007$ (Cape, Johnson) and $\pm0^{m}{\cdot}010$ (Willstrop), leaving only $\pm0^{m}{\cdot}007$ to be explained by measuring spectral regions of different equivalent wavelength and very different width. It follows that with an error tolerance $0^{m}{\cdot}01$ there should be no difficulty in reconciling visual magnitudes measured in slightly different spectral regions, in either broad or narrow bands. The only condition to be met is to measure the magnitudes accurately in the first place. The colour indices b_1-y and b_3-y of the artificial star and of early type stars were compared with Greenwich gradients measured by Gascoigne. The relation found between Greenwich gradient G and absolute gradient ϕ is $\phi=G+1{\cdot}11$, agreeing identically with the best previous results. The b_2-y colour indices are affected by the spectral line $H\gamma$ and were not used.

The Astronomer Royal. Congratulations on this work. I am surprised that the narrow band at 4401 A works so well in late type stars.

Dr. R. V. Willstrop. These interference filters have broad tails which may help in this case.

Dr. M. W. Ovenden. When doing your comparison you assume that stars form a one-parameter system. Are there anomalous cases such as unresolved binaries?

Dr. Willstrop. Yes. We try to avoid stars such as these. The b_1-y colour index of the B8–K0 star η Car departs by more than $0^{m}{\cdot}1$ from the colour expected from its $B-V$ index and the relation for single stars.

The President. Thank you, Dr. Willstrop, for such an interesting and clear presentation of your first paper to the Society. The meeting is now adjourned to 1960 May 13.