One part of the curve is red, another green, another purple, some are of mixed shades; indicating that the colour of the image (which is normally *time-integrated* by the eye, as in Newton's 'colour disk') is *differentiated* out into a constantly varying colour sequence. With a pair of light red filter-glasses placed in front of the eye-lenses, the star is seen to go right out about thirty times a second, the circle of light now being discontinuous and composed of red arcs separated by dark spaces. Frequency may be estimated roughly by comparison with a mains-run neon lamp placed in the same field of view. These colour changes decrease with increasing altitude, but on frosty nights when the 'seeing' is poor, they can be seen even in bright stars near the zenith. If we turn next to Saturn, which at the time of writing is readily observable at the same altitude as Sirius, we find that the colours are entirely absent—the 'time-base' ring of light is quite uniform both in colour (a pale yellow) and intensity.

It seems very desirable, from the astronomical point of view, that the physical characteristics of these colour variations in star images should be investigated in greater detail, especially in regard to their dependence upon meteorological conditions and upon telescope aperture. One method which suggests itself is to direct the converging beam through a narrow band-pass filter before it reaches the principal focus of the telescope and there to receive it in a multiplier photocell. If the main beam were interrupted at a suitable frequency and the cell output displayed on the time-base of a cathode-ray tube, the fluctuations of intensity could be measured in light of different colours.

Seeking a physical explanation for these effects, it seems clear that the foci for the various wavelengths must be oscillating rapidly in and out *along the line of sight*. This must arise from the interposition in the parallel beam of starlight of weak atmospheric 'lenses'—some effectively converging, some diverging—forming and dissolving at a frequency of perhaps thirty times a second. It seems likely that these air lenses are identical with the moving striæ which have been postulated by R. W. Wood and others to account for scintillation and 'shadow-bands'. Chromatic aberration will be inherent in such lenses and will bring different colours to separate foci. As the lenses 'bulge in and out' and change their power, these colour foci may be expected to oscillate backwards and forwards in the line of sight. Visible scintillations probably arise from occasional abnormally large oscillations of this type. Finally: Do 'monochromatic' stars, like Antares, appear to scintillate more noticeably than white ones, like Sirius?

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THE following observation seems to show that any physiological component¹ in the twinkling of the brighter stars is negligible compared with the physical. If half of a 1 cm. × 2 cm. rectangular hole in a sheet of cardboard is covered with a prism deviating light through about 7°, its apex towards the middle of the hole, an observer about 50 cm. behind the hole can place himself so that he sees one star through both halves of the hole, light from the uncovered half passing straight to one eye, light from the covered half being deviated by the prism

to the other eye. If now the observer (assumed emmetropic) puts on -2D spectacles and converges his eyes to the hole, he sees two well-focused images which, if of a bright and twinkling star, can easily be seen to twinkle almost exactly synchronously.

This is what is to be expected on R. W. Wood's explanation of twinkling, if the bright and dim bands in which a star is at any instant illuminating the earth's surface are of a size greater than about 1 cm., the distance between the rays passing from the star to the eyes in my observation. That they are so can be observed directly: if the eye is placed at the image of a star in a large convex lens, these irregular bands of light and darkness can be seen fitting across the lens. They are about 2-10 cm. in width.

Any physiological explanation of synchronous twinkling in this observation would necessarily apply also if the images seen in the two halves of the hole were not of the same star but of different stars of the same magnitude. This alteration can be achieved by removing the prism and suitably adjusting the distance and position of the hole; and it is then seen that the twinkling of the two images is not synchronous.

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¹ Hartridge, H., *Nature*, **164**, 999 (1949), and **165**, 146 (1950).

BEFORE replying to the above letters there is one point which seems to require elucidation, namely, the exact meaning of the word 'scintillation'. Mr. Weale and myself, following the lead of Prof. R. W. Wood, limit it to two alterations only, namely, brightness changes and colour changes, and this fact we tried to make clear in our first letter to *Nature*. But it is obvious from many personal letters which we have received that some other people use the word in a much broader sense, and include such things as the apparent oscillatory movements of a star image at right angles to the line of sight as seen through an astronomical telescope, or the changes of focus which are also sometimes observed. "Scintillations can be photographed" they write. Further inquiry shows that they are usually referring to these oscillatory movements, and changes of focus, but sometimes they are also referring to alterations of shape, size and total brightness.

I am very grateful to Prof. M. Minnaert for the two references which he has given in his letter. Previously the only one known to me was "Physical Optics", by Prof. R. W. Wood. Here are some comments on the points which Prof. Minnaert raises.

(1) The statement that the movements of star images are too small to be visible to the naked eye surprises me. While average star-image displacements usually have values of 3", exceptional ones, I understand, may have values as large as 10" or even 15". If, then, two neighbouring stars are moving towards, or away from one another, their relative angular displacements of 20-30" should be visible to the naked eye of an observer who has good visual acuity for detail. Thus if the entire sky were filled with stars undergoing such displacements, it would superficially resemble Brownian movement, as I suggested in my first letter.

(2) If I am under a misapprehension, as Prof. Minnaert suggests, when I expect the stars to make greater apparent movements in the direction of the wind, perhaps I may briefly state the reasons for that

conclusion. At the interface between the layers of warm and cold air, 'waves' are stated to occur. Now waves more closely resemble cylinders than they do spheres; moreover, the axis of the cylinders would normally be at right angles to the direction of the wind. Starlight viewed through such cylindrical waves would suffer continual alterations of refraction, which would be much greater in a direction at right angles than perpendicular to the long axis of the waves. Thus the apparent movement of the stars should take place preferentially in the direction of the wind.

It has been suggested to me that the words 'waves', 'striae' and 'bands' which are used in R. W. Wood's description of the theory of scintillation give a false picture of what is actually taking place, and that at the interfaces between layers of air at different temperatures and water-vapour content, the surfaces do not resemble cylindrical but spherical lenses. Now this suggestion does not agree with the photographs obtained by Dr. Gaviola, which show the band-like striations extremely clearly. It will be recollected that he used a camera which had a lens of 75 mm. focal length, that this was focused on the primary mirror of the 60-in. reflector of Cordoba Observatory and that the exposures ranged from 0.1 to 0.01 sec. Seven photographs taken by the light of Sirius showed the shadow bands very clearly; and they provided evidence either for elliptical or for cylindrical lenses, but not for circular ones. The suggestion that the shadow bands might be due to strata of warm and cold air inside the tube of the telescope finds no support because, even if such strata were present, their optical properties would not disclose themselves unless a carefully adjusted Foucault knife-edge or diaphragm had been placed in front of the camera lens, and there is no evidence whatever that this had been done. In conclusion, therefore, it may be stated that striae are wavelike and should therefore cause bigger star movements at right angles to the long axis of the waves than parallel to that axis as suggested in my second letter to *Nature*.

(3) With regard to temporary refraction spectra, I agree with Prof. Minnaert that these must be small. It is probable that they do not exceed 0.5", and would therefore be unobservable with the naked eye. But I still think that they should be visible through a good astronomical telescope, the Airy disk of which may subtend an angle of less than 0.2". If they are never seen at all, then I think that something must be wrong with the refraction theory; and evidently I am not alone in holding this view, since Prof. Minnaert refers to it as "widespread".

If the alterations in the colours of the stars are due, as Prof. Minnaert says, to regular atmospheric refraction, why are red and blue-green the only colours seen? Why are not scintillations of the other spectral colours witnessed: for example, orange, yellow, yellow-green, blue or even violet; and at times purple and mauve? This limitation of the colours to pink and blue-green is, in my opinion, one of the more serious defects of the atmospheric refraction theory.

(4) There is no doubt that simultaneous observation by right and left eyes does require practice. Recently I have been using two weak prisms, one base up, the other base down. First one of these is placed in front of, say, the right eye; then when the two star images tend to draw together into coincidence the second prism is substituted; and so observation is continued using the prisms in turn.

(5) For stereoscopic vision, precise binocular adjustment is not necessary. This fact is brought out clearly by the correct observation of stereoscopic effects by the single flash of an electric spark. Prof. Minnaert's conclusion is true, however, that if the lateral displacements of the star image are no larger than a few, say five, minutes of arc, they would probably not cause noticeable stereoscopic effects. But I am informed that at times they may have two or even three times this value, and in these circumstances stereoscopic effects should sometimes be visible.

(6) Prof. Minnaert asks how does physiology account for the striking difference between stars of high and of low altitude above the horizon. Personally, I would not describe the difference as "striking". I have often compared stars of similar brightness, one near the horizon, the other near the zenith, using direct vision for one star and vision through a right-angled prism for the other. Sometimes differences were clearly seen, but at other times they seemed small or even negligible. Apart from this, however, the fact that Prof. Minnaert asks how physiology accounts for the difference suggests that he has misunderstood my previous letters. On no occasion have I claimed that all the phenomena connected with scintillation can be explained on purely physiological grounds, and I have always stated that there must be a physical as well as a physiological basis.

Mr. H. F. H. Jones directs attention to the interesting point that to most people in the tropics most stars do not scintillate. How do supporters of the view that scintillation is due to atmospheric disturbances account for this strange fact? The inference that turbulence is nearly absent or inappreciable in the tropics is surely contrary to what would be expected in view of the great heat and the sudden fluctuations of temperature which are common there.

Mr. K. Harrison in the first part of his letter refers to the tremulous star movements which have been mentioned earlier in this letter. He holds the view that since they can be photographed they cannot be explained on a physiological basis and are most likely due to atmospheric disturbances. With this idea I am in complete agreement. Mr. Harrison then goes on to mention "jumping stars". I have never witnessed these and so am not in a position to comment on them, but I have seen movements in artificial stars which could be described in this manner.

Prof. G. C. Varley asks "if stellar scintillation were largely a retinal phenomenon as suggested by Hartridge and Weale . . .?" I would like to point out that Prof. Varley has misunderstood our point of view, because we are convinced of the existence of an important physical factor, and it seems likely that this plays a part quite equal to that of any physiological processes. At the end of this letter I will briefly discuss what these might be.

Before beginning my reply to Dr. M. A. Ellison, may I express my admiration for his experiment with the prismatic telescope. The colours which he sees with his binoculars do puzzle me, however, because he writes: "One part of the curve is red, another green, another purple, some are of mixed shades". The statement that purple is seen surprises me for two reasons: (1) I have never seen purple in connexion with scintillation, and (2) I cannot see how this colour can be produced by atmospheric lenses. According to R. W. Wood, the diameters of these lenses is about 4 cm. and their focal lengths perhaps

4,000 m. On applying the formula for the radius of the first dark ring of the diffraction pattern: $R = 1' 22'' \times \lambda \times F/A$, the radius is found to have a value of about 60 mm. for green light. Now this is large in comparison with the size of the pupil of the observer, so that it appears to be unlikely that there would be any noticeable colour-selecting effect, such as Dr. Ellison has suggested. Let us suppose, however, that sharp foci are formed for the different coloured rays, and that these coincide in turn with the centre of the observer's pupil. When blue rays are in focus they enter the eye of the observer with undiminished intensity, but the rays of longer wave-length which have not yet come to a focus will encounter the edges of the pupil and in consequence will have their intensity reduced. Similarly, when the green rays are in focus on the pupil, both the blue rays and the red rays will suffer a reduction, and when the red rays are in focus those to suffer a reduction will be the ones with a short wave-length. Therefore as the focal lengths of the atmospheric lenses alter, it would be expected that an orderly sequence of colours would be observed: bluish-green when the short wave-length rays are in focus, green when the medium wave-length rays are in focus, and orange-yellow when the short waves are in focus. In the first place it will be noticed that purple, which is one of the colours suggested by Dr. Ellison, is not a colour which would be expected, and in the second place that two of the expected colours, namely, green and orange-yellow, are not seen in association with scintillation. It will be recollected that the colours observed are blue-green and pink, and mixtures of these colours with white.

Let us now consider the time during which colours are visible according to Dr. Ellison's hypothesis. The cycle through which the atmospheric lenses pass consists of two phases: a divergent phase, and a convergent phase. During the former, no focusing of the light rays occurs. During the latter, foci are formed, but at first they are situated below or behind the observer. But as this phase continues, the focal lengths of the lenses progressively decrease until the focusing points approach the position which is occupied by the eye of the observer. The first rays actually to come to a focus on the pupil are the blue and violet rays, then follow in order the blue-green, the green, the yellow, the orange and the red. It is during this period that the first colour change occurs. With a further shortening of focal length the rays come to a focus in the space between the lenses and the observer, and no colours are visible. Shortly after this a reversal occurs, and the focal lengths of the lenses begin to increase at first slowly and then rapidly. The second colour change now takes place, but in the reverse order to the first colour change. With a further increase in the focal lengths the foci are formed beyond the observer, and soon reach infinity. The divergent phase now recommences, and so the cycle goes on. Now there are insufficient data to enable an exact calculation to be made of the time during which colours are produced in comparison with the total time of one complete cycle, but the former must be quite short, probably not more than 4 per cent of the total time. So that if thirty complete cycles occur per second, as Dr. Ellison suggests, then there will be sixty colour flashes per second, each of which has a duration of less than one-thousandth second. Would colour changes be visible at all in such circumstances? Experiments with rotating disks show clearly that blending of colours

occurs when colours are presented to the eyes in rapid succession. The success of 'Technicolor' depends on this fact. Moreover, observations on such stars as Sirius show that colours are not visible all the time; but they definitely appear to be visible for longer periods than the above values would suggest. So that in this respect also the hypothesis appears to be at variance with observation. There is one further point, namely, that on Dr. Ellison's hypothesis it would be expected that neighbouring stars would show similar, if not identical, changes of brightness and colour. But this they do not do. On all the above grounds, it must be concluded that this new hypothesis is no more in keeping with observed facts than the parent hypothesis advanced by R. W. Wood.

Mr. G. S. Brindley's letter is valuable from several points of view; in the first place, he describes a simple way of observing a given star by means of two light rays which are only a short distance apart, and in the second place he provides additional proof that an observer can, in fact, tell when the images at the eyes are synchronized and when they are not. This agrees exactly with my views, but I am particularly glad to have independent confirmation of them. According to some tests which I have performed with a rotating sector, it is easy to decide whether the star images are altering in brightness synchronously or not, provided that the alterations are not quicker than, say, three to five cycles per second. If they are more rapid than this, say, ten cycles per second, then it is difficult or impossible to tell whether they are altering at the same time or independently. Now Mr. Brindley finds that the irregular bands of light and darkness are 2-10 cm. in width, that is, 4-20 cm. from the centre of one bright band to the centre of the next. Now, as stated above, in order that the observer may be able to decide whether the bands seen by the two eyes are synchronous or not, they must not arrive at the eyes more rapidly than about five bright bands per second. If, then, the centres of the bright bands are 4 cm. apart they must travel at a speed not in excess of 20 cm. per second, and if they are 20 cm. apart their speed must not exceed 100 cm. per second. How do these speeds compare with those arrived at by Wood? He writes, "Just before the total stage of a solar eclipse the bands are seen to travel over the ground with a velocity of usually from 10 to 20 ft. per second". It will be seen at once that these speeds are much greater than those arrived at on the basis of Mr. Brindley's data. Here, then, is one of the inconsistencies encountered in the theory of atmospheric turbulence.

Now to me it is an astonishing idea that while wind velocities just above ground-level may vary from almost zero to quite high speeds, those of the upper atmosphere which contain the striæ are quite low, being only a few feet per second. If this is true, it certainly requires some explaining.

Here is another inconsistency: according to Wood, the widths of the bands are frequently not more than 3-4 cm. Mr. Brindley in his letter puts them at 2-10 cm. Dr. Gaviola concluded that they vary between 5 and 50 cm. So far agreement does not appear to be too bad, but there is another important set of observations to be considered: Schlesinger found that the plots of the trails of Merope and Aleyone showed almost identical displacements. "Small differences in the two paths of the stars are attributed to errors in measurement," he writes, and "as in the case of the low oscillations which we have

discussed, we have convincing evidence that this path is very precisely the same for all the stars on the photographic plate". Now for the paths to be the same over a substantial part of the sky, the striæ would have to have correspondingly large dimensions, subtending at the eye angles in excess of twenty minutes of arc, being, say, ten metres at least in width if the striæ are situated 2,000 metres above the observatory. Now these large dimensions do not agree with the much smaller ones referred to above. Nor do they agree with the small dimensions which are required in order to explain the presence of marked scintillation of the stars, but its almost complete absence in the case of the planets. It may be contended that there may be both small and large striæ present at one and the same time, but this cannot be accepted because if there were small striæ they would disturb the star images on a photographic plate. Here then is another inconsistency in the turbulence theory.

I would now like to report a recent experiment in which atmospheric effects have been largely, if not completely, eliminated. Into a brass tube were placed: a miniature tungsten-filament electric-light bulb and a number of microscope objectives in series so arranged that a greatly diminished aberration-free image of the source was presented to the observer's eye. Both ends of the tube were closed and were made vacuum-tight, at one end being a glass window through which the observer could look. The brass tube was connected to a vacuum pump and the pressure of air inside was reduced to a few millimetres of mercury. As a further precaution, an electric fan was placed close to the observer so as to blow away any warm air which might be rising between the end of the tube and his eye. The removal of atmospheric effects did not alter in any way the appearance of scintillation as seen on looking down the tube, nor was the sudden rush of air into the tube on disconnecting the vacuum pump accompanied by any visible effect. We conclude that the turbulence of the atmosphere plays no observable part in the changes of brightness and colour which are seen to affect a local light source.

While, on one hand, these experiments appear to provide proof of a physiological type of scintillation, on the other hand, our recent observations of the twinkling of true stars and their comparison with the twinkling of local sources has convinced us that the true stars twinkle more effectively than the artificial ones. In other words, as we have thought all along, there appears to be a physical cause of twinkling as well as a physiological one.

To sum up, I am quite convinced that the theory of irregular atmospheric refraction does not explain all the observed phenomena. It does not account for the colours of scintillation being limited to red and blue-green. It does not explain why small stars twinkle less than large ones. It does not cover the scintillation of a local source viewed through a vacuum tube. In all these respects and many others besides, it is incompatible with known facts. It seems reasonable to suppose that the atmosphere is one factor in company probably with several others, some physical and some physiological, which together produce the stellar changes which are observed in position, in shape, in size, in brightness and in hue. That physical factors play a part, as we have believed all along, is clearly shown by the facts mentioned by Prof. Minnaert that changes of star brightness can be photographed and can be recorded by means of

a photocell. That physiological factors also may come in is shown with equal clearness by the vacuum-tube experiment described above.

Now what could the other physical factors be? Could it be that the stars themselves emit light in an irregular manner? Some hold the view that the sun contains light under high pressure and that this escapes in jets; could the same process be occurring in the stars? Are there starspots as well as sunspots? We know that light, since it possesses energy, must also possess mass, and we know that gravitation affects light by virtue of that mass. Is it not possible, then, that the mass of one lot of light attracts the mass of another lot of light, and thus in course of time produces what may be called 'light aggregates'? The seismograph shows that the earth's crust is in a constant state of tremor, and therefore an observatory, since it is attached to one part of the crust, must be in a constant state of tremor too. Would this not cause the star images to undergo synchronous tremulous movements of the kind recorded by Schlesinger? Here, then, are some tentative hypotheses, probably of little weight in themselves, but suggesting the kind of survey which I think is wanted. Every part of the system should be scrutinized: source, light path, telescope, camera and eye, because any one of these may be contributing to the phenomena of scintillation.

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OBITUARIES

Prof. C. S. Gibson, O.B.E., F.R.S.

CHEMISTS, not only in Great Britain, but also abroad, will have learnt with deep regret of the death on March 24 after a long illness of Prof. C. S. Gibson. He was born on February 8, 1884, and was educated at the Manchester Grammar School, where he was fortunate in coming under the influence of that great chemistry master, Francis Jones. With a natural science scholarship he went in 1901 to Corpus Christi College, Oxford, graduating with honours in 1905. Shortly afterwards he became research assistant to the late Sir William Pope, then professor of chemistry in the College of Technology in Manchester. This was the beginning of a life-long friendship. Holder of a Senior Goldsmiths research studentship, Gibson followed Pope to Cambridge, where he became a member of Sidney Sussex College and later an assistant lecturer and demonstrator in the University Chemical Laboratory. He remained in Cambridge until 1912, when he was appointed professor of chemistry in the Maharaja's College, Trivandrum. Here he found the conditions to be very primitive, and it was only by frequent visits to my laboratory in the Presidency College, Madras, that he was able to continue his original work.

At the outbreak of the First World War, Gibson returned to Cambridge and, in collaboration with Pope, he was responsible for work of first-class importance in the manufacture of mustard gas. Unfortunately, he was badly gassed on more than one occasion and he suffered from the effects for the remainder of his life. In 1919 he was appointed