

The unaided eye part III

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THE UNAIDED EYE

PART III*

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SYNOPSIS

Coincidence observations; description of apparatus and safeguards. Coincidence accuracy observations with results. Variables that affect the observations. Distinction between precision and adjustment errors. Variation of precision with length of thick coincidence lines, similar results for thin lines. Minimum precision error relatively to length of coincidence line. Effect on precision of separating line thickness. Adjustment errors. Effect on adjustment of separating line thickness. Variation with length of line. Variation with thickness of line. Effect of premature judgment. Verticalising power of eye. Relation to coincidence adjustment error. Effect of astigmatism on error.

Theories of vision. General review of subject. Statement of proposed series theory. The location of images independent of cone diameters. The form apparatus. The colour apparatus. Its explanation of red, green and blue colour blindness, monochromatic threshold vision. Anomalous trichromatic vision. Polychromatic foveal vision. Contrast phenomena. Suggested functions of retinal layers of ganglion cells. Their relation to contrast and irradiation phenomena. Axis cylinder of nerve fibres.

WHENEVER an accurate measurement is desired, it is well known that the best results are obtained by the use of coincidence methods when such methods can be employed. For the accurate reading of a graduated circle a vernier is used, and the observation then depends upon the discrimination between the continuity or discontinuity of a line on the circle and one on the vernier. The application of coincidence methods of observation to range-finders is already well known, and as the application of such methods is becoming more general, as for example, in the reading of the bubble of surveying instruments, the particulars of certain systematic experiments recently carried out by the writer on the coincidence or aligning power of the unaided eye may be of interest to the members.

As it is essential in precision experiments that the genuineness of the observations should be unquestionable, the arrangement of the coincidence apparatus employed will first be described. However honest the observer is, his readings may be unconsciously influenced by circumstances such as the knowledge that in the previous observation the operating handle occupied a particular position.

Figs. 1 and 2 show the coincidence apparatus employed, the former being a front view and the latter a plan. *A* and *B* are two metal plates capable of relative movement along a well-fitted line of separation *CD*. Lines *E* and *F* were engraved, painted or mounted as the case might be upon the matt surfaces of the plates. Black

* Parts I and II appeared in *Trans. Opt. Soc.* 20 (1919), 209 and 21 (1919), 1 respectively.

lines upon a white ground were most commonly used, but white lines on black grounds and contrast colours were also tested. Whereas plate *A* was fixed, plate *B* could be translated horizontally by means of an electrically operated step by step motor *G* acting through the intermediary of a pinion *H* and wheel *J* and a micrometer screw *K* working in a nut *L* integral with the translatable plate *B*.

The gearing was such that one step of the motor corresponded with a displacement of the plate of one ten-thousandth of an inch, which represents an angular displacement of one-tenth of a second subtended at a distance of 200 inches.

From the motor *G* a flexible cable was carried to a transmitter at the observer's station, the distance of which was varied from 8 to 115 feet. By rotating the transmitter handle forwards or backwards the plate *B* could be translated step by step towards the left or the right as required at any speed up to 20 steps per second.

For the determination of the displacement of the plate *A* relatively to *B* there was provided a microscope *M* mounted rigidly upon *A*, and so arranged as to

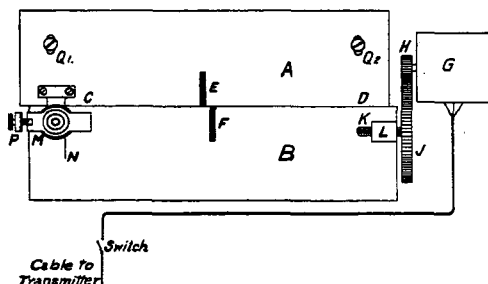


Fig. 1. Front view.

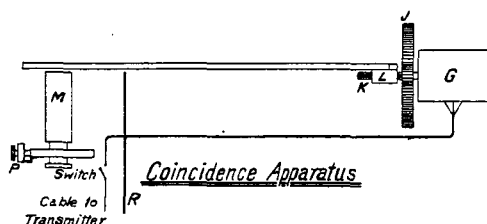


Fig. 2. Plan.

observe an index line *N* engraved upon *B*. The position of the line was measured by means of a micrometer eyepiece *P*.

Adjustment of the width of the separating line *CD* was effected by a vertical movement of the plate *A* which was clamped in position by the screws *Q*₁, *Q*₂, the necessary slotted holes being provided in the plate.

Now to prevent unconscious manipulation of the observations there was provided in the electrical connections between the motor *G* and the transmitter a switch controlled by the separate microscope reader, who could interrupt the circuit at frequent intervals during the operation of making the coincidence observations. The movement of the switch could not be seen by the coincidence observer, and the microscope scale reader could not see the coincidence objects which were shielded by a diaphragm *R*. After each setting of coincidence a signal was transmitted to the microscope scale reader, who then noted the indication.

In the first instance a series of observations was made with the particular object of obtaining some idea of the greatest precision that could be realised, and for this purpose no limit was imposed as regards the time taken in making the several observations. An error of observation may be divided into two components—one

the precision error which indicates the average accuracy with which an observation can be repeated irrespective of its value, and the other the adjustment error, which represents the deviation of the mean observation from the true known value. The method of reckoning these two types of errors has already been described in the second lecture.

Black lines having a width of 0.43 inch mounted on a white background were used and the separating line was made as fine as possible, its thickness being about four-thousandths of an inch. Nine sets of observations lettered A to I, each comprising five readings, are included in Table I. Set A is given in full, but columns (3) and (4) alone are indicated in the subsequent sets. For the first three sets A, B and C, one revolution of the micrometer head equalled 0.0023 inch. The distance of the observer was 112 inches, therefore one revolution of the micrometer head represents a subtended angle of 4.32 seconds. For the sets D, E, F, G, H and I a different microscope was used, one revolution of the micrometer head of which represented a displacement of 0.0056 inch. The distance was 488 inches and therefore one revolution of the head represents a subtended angle of 2.36 seconds.

Table I

In sets A, B and C one div. = 4.32 secs.

A				B		C	
(1)	(2)	(3)	(4)	(3)	(4)	(3)	(4)
Index reading 17.18 revs. of micrometer head	Actual reading	(2) — (1)	Diff. from mean				
	18.44	1.26	0.06	1.22	0.43	0.75	0.29
	.44	1.26	0.06	0.58	0.21	0.95	0.09
	.54	1.36	0.16	0.76	0.03	1.05	0.01
	.35	1.17	0.03	0.70	0.09	1.13	0.09
	.12	0.94	0.26	0.68	0.11	1.30	0.26
	Mean	1.20	0.114	0.79	0.17	1.04	0.15
Equivalent seconds 0.49				0.74		0.65	

In sets D, E, F, G, H and I one div. = 2.36 secs.

D		E		F	
(3)	(4)	(3)	(4)	(3)	(4)
1.58	0.16	1.46	0.26	1.90	0.27
1.27	0.15	1.57	0.15	1.75	0.12
1.12	0.30	1.97	0.25	1.76	0.13
1.79	0.37	1.67	0.05	1.20	0.43
1.32	0.10	1.95	0.23	1.53	0.10
Mean 1.42	0.21	1.72	0.19	1.63	0.21
Equivalent seconds 0.5		0.45		0.5	

Table I (continued)

G		H		I	
(3)	(4)	(3)	(4)	(3)	(4)
1.60	0.02	2.13	0.22	2.28	0.01
1.45	0.17	2.46	0.11	2.42	0.15
1.33	0.29	2.70	0.35	2.72	0.45
1.42	0.20	2.20	0.15	2.05	0.22
2.30	0.68	2.25	0.10	1.87	0.40
Mean	1.62	0.27	2.35	0.19	2.27
Equivalent seconds 0.64		0.45		0.59	

Table II

Extreme difference of readings in each set of five

A	B	C	D	E
1.36-0.94	1.22-0.58	1.30-0.75	1.79-1.12	1.97-1.46
Divs. 0.42	0.64	0.55	0.67	0.51
Secs. 1.82	2.77	2.38	1.58	1.20

F	G	H	I
1.90-1.20	2.30-1.33	2.70-2.13	2.72-1.87
Divs. 0.70	0.97	0.57	0.85
Secs. 1.65	2.29	1.35	2.00

As already stated it is only the precision that is of interest at the present moment. The observer was not concerned with the absolute value of the readings, but only with the precision with which the readings could be repeated. In the first series A, B, and C the respective precisions for sets of five observations were 0.49, 0.74, and 0.65 seconds of angle at the unaided eye, the average thus being 0.63 seconds. From Table II it will be seen that the extreme variations of the readings of each set were 1.82, 2.77 and 2.38 seconds, the average being 2.32 seconds.

In the second series of readings D to I, taken under different circumstances, the respective values of the precision are 0.5, 0.45, 0.5, 0.64, 0.45, and 0.59 secs. and the average is 0.52 seconds. The extreme variations of the readings in each of the respective sets are 1.58, 1.2, 1.65, 2.29, 1.35, and 2.0, the mean being 1.68 seconds. The average precision for the nine sets is 0.58 seconds.

In the foregoing precision tests, the only purpose of which was to attain the greatest possible precision, light from the object alone reached the observer's eye, all extraneous light being excluded. Throughout subsequent tests extraneous light was only excluded by means of window blinds and the conditions were therefore more nearly normal.

In all cases the coincidence observer had no knowledge of the readings or any information as to whether his performance was good or bad. It will be understood

that in the subsequent tests about to be described the main object in view was to determine the comparative effect of a variety of conditions upon coincidence. Of these conditions the principal ones that were varied in the tests were as follows, but owing to limits of space only a selection of the results will be included in this lecture:

- (1) The width of the coincidence line.
- (2) The height above or below the separating line of the coincidence line.
- (3) The width of the separating line.
- (4) The distance of the observer.
- (5) The colour contrast, such as black or white.
- (6) Verticality of the coincidence lines. A small sidewise inclination may seriously affect the results.
- (7) Illumination.
- (8) Effect of horizontal, vertical, and oblique reference lines.
- (9) Position of the observer's head. A sidewise inclination of the head may affect the results.

Precision observations as distinct from adjustment error observations will first be considered.

Fig. 3 represents a typical series of results obtained by an alteration of the height of the coincidence line measured from the separating line, and therefore of the angle subtended by the coincidence line height at the observer's eye situated at a distance of 200 inches from the object. Similar curves are shown corresponding with five different widths of separating lines, subtending angles of 4, 82, 185, 526, and 1130 seconds respectively. Ordinates represent angular heights expressed in minutes and abscissae the precision in seconds of angle. The thickness of the coincidence line was 0.43 inch.

A similar series for a much finer coincidence line having a thickness of only 0.05 inch is indicated in Fig. 4, the distance of the observer being again 200 inches. As before each curve corresponds with a particular thickness of separating line, the thicknesses in the two series being approximately though not exactly the same.

Reverting to Fig. 3 it will be seen that in all the curves, as the height of the coincidence line is diminished, the precision error increases, until a point is reached where the height subtends an angle of about 0.6 minute, under which conditions the eye appears incapable of observing with precision any want of alignment. In the case, however, of the very broad separating line, subtending 1130 secs., the limit appears to be reached at an angular height of about 2 to 3 minutes, and a similar but smaller tendency is observable in the 526 seconds curve.

That there should be greater difficulty in aligning the broad partial images the thicker the separating line, is to be expected, but this is not confirmed in the second series, Fig. 4, where it should be observed that the coincidence line was much thinner, being 0.05 inch as compared with 0.43 inch. It will be noticed that in the latter series the lower limit at which the resolution fails is more constant than in the case of the thick coincidence line.

As the height of the coincidence line increases the precision error decreases,

but for each width of separating line there is a limit beyond which an increase in the height has no further influence on the accuracy. Thus from the curves of Fig. 3 it will be seen that in the case of the thickest separating line, namely 1130 secs., the precision error decreased as the height of the coincidence line was increased until at an angular height of about 12 minutes a limiting precision of 5.1 seconds was reached. An increase of the height had thereafter practically no effect. It would appear that, in estimating the alignment, so far as precision is concerned, the eye takes no account of the portions of the coincidence line beyond this limiting angular height. In the case of the thinnest separating line, namely 4 secs., the limiting precision is about 1.1 seconds at an angular height of about 9 minutes.

The curves *F* of Figs. 3 and 4 indicate how the minimum error of precision varies with the angular width of separating line. The thinner the separating line

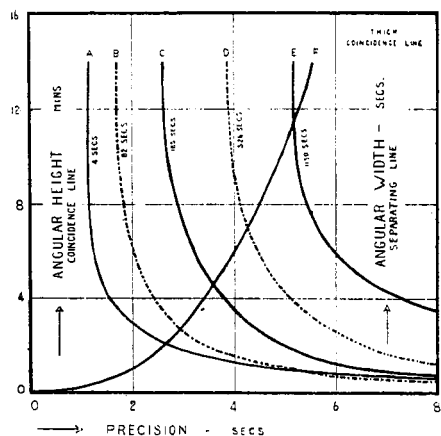


Fig. 3.

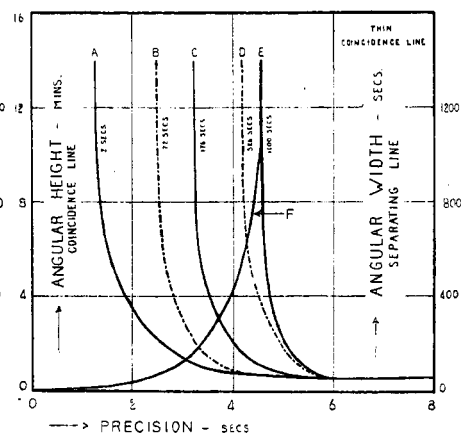


Fig. 4.

the greater the precision attainable, and to a slight extent, the smaller the height of the coincidence line necessary for the attainment of this limiting precision.

Owing to the limits of space it will not be possible to give particulars of coincidence results taken under the various conditions. Fig. 5, however, may be referred to briefly as indicating how the precision of coincidence observations is directly affected by the thickness of the separating line.

In the particular example illustrated the distance of the observer was 200 inches and the thickness of the coincidence line was 0.43 inch, which thus subtended an angle of 7 minutes. Three curves corresponding with coincidence lines of three different angular heights, namely 15.8 minutes, 4.28 minutes and 0.85 minutes respectively, are indicated. As is to be expected from the previous diagram, the very short line is widely separated from the longer lines subtending more than 4 minutes. Ordinates represent the thickness of the separating line expressed angularly in seconds, and abscissae the precision of coincidence setting. The curves in question have been smoothed and only indicate the general tendency. As the

thickness of the separating line increased the precision error increased, the increase being comparatively regular in the case of the very short coincidence line. For the longer lines the precision error tends towards a maximum value in each case, the maximum for the longer line being about 6 seconds, and for the intermediate line about 9 seconds, but too great importance should not be placed upon these particular results as consistent readings could only be obtained with difficulty when the separating thickness was great.

From the practical point of view the adjustment errors are possibly of more interest than the precision errors where absolute measurements are concerned. There is more prospect and necessity for an improvement in the comparatively large adjustment errors of coincidence than in the precision errors.

As the width of the separating line is increased the adjustment error increases approximately in direct proportion, but the rate of increase is very variable, not only for different lengths and thicknesses of the coincidence line, but also for

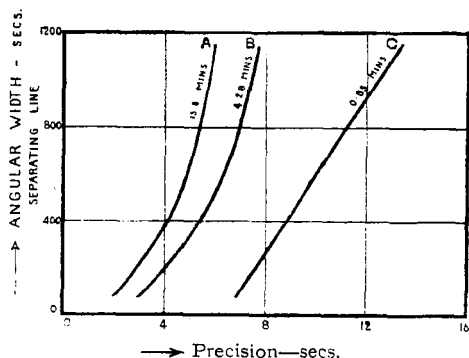


Fig. 5.

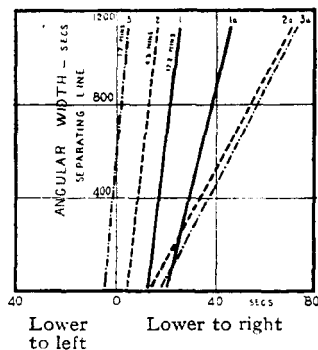


Fig. 6.

different conditions and observing eyes. This is illustrated in Fig. 6. Ordinates represent the angular thickness of the separating line which was varied from a few seconds up to about 1100 seconds. Abscissae represent the adjustment errors in seconds of angle. Those on the right were obtained by moving the lower image into coincidence from the right and similarly those on the left by movement of the lower from the left. It will be observed that the curves of each pair are not disposed symmetrically on the left and right of the zero ordinate. For this particular eye the curves which normally should lie on the left-hand side lie almost wholly on the right. There is an apparent displacement of the mean zero of about 20 seconds for the finest separating line. As the thickness of the separating line is increased the divergence and displacement appear in general to increase, but the results obtained by different eyes vary greatly, the primary cause being probably visual abnormalities. The change in the slope of the mean zero line of each pair of curves for the coincidence lines of different heights was characteristic of other sets of observations. That is, as the length of the line was diminished, the mean zero line

became more oblique. The difference in the slopes of the curves of each pair should also be remarked.

The variation of the adjustment error with the angular height of the coincidence line is indicated in Fig. 7, where the abscissae are as in the previous diagram, and the ordinates represent angular heights, in minutes, of the coincidence lines. The curves of each pair are separated as the result possibly of premature judgment, and when the height is very small the divergence increases rapidly and may become very great. Three pairs of curves for three different thicknesses of separating line, namely about 1 second, 526 seconds and 1130 seconds respectively, are indicated. All three pairs are displaced relatively to the zero line and towards the right side. The curves in question are for a particular eye. Curves similar in form but having entirely different values particularly as regards their displacement may be obtained for the separate eyes of one observer.

There is a natural tendency to be prematurely satisfied with the setting of the images, and as any attempt to compensate this tendency was avoided a difference

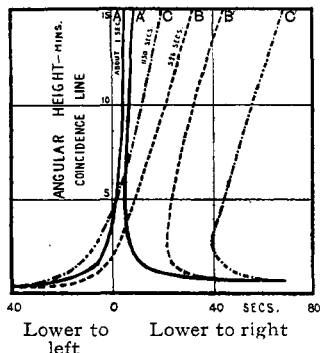


Fig. 7.

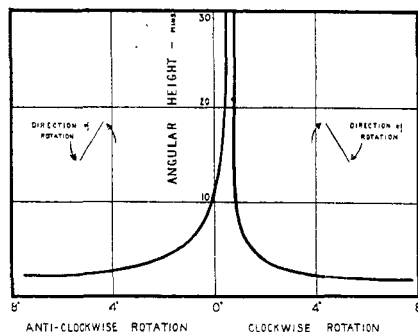


Fig. 8.

between the two sets of readings obtained by moving the image into coincidence from one side and the other is to be expected. From the diagram it will be seen that the interval between the right and left curves is comparatively great when the separating line is thick, and that the interval is only about ± 2 seconds when the separating line thickness is less than a second. Under the conditions of these particular observations illustrated in Fig. 7, the adjustment error in the case of the very fine separating line, curves A and A', is practically independent of the height of the coincidence line except when the height is less than about 3 minutes. That is to say the curves, except at the lower end, are parallel to the vertical zero line from which, however, they are displaced by a small amount towards the right. In the case of the thicker separating lines the curves above the 3 minute position are parallel to an oblique mean line. The displacement of the mean lines from the zero and the separations of the curves of each pair increase with the thickness of the separating line.

Similar characteristic curves have been obtained for white separating lines with

white coincidence lines on a black background, instead of black lines on a white ground, such as were used in the experiments described, and also for single edges instead of lines. The results differ principally in degree, being in general the more accurate the finer the lines.

From the results of other experiments it was concluded that the displacement of the mean adjustment line was related in some way to the verticalising power of the eye. It is well known that a line which appears vertical to the one eye may appear inclined sideways to the other eye, and that the verticalising power of different eyes varies considerably, from 0 to about 2 or 3 degrees in either the clockwise or counter-clockwise direction. That the amount and character of the adjustment error were very variable was clearly demonstrated by a comparison of the observations of a large number of persons.

To obtain more precise information regarding the verticalising power of the eye, the observer was placed at one end of a black chamber having at its opposite end an externally illuminated line, the orientation of which could be recorded by a separate operator. As there were no reference marks whatever that would aid the observer in the setting of the line, the verticalising sense could only be attributed to the action of gravity on the body of the observer who was seated with the feet not resting on the ground, or to the action of gravity on the contents of the semi-circular canals of the ears or on the eye and its contents. Fig. 8 shows the results of a series of verticalising experiments made upon lines of various lengths. Ordinates represent the angular heights of the various lines expressed in minutes of subtended angle, and abscissae to the right and left of the zero centre line represent angles of rotation of the line in a clockwise or anti-clockwise direction.

There is a remarkably close resemblance between these verticalising curves and the coincidence adjustment curves previously illustrated, particularly as regards the displacement of the mean ordinate. As the length of the line is diminished the estimation of the vertical is not appreciably affected until the height subtends about 5 to 10 minutes, when the verticalising errors rapidly increase. In the case of the coincidence adjustment experiments there was the same rapid increase of the error when the height of the coincidence line was reduced below 5 minutes. It will be seen that this particular observer, whose eyesight was reported by an oculist to be very good, was able to verticalise a line to within about 40 minutes in the case of the longer lines, and it would be interesting to know to what sense this verticalising power of the eye is attributable.

Curve *A* of Fig. 9 shows the precision with which the setting of the vertical could be repeated. The minimum precision error of about 10 minutes is reached in this instance when the coincidence line has an angular height of 20 minutes or more. Below 20 minutes the error rapidly increases. The curve *B* represents the same observations plotted logarithmically. Curve *A* can therefore be represented by the equation

$$\text{precision error} = \left(\frac{316}{\text{angular height of coincidence line}} \right)^{0.9}$$

where the precision and height are expressed in minutes of angle.

It was concluded that the displacement of the zero was due to some defect of the particular eye, and that the defect in question was one of astigmatism. This conclusion was tested and confirmed by placing in front of the observer's eye astigmatic lenses which could be rotated by definite amounts. It was found that the mean adjustment line in the verticalising and coincidence experiments could be shifted from one side of the zero line to the other. Fig. 10 shows the effect in coincidence adjustment experiments of the rotation of a 0.75 dioptré astigmatic lens placed immediately before the observer's eye. Ordinates indicate the angular position of the axis of the cylindrical lens, and abscissae the position of the mean adjustment line. The desirability of correcting the defects of the eye when making precise absolute observations was already well known to the writer, more particularly in range-finding observations, but it should be remembered that such correction must be made with the greatest care and judgment, as otherwise the errors, instead of being improved, may easily be increased.

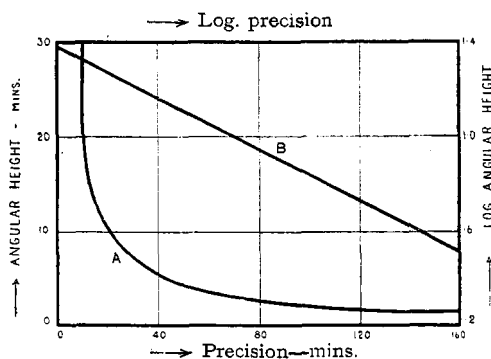


Fig. 9.

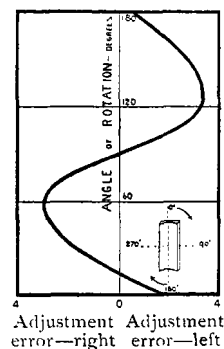


Fig. 10.

As I wish to consider certain theoretical aspects of the mechanism of vision it will not be possible on this occasion to discuss the variations of precision and adjustment under other conditions than those already mentioned, such as, for example, the variations dependent upon the distance of the observer, or the effect of oblique lines of reference, or to deal with the power of the eye in the discrimination of objects of various forms.

It is hardly possible to make measurements of the performance of the eye without being tempted to imagine a mechanism that will explain them. If the existing theories satisfactorily explained the great variety of phenomena that have been recorded by various workers, to add to the number would not be justified. But the very fact that so many theories have been propounded is in itself evidence that no one theory is at present wholly acceptable.

It is unnecessary to refer in detail to these theories which have all been the subject of much controversy; a brief summary must suffice. First there is the pioneer trichromatic theory of Dr Thomas Young, who presupposed the existence of three

types of retinal receptors capable of appreciating red, green, and violet light vibrations. Each receptor apparently responded solely to a particular vibration, and difficulty accordingly was experienced in explaining certain colour mixture phenomena. Helmholtz to some extent overcame these difficulties by assuming that all three receptors responded to each of the three fundamental vibrations, but in varying amounts. Thus red vibrations according to their intensity affect the red receptors to the full extent, and the green and violet only slightly. Clerk Maxwell, Fick, and König substituted blue or indigo blue for violet as one of the fundamental colours, and the last mentioned worker derived from his experimental results a series of excitability curves showing the extent to which the three fundamental types of receptors of the retina are affected by light of any particular spectral colour.

Many writers have emphasised the existence of a white as distinct from a colour sensation, or the white arising from the admixture of fundamental colours. Objections to the trichromatic receptor theory have been based on the difficulty of explaining the peripheral and threshold white light sensation of the eye, and the absence there of all colour sensation. Captain Abney with others suggests that the retinal apparatus must be able to appreciate white light in addition to the three fundamental colours and their combinations, in which a white sensation must also be included. Hering met this supposed difficulty by introducing a theory which appeals particularly to the physiologist. His theory is based on the existence of three pairs of contrasting sensations, white and black, red and green, yellow and blue. According to Helmholtz, Hering's red was what is generally called purple. The first sensation of each pair is the result of katabolic action, that is, a dissimilative or molecular reduction of the living visual substance to a less complex and more stable condition, whereas the second sensation of each pair is due to a reverse anabolic or assimilative change of the substance to a more complex and less stable state. This theory presumes the existence in the retino-cerebral system of three kinds of living visual substance capable of responding katabolically or anabolically to the elementary light vibrations of each pair respectively. Hering evidently regards black as a colour sensation rather than the mere absence of light sensation such as a blind man is afflicted with.

Kries has advanced a duplicity theory which discriminates between the functions of the rods and cones. The former, which are more numerous towards the periphery of the retina, are concerned, he suggests, with achromatic scotopic vision, that is, the vision of the dark adapted retina, whereas the cones are concerned with polychromatic photopic or light adapted vision.

Donders elaborated the Young hypothesis by adding certain psychological or cerebral sensations which necessarily increase the speculative character of a theory.

There are numerous elaborations of these typical theories which need not be referred to, but it is desirable to mention the distinctive theory of Dr Edridge-Green, according to which the function of the rod appears to be to liberate, under the action of light, visual purple which spreads over the otherwise light insensitive cones which are stimulated, however, by the decomposition of the visual purple when light falls upon the retina.

Practically all the theories advanced concern themselves with the appreciation of colour and but little with the appreciation of form, except in so far as some writers consider it sufficient to state that appreciation of form is essentially equivalent to appreciation of colour contrast, black and white being regarded as colours. They have been established to a large extent upon the results of observations made with colour blind people of various types. Appreciation of form has received comparatively little attention from observers with the exception of Helmholtz, whose explanations have received general acceptance. In any theory of vision it is desirable that the appreciation of both form and colour should receive consideration.

Although Helmholtz's theory is so clearly explained in that classical work, the *Handbuch der Physiologischen Optik*, it is remarkable how frequently the theory is misstated. In view of this it may be of interest to quote the essential portion of Helmholtz's description, translated from the 1896 edition:

When two light-emitting objects, whose widths are negligible compared with their separation, are observed, they can only be recognised as being two objects when between the retinal elements which receive their images there remains one which remains dark. The diameter of such an element must therefore be at least smaller than the separation of the two bright images.

From this description it will be seen that there must be a retinal element corresponding with the dark interval between the objects, if the separate images are to be resolved, that is to say, in the resolution of two separate point images there are three features that have to be appreciated by the eye, namely, the two point sources and the intermediate dark space. Three retinal elements are therefore involved in the appreciation of the system. Helmholtz proceeds to consider objects whose width is equal to that of the dark strip between them, and concludes that all that can be said with certainty is that, at the limit of resolution, the retinal element must be smaller than the separation of the centre lines of the bright strips. This conclusion is based upon the assumption that a small part of the image falling upon one element will not be recognised if the greater part of the image falls upon the adjoining element, provided the quantity of light in the smaller portion is small compared with that of the latter. Doubtless this conclusion was considered necessary in view of the fact that there is a minimum limit to the diameter or width of an image that can be formed upon the retina owing to aberrational defects.

Neglecting the large chromatic aberrations, the bulk of which are not recognised by the retinal mechanism, as is demonstrated by Helmholtz, there is a well-defined enlargement of the image due to diffraction. Theoretically, for a normal pupil diameter of 4 mm. the angle subtended by the retinal image of a point object is about 1 minute, reckoned over the first dark ring. In the macula lutea the average individual cones also subtend an angle of one minute and as in this region they are in close contact, they are separated by this angular amount. Helmholtz accordingly assumed the resolving power of the eye to be 1 minute. This estimate is approximately in accordance with the results of contact observations. But as the diffraction image of a point object subtends about the

same angle, may not the results of these observations be merely a demonstration of this simple fact, and not of the actual resolving power of the eye, which may conceivably have no direct relation to the cone diameters? Is it possible in any case to produce upon the retina an image that subtends an angle of much less than 1 minute? Even if the resolving power were not determined by the diameter of the foveal cones, the relation of these diameters to the diameter of the maximum retinal image of an ideal point source may be of importance as determining the grain or texture of the retinal picture as distinct from the form. It should be observed in this connection that Helmholtz based his calculations upon the value of the cone diameters as determined by Koelliker, although the diameters as measured by other observers varied greatly, being in some cases less than one-third. This, however, does not negative Helmholtz's conclusions, since the contact resolving power of the eye as measured by various observers also varies considerably, although not to the same extent.

The accuracy of coincidence observations is well known: It is certainly less than one minute or even half a minute, and the precision with which a coincidence observation can be repeated is a question of seconds. It is not sufficient to say that coincidence observations are of a different type from contact or overlapping observations. That there are apparent differences is well known, although perhaps they are not so generally recognised in text books as they ought to be. For example, resolution of contact images is dependent upon the aperture of the optical system, whereas in the case of coincidence observations the precision of setting is not in the same way a function of the width of the aperture. If the objective which forms a fine line image in the coincidence field of view is reduced in diameter, the width of the image will increase, but it is about as easy to bring a thick line into coincidence as a thin one, and indeed the presence of fine diffraction marginal lines may even aid the coincidence observation.

Whatever the apparent differences between the various types of observation, it is desirable that any theory of the eye should take all aspects into consideration. Helmholtz himself considered it necessary to propound an alternative resolution theory, and it is interesting to note that this alternative theory is more frequently referred to by him in the later part of the work than is the former. In the section of the optic nerve where it pierces the choroid it is estimated there are about one million fibres. But the retina contains about three million cones and, neglecting the eighteen million rods which Helmholtz assumes may take no part in the resolution of form, it is evident that in the majority of cases several cones must be connected to each nerve. To meet this difficulty Helmholtz propounded his *alternative theory*. Suppose a layer of sensitive elements is overlaid by a network of interconnected nerve fibres; that this network on the one side is connected by nerve fibres to the more numerous sensitive elements of the retina, and on the other to the less numerous fibres of the optic nerve. Now if a retinal nerve element is excited, the effect will be transmitted to the nearest nerve fibres through several paths, and the relative amounts of the sensation in the nerves will depend on the respective lengths of the paths. The position of the excited element will then be appreciated by the

receiving mechanism with an accuracy determined only by the difference in intensity of sensation that can be appreciated.

In offering the following suggestions regarding the sensitive mechanism of the eye I have distinguished the appreciation of colour from the appreciation of form, and as regards colour I have adopted the assumption that the eye can recognise an impression of light, varying from a mere impression of grey white at the macula lutea to white light near the periphery, as well as three fundamental colours such as red, green, and blue and their combinations, including white light.

Practically all the reliable observational evidence so far accumulated points to the rods and cones of the retina as being the sensitive elements. It is convenient to distinguish by name the rods from the cones although in many respects they may have common characteristics and corresponding functions differing in degree.

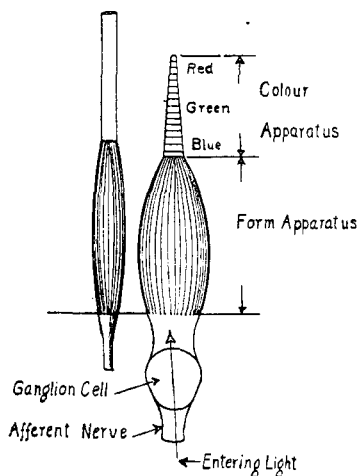
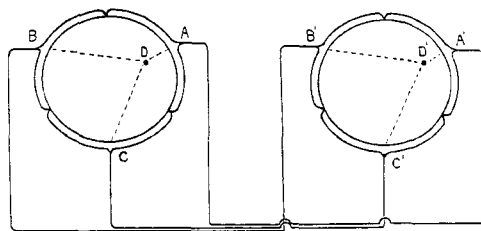


Fig. 11.



Retinal Transmitter Cerebral Receiver

Fig. 12.

Fig. 11 represents diagrammatically a rod on the left side of the illustration and a cone on the right. Each element has two distinctive parts—a thicker inner portion facing the incident light and a thinner outer terminal portion. In the case of the cone the inner portion is ribbed vertically and the outer portion is ribbed horizontally. If these cones and also the rods are desiccated with sulphuric acid, the terminal portions separate into layers, thus indicating the existence of transverse septa. Horizontal layers have only been indicated in the outer portion of the cones, although actually they are present also in the rods. Further the two portions appear to have different compositions. The outer end of the cone contains the most highly refracting substance found in the eye. It is stained brown by osmic acid, which does not affect the inner portion. This latter, however, can be coloured with aniline dyes. There is obviously a strong distinction possible between these portions. The outer portion I shall call the form apparatus and the inner the colour apparatus.

Suppose, as indicated in Fig. 12, which represents a cross section of the form apparatus, that three nerves or sets of nerves *A*, *B* and *C* radiate from, say, the walls of the cone under consideration, and suppose that an entering ray of light traverses the point *D*. The action of the light upon the contained living substance is transmitted to the walls in a form capable of exciting the associated nerves. If the nerves *A*, *B* and *C* are connected to a similar cerebral mechanism, the apparent position of a point *D'* corresponding with *D* would be determinable with an accuracy independent of the diameter of the element and dependent only upon the precision with which the sensations transmitted by the nerves could be appreciated, that is to say, the position of a point might be located to a fraction of the diameter of a cone.

Although it is not essential to know the means by which the light vibrations are converted into a form capable of exciting the nerves, it is interesting to recall the theory recently propounded by Sir Oliver Lodge, who suggests the improbability of stimulations being due to the motion of any gross particles of the visual substance of the rods and cones under the action of the rapid vibrations of light. It is more probable, he thinks, that bodies moving more nearly in accordance with the velocity of light are involved. As is well known, the atoms of matter are now supposed to comprise a positive nucleus surrounded by a system of electrons of negative electricity revolving like planets round the nucleus with various velocities, the inner ones moving with velocities comparable with that of light. Under the cumulative effect of the light vibrations the equilibrium of these corresponding inner electrons may be so affected that they break loose and being projected from the atom, they may impinge upon the nerves disposed around the walls of the cell or dispersed throughout the mass.

A three wire system of the type illustrated in Fig. 12 for the location of position appeals to an engineer. If called upon to satisfy the conditions referred to, it is the system he would naturally adopt. But it is not possible to form an ideal point image on the retina, and even if a point image were formed it is possible that the cerebral apparatus would appreciate it as a minute area. The three wire system will only locate the centre of light intensity of any area upon which the light falls and it is possible that the brain would appreciate the area as a spot having its centre located at the centre of intensity of the image. Suppose for example the image of a broken edge falls upon the fovea centralis where the cones are pressed together, as in Fig. 13, and have presumably a hexagonal section, as has actually been observed to be the case in the fovea centralis. On the assumption that a fully illuminated cone will produce a cerebral impression of a spot of light, and a partially illuminated cone a spot of smaller diameter, proportional to the quantity of light falling upon the cone, and having its centre corresponding with the centre of intensity of the illuminated cone section, the cerebral appearance of the discontinuous line of Fig. 13 *a* will be as indicated in Fig. 13 *b*, from which it appears that a want of alignment of a fraction of a cone diameter can be recognised as a bend in the line. Instead of a sharp break the visual impression should be that of a rounded or oblique break. When the partial images subtend an angle of one minute

as in Fig. 13 *c*, the cerebral impression should be akin to that of a rhomboid, as indicated in Fig. 13 *d*. This is very easily tested by the observation of short coincidence lines and when this is done the rhomboidal appearance is indeed very striking. If the angular height of the lines is about 1 minute, it is hardly possible to distinguish between a want of coincidence and a simple rhomboidal form. The greater the want of coincidence the more oblique is the rhomboid, and the act of bringing short images into alignment really consists in the rectification of a rhomboid. This applies only to the case of very short lines subtending one or two minutes of angle, because when the lines are longer very close concentration is necessary to observe any rhomboidal appearance at the junction of the lines.

If the image of a grating in which the black and white spaces are about equal is considered it will seem that, when the spacings, as in Fig. 13 *e*, subtend about 1 minute, the cerebral impression should be that of a wavy line, Fig. 13 *f*, and under some conditions a knotted line. These impressions have been actually observed and recorded by Helmholtz. The same wavy appearance is often noticeable in observations of lines under natural conditions.

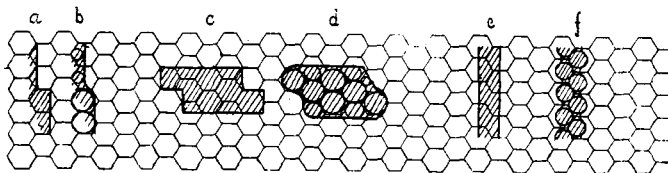


Fig. 13.

The function of the form apparatus is, I suggest, two-fold. Not only does it locate the position of the image falling within the confines of each cone of the macula lutea, but it also gives the impression of a grey or white light irrespective of the colour of the actual light. Over the central regions of the retina the form apparatus exercises primarily its function of locating the image, and conveys only in a secondary way the impression of light, which may under normal conditions be a negligible impression in comparison with the sensation of light transmitted by the colour apparatus. Near the periphery of the retina the rods preponderate greatly over the cones to which they have a characteristic resemblance. It is possible that the form apparatus of the rods may function primarily with regard to the impression of white light they convey, and only secondarily, if at all, with regard to the location of the image. Thus, neglecting the colour apparatus meanwhile, the central regions of the retina should recognise form well and the white impression of light only comparatively well, while the peripheral regions should recognise white light well and form badly. The small number of peripheral cones may aid in the detection of such form and movement as is recognisable over the outer regions. A gradual, although not necessarily regular, transition from the centre outwards might be expected in the case of both functions.

Now let us consider the outer or colour apparatus of the cones. A ray of, say, white light, which penetrates the form apparatus, will traverse the whole length of

the colour apparatus if of sufficient intensity and may pass through into the pigmentum epithelium, where it would be absorbed or utilised in other ways. It is reasonable to presuppose throughout the length of the colour apparatus a more or less continuous transition in the character of the visual substance as regards its response to light of the various wave lengths. Suppose therefore that the colour apparatus is divided into three portions connected to the brain by three nerves or groups of nerves respectively; that the outer or terminal portion responds to red light, the intermediate to green, and the inner to blue, these sensations being arranged in the order of the wave lengths to which they primarily respond. It will be understood that possibly no sharp line of demarcation between the compartments exists. In accordance with the recognised colour intensity curves, as for example those of König and Helmholtz, a ray of unsaturated red light would produce a large effect upon the red end; a small effect upon the green end and a negligible effect upon the blue or inner end. Captain Abney in his work on *Colour Vision* explains by the analogy of a double pendulum system how it is conceivable that more than one vibratory mechanism may respond to wave motion of a period with which they are out of tune. A ray of white light which passes through the form apparatus and traverses the whole length of the colour apparatus would stimulate all three sets of nerves and produce the sensation of white. A ray from the blue region of the spectrum would excite the inner end and to a small extent the middle section but would leave the extreme or terminal red end practically unaffected.

So far as colour appreciation is concerned there is an abundance of data already obtained from the observations of the various types of colour blind persons. The great majority of such persons are incapable of appreciating wholly or partially a red sensation although they can distinguish the other fundamental colours and their combinations. Cases of red and green blindness are not numerous, and the absence of all three colour sensations in a healthy eye capable of vision is a rare phenomenon.

On the basis of the proposed series theory the great preponderance of red blind cases might be explained as being due to the incomplete development of the terminal end of the colour apparatus and in consequence to the entire or partial absence of the visual substance that responds primarily to red rays, or to the absence of the associated nerves. Although incomplete development of the terminal portion of the cone is a probable explanation of red blindness, it will be evident that the series arrangement of the fundamental colour sensitive elements offers a variety of alternative explanations, especially if the cerebral mechanism is of the same character. If, on the other hand, one fundamental colour sensation were allocated to one particular set of cones it would not be easy to account for the absence of only those cones capable of appreciating say, red.

Alternative explanations on the series system are a possible absence or wrong distribution of the nerves of the various colour sections, or some incapacity of the visual substance to respond to light of certain wave lengths. Suppose, for example, that the development of the cone and the constitution of the visual substances were

both normal, but that the nerves were distributed more over the red and blue ends at the cost of the intermediate green section. In such a case the eye in question would be wholly or partially green blind to an extent dependent upon the defect of the nerve distribution. Lord Rayleigh in 1881 found that many people of normal colour vision so far as the recognition of the fundamental colours is concerned, require when making colour mixtures an abnormal amount of red or green or blue, as the case may be. Such anomalous trichromatic vision might be explained as being due to small abnormalities in the distribution of the nerves over the colour section or of the contained visual substances. For example, if the nerves were concentrated more over the blue end at the expense of the red and green, an extra admixture of these colours might be required when matching white.

There is an equally important series of threshold vision phenomena which must be explained by any satisfactory theory of vision. Abney describes a well known experiment which demonstrates what happens when a coloured light of diminishing intensity is viewed. Under a glass beaker covered with white filter paper he placed an incandescent lamp the current through which could be gradually reduced by means of a resistance. As the intensity was reduced, the yellow white changed to red and finally to a faint sensation of white free from any colour, although when the cover was removed the filament was still red. Placing a red, green, or blue gelatine cover over the lamp resulted in the same final white impression. As the result of other threshold experiments made with spectrum colours, Captain Abney in his work on *Colour Vision* states that, "Here then we have an indication that a person with normal vision passes through a stage of red blindness as the intensity is diminished before he arrives at absolutely monochromatic vision."

According to Abney, if the fundamental spectrum colours are viewed, and the intensity is continuously reduced, "the red becomes enfeebled before the green, then the red disappears while the green shines out brightly, and finally the green acquires a bluish hue." In experiments of this kind, the initial intensities of the various colours must be taken into account. At the fovea centralis, however, it is very difficult, if at all possible, to observe the final change to white light at the threshold of vision. Abney's statement is apparently not applicable to the foveal part of the retina.

Now to explain these threshold phenomena is ordinarily very difficult. Let us see, however, how they accord with the proposed series theory. A ray of white light enters, say, a cone and penetrates to the extreme end. All three colour sections are thus excited and the resultant impression is one of white light. If the intensity is reduced, the ray may penetrate the blue and green sections but fail to reach the red. At this stage the normal colour vision will have become red blind. Continued reduction of the intensity will result not only in red, but also green blindness, and ultimately in total colour blindness. At this stage there will only be an impression of light resulting from the excitation of the inner or form portion of the cone or rod. Further reduction of the light results in darkness.

Since the form apparatus is still excited when the light is too feeble to penetrate the colour apparatus, it could be prophesied that the perception of form would

persist to the end, and further, since the location of a point image by the form apparatus is dependent upon the ratio of the sensations in the three nerves, it is to be expected that the accuracy of coincidence observations would, generally speaking, be independent of the intensity of the light. So far I have not been able to detect any important differences in the precision of coincidence observations when using lights of different colours, and the precision certainly does appear to be independent, within limits, of the illumination. In certain experiments a reduction of the illumination of 1000 fold resulted actually in a small improvement of the precision. But accurate form observations cannot apparently be made at very low intensities in the neighbourhood of the threshold. This is probably due to the quantity of light near the threshold being insufficient to excite the reflex mechanisms that control the accommodation apparatus of the eye, in consequence of which it is not possible to keep the images in focus.

As the intensity of white light is reduced, the quantitative effect upon the terminal red end will also be reduced, with a consequent shift of the mean intensity position towards the green blue end. This shifting of the intensity towards the blue end as the quantity of light is reduced was observed by Purkinje, whose name is now generally associated with the phenomenon.

Reference has already been made to the fact that over the fovea centralis there is no appreciable change from coloured light to white light at the threshold of vision. It will be recollected that in the fovea centralis there are only cones, and that these cones differ appreciably from those distributed over the more peripheral parts of the retina. The foveal cones appear to be more slender and their terminal colour apparatus portions are lengthened at the expense of the inner portions.

That there is a great similarity between the rods and cones is evident and it is reasonable to expect that in many respects their functions may be similar, although they may differ in amount and in some cases be entirely absent. It should not be forgotten, however, that, so far as is known at present, the rods alone appear to be directly associated with the secretion of the retinal purple which was first demonstrated by Boll in 1876. In view of the entire absence of rods in the fovea centralis and in view of the fact that over this area coloured lights do not finally appear white when the threshold of vision is reached, it is a reasonable conclusion that the absorption of the light in the cones may be small compared with the absorption in the rods which predominate in other parts of the retina. Thus in the case of the rods, red light of low intensity might be all absorbed in the form section of the rod and thus finally appear white, whereas in the case of a cone a small difference of intensity would not be recognised in the same way. It will be recollected that at the fovea centralis the overlapping portions of small images did not appear more intense than the remaining portions.

It is not necessary to discuss the question of complementary after images which can readily be explained, but it is desirable to refer to the subject of contrast colours, the explanation of which is usually avoided by ascribing them to psychological impressions. Contrast colour phenomena necessarily imply some interconnection of the various parts of the retina. A similar interconnection is

suggested by the reflex actions that depend upon the total quantity of light falling upon the retina and necessitate some integrating device. Now the rods and cones are not connected to the brain by simple continuous nerve fibres; the connection is a highly complex one. It will be recollected that the nerves from the rods and cones pass to the surface layer of the retina through three clearly defined layers of ganglion cells separated by distinct layers of fine molecular substance. This was illustrated in the second lecture by a diagrammatic section of the retina. Such an arrangement is an ideal one for the fulfilment of the conditions described. Suppose the nerves that control the reflex mechanism are associated with the three layers of ganglion cells. Suppose also that the nerves from the red sections of the rods and cones are connected to the first layer of ganglion cells and those from the green and blue to the second and third layers of ganglion cells respectively. If now the cells of each layer are interconnected by nerves associated with the accommodation mechanism, then the excitation, say by red light, of one or several parts of the retina would result in the stimulation of the corresponding portions of the ganglion cell layer with a consequent indirect excitation of the associated nerves controlling the reflex action. It should be understood that the stimulation of these nerves may be of quite a different character from that of the visual nerves. The total quantity of the red stimulus would thus be integrated and determine the amount of the reflex action. Similarly the green and blue ganglion layers would integrate the effects of these colours or their combinations. But if the ganglion cells of each layer are indirectly connected in this way, it is conceivable that the visual stimulation of one group of cells may have a suppressive effect upon adjacent groups through which the transmission of visual stimulations would be reduced. Thus, suppose one retinal area is stimulated by red light and an adjacent one by white light, it is conceivable that the passage of the red light stimuli through the corresponding ganglion cells would react upon the adjacent interconnected cells of the same layer, through which the red elements of the white light stimuli are passing, and tend to suppress their activity so far as the transmission of the red impulses is concerned. As the activity of the green and blue ganglion cell layers would be unaffected, a partially red blind impression of the white light would result, as is approximately the case in contrast colour phenomena of this kind. The intensity of the red area itself would be slightly reduced by a similar reaction but its colour would still be red, and the red blind impression of the white would not be affected. It will be understood that the reaction referred to is probably of quite a different character from the light impulse transmitted through the ganglion cells by the nerves.

Irradiation can hardly be attributed to defective insulation of the rods and cones in view of the presence of the pigmentum epithelium. If the above assumption is accepted, it seems more probable that irradiation may be due to defective insulation of the ganglion cells so far as the direct light stimuli are concerned.

In view of the fact that the optic nerve contains only about one million individual nerves, it may be objected that to allocate six nerves to each cone of the fovea centralis and possibly three colour nerves to many of the remaining rods and cones is not justifiable. There are about three million cones in the retina and about

eighteen million rods. In the more peripheral parts it is often assumed that the elements may be grouped. But although in the optic nerve there are one million individual insulated nerves, there is little justification for asserting that each is an elementary nerve. The axis cylinders of the individual nerves appear to be bundles of fibres, and although little is known of their details or the extent to which the fibres are insulated from one another, there is good authority for the suggestion that they may be capable of transmitting separate stimuli. The multiple character of the nerves is also suggested by the way in which the fibres of the axis cylinder ramify through the substance of the neurones and recombine to form other nerves.

Much experimental data regarding the detailed performance of the human eye has already been accumulated by many separate investigators, but very much more is necessary. Continuous organised research, not only of the functions of the eye, but also of its histological details, is essential, and I sincerely hope it may be possible in one or other of our leading technical institutions, where such combined effort is alone practicable, to allocate to this important work a sufficient number of research scholars.

Erratum in "The Unaided Eye," Part I. On pp. 224, 225, and 229 the values of x in the equation $\text{Area} = K (\text{Illumination})^x$ should in all cases be *negative*.

DISCUSSION

Professor Cheshire expressed his admiration of the apparatus employed by the author in obtaining the results he had shown. The importance of these in their application to rangefinders was evident, and they went far to explain the advantage which the coincidence type of range-finder had in practice been found to possess. He should like Mr French to make one extension of his observations, and repeat Helmholtz's study of coincidence settings with objects at different distances, using stereoscopic vision. At the time those experiments were made the importance on the result of some of the precautions which later workers had taken was not realised. It would be most valuable if Mr French would obtain corresponding results for stereoscopic settings, and he hoped this would be done and the conclusions described to the Society.

Mr T. Smith (remarks partly communicated) asked the author's reason for adopting an unusual form for the expression of the errors of observation. The introduction of new methods and terms was somewhat confusing and the value to be attached to them was not at once evident. Regarding the experiments referred to in the paper, was the accuracy of setting the same when the object consisted of a white line on a black background as when the line was black and the background white? If not, which was the better and by how much?

He was greatly interested in Mr French's theory of vision and admired the skill with which it had been presented, which lent it such an air of plausibility. As he did not labour under the disadvantage of having studied any theory of vision,

he felt no diffidence in raising objections to the author's theory and suggesting an alternative. There were really two separate theories, the one dealing with the perception of colour, the other with form. The suggestion that the recognition of colour should be associated with the stratified ends of the cones had been previously made before the Society by Mr Chalmers, who however associated the smaller discs with the shorter wave-lengths, the larger ones with longer wave-lengths. The theory presented difficulties to his mind, but he proposed only to criticise the theory for the perception of form. The author's task is essentially to offer some explanation of the fact that, while the resolving power of the eye, the size of the nucleus of the image of a point or line source, and the spacing of the sensitive elements on the retina are all about one minute, the collineation of two lines, a systematic error apart, can be repeated again and again within a fraction of a second. The attempts to reconcile these two figures has led to a suggestion which involves a complex mechanism within elements which are themselves extraordinarily minute, the aim of which is the location of the centre of intensity of luminous radiation within a small fraction of the diameter of the element. Doubtless the theory cannot be rejected on the ground that the presence of such mechanism within so small a space is incredible, but it is quite pertinent to ask whether it is necessary. Incidentally this theory greatly increases what is in any case a difficulty, the disparity between the number of recording elements on the retina and the number of fibres in the optic nerve. Admitting the author's suggestion, the subdivision of the fibres must have a finite limit, and the more a theory strains our ideas of the fineness to which the subdivision may be carried, the less must we regard it as likely to be true.

Objections to the proposed theory, however, need not rest upon such debateable ground. On closer examination it is seen to fail where at first it appears most successful. Take for instance the wavy appearance of a straight line, illustrated in Fig. 13 *f*. The "wave-length" on this theory is essentially of the same order of magnitude as the spacing of the cones, which agrees with the resolution limit. Is it credible that so fine a corrugation would be appreciated by the eye as waved? Even more is it not an absolute contradiction on the author's theory to admit that a straight line appears waved with corrugations about a minute deep and yet claim for the eye ability to detect a want of alignment so much smaller*?

A still greater difficulty arises when the results of the theory are examined quantitatively. The three wire system locates, to a fraction of a second, the position of maximum intensity of the diffraction pattern of the image. The distribution of light intensity in this pattern is known, and the necessary sensitiveness of the system to variations in intensity can be calculated. The speaker has made such calculations—for the sake of ease of calculation they were for a rectangular aperture, but the order of magnitude will be the same for a circular aperture. Taking one minute as the width of the central band of the image, the mechanism must reject as incorrect a point where the intensity is less than the maximum by 1 part in 1090 for an accuracy of setting of one second, and by 1 part in 4360 for an accuracy of

* *Note added later.* Unless it can be shown that the waviness described by Helmholtz has this very small period, it is not explained by the proposed theory.

half a second. The possibility of detecting such small variations is undoubtedly negated by our knowledge of the accuracy attainable in photometry.

By considering the diffraction pattern of a double line it may be shown that the above figures involve ability to resolve lines closer by about 15 per cent. than is usually supposed*.

On seeking for an alternative explanation free from these defects, the suggestion which seems simplest to the speaker is one which does not modify the prevalent view that an individual rod or cone will simply transmit to the brain the information whether visible radiation is falling upon it, its intensity, and possibly its colour, but with no finer localisation than the size of the rod or cone itself implies. The smallest detectable difference of intensity would not be less than that found in photometric measurements. The limits for resolution, which are satisfactorily in accord with theory, are unaffected. The explanation of the very fine setting for alignment should be sought on the supposition that the eye makes small movements in its socket rather than by assuming it to be quite still. Such movements take place without the knowledge of the subject, but conscious effort may easily alter the character of these movements. When attention is directed to ascertaining whether two lines are accurate continuations of one another, these small movements may be restricted so that the image of the line makes no side movements relatively to the retina. As soon as there is any consciousness of change in the illumination of any elements in the neighbourhood of the image, an appropriate corrective movement is automatically applied. If the lines do not join accurately, the change of illumination at that point will be noticed. The high degree of accuracy is readily understood if it is assumed that the sensitive elements on the retina are not necessarily symmetrically distributed, so that the line will just touch some elements and just miss others. This may be facilitated by ability to alter slightly the orientation of the eye, so that the elements are arranged to give the greatest exactitude in such a process. More exactly, since there is no hard line of division between light and dark in the image, the observer will be conscious of a change in the amount of the light falling on the element. The accuracy of setting will thus be effected, not at the centre of intensity, but at the margin of the diffraction band where the intensity changes most rapidly. This would explain why the change from a thin to a thick coincidence line is not greater. As the line broadens from being very fine to a considerable thickness, the diffraction pattern changes from that of a fine line to that of an infinite plane divided by a straight line into dark and light regions. It would be interesting to compare Mr French's curves with calculations on this basis. The experimental results recorded in Figs. 3 and 4 are, qualitatively at least, of the character that this theory would lead one to expect.

Such a theory is compatible with various theories of colour vision. It is unnecessary to suggest one rather than another, but it may be pointed out that small eye movements may themselves be introduced as a not unimportant factor in a colour theory. We may suppose that each element responds to a range of colours corresponding to only one of the fundamental sensations, whether by a filtering

* *Note added later.* On this particular point there is no objection to raise.

of the light before the sensitive element is reached, or by ability to respond only to vibrations about a certain mean frequency, or by other means, and that in a small area of the retina capable of colour differentiation a number of the different elements are indiscriminately mixed up in the same kind of way as starch grains differently stained are packed together on an autochrome plate. The brain may then interpret the results either by integrating over a small area of the retina or preferably by recording the mean impression over a certain interval of time during which elements susceptible to different spectral regions have swept over each portion of the image. Methods of testing such a kinematic theory of vision, either as regards form or colour, in comparison with a static theory should not be difficult to devise, and the results would undoubtedly be of sufficient interest to justify the attempt.

Mr L. C. Martin (communicated remarks): I wish to express my appreciation of the careful and important experimental work of Mr French. I cannot agree with his explanation of the difficulty of form observations near the threshold (given on page 145). My ordinary experience of ill-defined outlines of dark objects seen on a dark night, while stars are seen perfectly focussed, seems to negative such a suggestion. Further it does not seem to me that the phenomenon is at all difficult to explain on the "interconnection theory."

I should like to suggest that the possible discrepancy between the number of retinal cones and optic nerve fibres does not necessitate interconnection of the cones in the fovea where *alone* clear form vision is possible. Interconnection probably does not exist outside the fovea, and the late Mr Chalmers* gave a very ingenious explanation of threshold phenomena on such an assumption—but foveal threshold phenomena are not well enough known to justify any opinion at present.

Instructor-Commander T. Y. Baker (communicated remarks): The Society is very much indebted to Mr French for the extremely able and interesting series of papers on "The Unaided Eye" now just completed. I cannot help thinking, however, that the author has, during the whole series, been desirous of showing how accurate the eye can be in making an adjustment for coincidence, a desire which perhaps is only natural under the circumstances.

The figures that Mr French gives to show what the eye can do in making this setting, appear, at first sight, to be very much below those found by Dr Bryan and myself in 1911. The casual reader of our paper at the last Optical Convention would receive the impression that the unaided eye can set within an error of approximately 10 seconds; Mr French's paper would suggest to him that the figure was about half a second. This large discrepancy arises partly in this way. Mr French's observer makes five settings for coincidence and the "mean difference from the mean" works out at half a second. There is not, except in one case, any indication as to what the actual errors in setting are. As against that, in the Greenwich experiments, the mean error only was retained except in one case. The comparison of 10 seconds against half a second is therefore not a fair one to make. Incidentally, it does not appear quite evident what justification there is for accepting the "pre-

* *Trans. Opt. Soc.*, 20 (1919), 207.

cision," as Mr French obtains it, as a criterion of what the eye can do. If a man counts a thousand sovereigns five times over and makes his counts 988, 992, 991, 989, 985, his "precision" is 2; but has the term any practical meaning? It may be argued that precision as applied by Mr French does connote a definite accuracy because in the use of a range-finder if the observer also adjusts for infinity, he will make the same kind of error there, and, subsequently, his readings will be correct within the "precision" and not within the "adjustment" only. I should be very dubious about accepting any such statement without evidence that in the generality of cases an observer's "precision" and "adjustment" were the same on the artificial infinity setting as during the ordinary work of the range-finder.

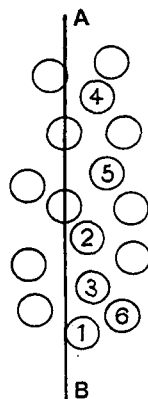
I do not, in fact, consider that it is fair, when speaking of the capabilities of the eye in such circumstances, to deal with anything but the actual errors which are made, and the quantities connected with them. If that is done, I should imagine the errors of observations obtained by Mr French would work out at one half or one third of those obtained by Dr Bryan and myself. That difference I put down to (1) the higher degree of refinement of Mr French's apparatus (our experiments were carried out under somewhat poor conditions of illumination of the target and observers on several occasions complained of eye-strain) and (2) the greater proficiency and skill of Mr French's subjects who, I suspect, were men who had had years of training on this type of setting, whereas some of the men we tested had never even seen a range-finder.

As I stated during the discussion, I think the terms employed in describing the acuity of the eye in this respect are somewhat liable to misconstruction. It is so easy to infer that, when Mr French states that the precision of "A" is 0.49 sec., the error with which that observer repeats his observation never exceeds that figure, whereas stated in the generally accepted terms of the theory of errors of observation 0.49 sec. is his "mean error" and 0.41 sec. the "probable error," i.e., an error of such magnitude that it is an even chance whether it is exceeded or not. An analogous quantity is used in ballistics when dealing with the fall of shot and is termed in that case the 50 per cent. zone, the double breadth being measured. The 50 per cent. zone in A's case would therefore be 0.82 sec., and this represents the space within which 50 per cent. of the errors may be expected to lie. That is very far from representing a space which includes the whole of the errors. If a zone is required which covers 90 per cent. of the errors, we must multiply the "precision" by a factor 4.1. The 99 per cent. zone requires the factor to be 6.5 and the 99.9 per cent. zone the factor 8.3. If a 99 per cent. probability be looked upon as near enough to absolute certainty, then it can be said that "A" repeats his readings within a space of 3.1 secs.

The question of nomenclature in cases such as Mr French has dealt with is really so important and the terms are often so loosely used and so liable to misinterpretation, that I feel I may be pardoned for raising the point and for pleading for something in the nature of a standard notation.

Dr R. S. Clay said that he agreed with Mr Smith in doubting the theory put forward by Mr French for explaining the high accuracy with which the eye can set

two lines in alignment. He thought it much more probably due to the slight irregularity of the positions of the rods and cones. Suppose AB a line of light falling on the retina and suppose the circles to represent rods and cones. If now AB is moved slowly to the right, the light would fall successively on those circles numbered 1, 2, 3, 4, 5, 6, and thus its movement would be observed even if the movement were only about one-sixth of the diameter of a cone. The larger the line, the more accurately this successive stimulation would allow its position to be recorded. This would also explain why practice would enable its position to be more accurately obtained, for by repeated experiments the brain would learn to record and interpret the successive stimulations with greater facility.



Mr French's explanation of colour effects in weak light was ingenious, but he thought that the ordinary physical laws applied to resonators subject to a certain amount of damping would explain the results obtained. If a very weak periodic disturbance acted upon a damped resonator, the response of the resonator would be only slightly affected by comparatively large changes in the period; in other words, if the damping absorbed most of the energy communicated to the resonator, the tuning would be very poor. Thus for instance a very weak red light might stimulate all three colour sensations almost equally. He did not see how Mr French could expect the absorption in a fraction of the length of a cone to do all he attributed to it after it had traversed unharmed all the other numerous layers of the retina.

Mr Guild (communicated remarks) said he was struck by the precautions which had been taken to eliminate the subconscious dishonesty by which an observer's readings might be effected. Such precautions were no doubt essential when the main object of the observations was to determine a property of the retina. From the point of view, however, of comparing the accuracy of different types of setting, measurements should be made under the circumstances which would usually occur in practice in which the observer's dishonesty has its usual scope. The tendency to self deception, and the influence which previous settings have on the next is a very real difficulty with most observers, and certain types of setting are much more liable to be affected by it than others. Consequently it has to be taken into account in considering the suitability of settings, and comparative measures are rendered *less* valuable if elaborate precautions are taken to keep the observer from knowing what he is doing, since such devices are impossible under the conditions in which most instruments are used.

The effect of astigmatism on coincidence settings is a serious matter. It detracts enormously from the advantage which the setting possesses of high precision in repetition. It would be interesting to have a comparison of the effect on other types of setting employed in accurate work.

Mr French's experience that the sensitivity was not affected by employing very feeble illumination is surprising in view of the fact that visual acuity falls off con-

siderably as the illumination is lowered. It appeared from this that the distinctness with which the lines were seen did not affect the precision of the setting.

The series theory of colour vision is most interesting, but when examined in detail some of the deductions from it clearly require modification. For instance the explanation of the Purkinje effect and the ultimate passage to colourless vision at low intensities as due to the rays of longer wave-length being unable to penetrate to the far end of the cones would require an enormous absorption coefficient when the small thickness of the material is taken into account. But the material of the cones is practically colourless and transparent.

Dr J. S. Anderson (communicated remarks): It is interesting to note that many of the author's results confirm those obtained by Dr Hans Schulz* in connection with an investigation into the accuracy of coincidence measurements in range-finders. Mr French, however, seems to obtain a much smaller value for the physiological limiting angle than Schulz's value of 10 seconds, which agrees with the results obtained by Commander Baker and Dr Bryan in 1911. An abstract of Schulz's paper will be found on p. 167 of this number of the *Transactions*. I think Mr French's method of attacking the problem is much to be preferred, as the method of determining the percentage of correct estimations of arbitrary displacements necessitates the making of a large number of observations before the theory of probability can be applied. Schulz's results are, however, of interest, as they represent the mean values obtained by over 50 trained observers.

Major E. C. Henrici (communicated remarks): The results of Mr French's tests on the accuracy of setting coincidence lines are most interesting, and the errors obtained are most unexpectedly small. The results are of great importance from the point of view of the accuracy obtainable with various measuring instruments, and therefore of the design of instruments for accurate measurements. It is to be hoped that Mr French will be able to publish further figures and details of his tests. Mr French gives 9 sets of 5 readings each of setting a coincidence line, but does not give full particulars as to how these sets differ from each other—were they taken by different observers and was the moveable line always brought up to coincidence from the same side? In accurate measurements one would naturally take an equal number of observations bringing the moveable line into coincidence from each side, when such a procedure is possible, as it generally will be, though not in the case of range-finders. This point has been dealt with to some extent later in the paper, and the results seem to differ very considerably. The results given in Fig. 6 show a systematic error of about 20 seconds in the best case, while those of Fig. 7 show only about 2 seconds—is this the difference between two observers, or were the conditions different? The systematic error of 2 seconds is comparable with the accidental error, that of 20 seconds is not.

The method of calculating the "precision error" adopted is one that will make this error appear a minimum. The error given is the average deviation from the mean of five observations, and though this is theoretically 0.7979 of the root mean squared error (or as I prefer to call it for short the "mean error") it is generally

* Dr H. Schulz, *Z. f. Instrk.*, 39 (1919), 91, 124, 242.

less than this when only a small number of observations is considered. The mean errors for sets A, B, and C work out at 0.69, 1.08, and 0.89 seconds respectively, while taking these as one set they work out with a systematic error of 1.01 and an accidental error of 1.12 seconds.

It would be most interesting if Mr French could publish further particulars of his numerical results; the ones given were admittedly taken in ideal conditions which would never be obtained in practice, and very few figures are given to show the effect of other variables, and how far different observers, with what is generally considered good eyesight, vary among themselves.

It is also to be hoped that further observations will be taken with other forms of intersection, e.g. a line bisecting a bulls-eye or fine parallel lines set over a central line. The apparatus Mr French has constructed seems ideal for this purpose, and if he is unable to spare the time for this work perhaps the research could be undertaken at the Imperial College, or elsewhere.

The author communicated the following reply (August, 1920). Owing to my absence on the Continent for several months I very much regret that circumstances have prevented me from replying as fully as I should have liked to the very interesting criticisms of the members and from carrying out, as I intended, the Helmholtz experiments suggested by Professor Cheshire.

In reply to Mr T. Smith, similar observations, although not so many, were made with white lines on a black background. The results were characteristically the same but generally speaking the white line results were about 5 to 10 per cent. less accurate than the black line results. This, however, may possibly be due to the greater interest paid to the black line experiments.

While it is quite true that others, prior indeed to the late Mr Chalmers, have suggested that the stratified ends of the rods or cones may be associated with the appreciation of colour, Mr Smith is hardly justified in regarding the theory as two separate ones dealing with the perception of colour and form respectively. To my mind the essential, and, so far as I know, characteristic feature is the series arrangement.

Mr Smith states that on closer examination my theory is seen to fail where at first it appears most successful, but I must confess a feeling of disappointment that Mr Smith in support of this statement has contented himself with merely asking, is a certain appearance credible? As the wavy appearance is one recorded by Helmholtz, it is quite credible. I, too, have certainly noticed the same kind of effect.

Mr Smith has calculated the theoretical width of the diffraction image and drawn conclusions with which I can hardly agree, but in any case the objection does not appear to be so very serious in view of his statement that "these figures involve ability to resolve lines closer by about 15 per cent. than is usually supposed." Considering how much depends upon the particular observer, there is nothing very remarkable about that.

With regard to the last paragraph of Mr Smith's statement, I presume Mr Smith's

own equipment at the National Physical Laboratory is most suitable for such work, but I think his task will prove a difficult one. It is much easier and indeed safer first to undertake the work of obtaining measurements and then of interpreting them rather than to propound a theory and then attempt to obtain results in support of it.

In reply to Mr Martin, at the threshold of vision a point source appears to me to broaden out and become an ill-defined patch of bluish light. That may be a peculiarity of my eye; on the other hand, in view of Mr Martin's description, it is probable that my threshold illumination was very much less than the ordinary dark but starlight night illumination.

I quite agree with Mr Martin that the allocation of one nerve to each cone of the fovea centralis does not much affect the average interconnection for the vast number of remaining elements, but the fibrous nature of the axis cylinder of the nerves appears to be so definite that I did not hesitate in going farther and allocating a series of fibres to each foveal element at least.

Commander Baker has, I think, overlooked Table II which gives the extreme readings of each precision set, of which full particulars are given for nine sets, A to I, in Table I, not merely one set, as Commander Baker states. These nine sets were really not made with any record-breaking intention. They were actually made by a lady who, although an excellent scientific observer, is not a skilled coincidence range-taker and has no special skill in such work. Every reading taken is included in the series as published. I have little doubt that with a selected operator still better readings might be obtained. In my opinion Table II is more interesting than Table I and should not be overlooked.

In reply to Mr Guild, I quite agree that in the first instance it is desirable to conduct practical experiments under the natural conditions. In coincidence range-finding that, however, has been done many times. I have access to thousands of observations made under all kinds of conditions. Having this information, the next stage is the allocation of these errors. This necessitates the elimination one by one of the sources of error. In my experiments I have eliminated as many as possible of the external sources and endeavoured to show that of the final error a considerable portion is attributable to optical defects such as astigmatism.

There is no suggestion in the lecture that the observer with all his defects of vision can observe to the amount of the precision error. Indeed the combined adjustment plus precision error may be exceeded when the operator is asked to state whether or not two lines are in coincidence instead of being permitted to bring the lines into coincidence. In the latter case the operator may unconsciously make some allowance for his known adjustment and premature adjustment errors.

With regard to the last part of Mr Guild's criticism, may it not be possible that fibrous or other layers between the septa affect the absorption?

Several members prefer that the precision should be calculated on the basis of the theory of probability. The system I have used is the one now generally

adopted in such work. It has the merit of simplicity and under the conditions of such work it is just as accurate. Professor Mellor in his *Higher Mathematics for Students of Chemistry and Physics* states:

As a matter of fact the theory of probability is of little or no importance when the constant or systematic errors are greater than the accidental errors.

The nature of the mean error was particularly described and illustrated in the lecture and I cannot see why there should be room for confusion as to what was meant.

Note added later. I understand from Mr Smith* that he regards the photometric objection as being of principal importance. In this connection I would point out that in most of the coincidence experiments made by me the image lies within the area of the macula lutea and in the majority of cases on the fovea centralis itself. To apply ordinary photometric observations that concern large areas of the retina to minute areas such as the fovea centralis does not seem to be advisable and, so far as I know, there is very little information regarding photometric values for such areas. Such observations as I personally have made seem to indicate that the general results are not applicable, and to attempt to draw any conclusions from such data is hardly possible.

* See footnotes on pp. 148, 149.