the northern hemisphere where the phenomena have been examined with equal care. This fact is not in accord with the opinions of those physicists who regard the solar action as conditioned in its exercise by the direction of the magnetic meridian at the particular station. In the different stations in the northern hemisphere, where the extreme deflections have been found to take place at the same hours of solar time, the differences in the direction of the magnetic meridian have not been less than 70°, equivalent to a difference of solar time of between four and five hours.

I ought not to close this paper without adverting to the success which has attended Mr. Scatchkoff's employment of native Chinese as his assistants in the work of the Pekin Observatory, holding out as it does an encouraging example to Directors of Observatories who may be similarly circumstanced. A very close test of the care and fidelity with which observations have been made and recorded is furnished by the lunar-diurnal variation, deducible from them when they have been re-arranged under the lunar hours to which they severally belong. Thus tested, the Pekin observations show no inferiority to those of other stations which have been similarly examined.

It is understood that the observations, which were discontinued at Pekin at the end of 1855, are about to be recommenced, or have been so already. It is greatly to be desired that hourly obervations of the Horizontal and Vertical Forces should be combined with those of the Declination at this important station. The self-recording apparatus of the three elements which has been in action at Kew during the last two years, has been found, by the reduction of its tabulated values at hourly intervals, to be in no respect practically inferior to the method of eye-observation, whilst it possesses many advantages which are peculiarly its own. The tabulation from the Photographic Curves, as well as the reductions, might be made, if more convenient, at the central Physical Observatory at St. Petersburgh.

March 15, 1860.

Sir BENJAMIN C. BRODIE, Bart., President, in the Chair.

Robert Patterson, Esq., was admitted into the Society.

The following communications were read :—

I. "Analysis of my Sight, with a view to ascertain the focal

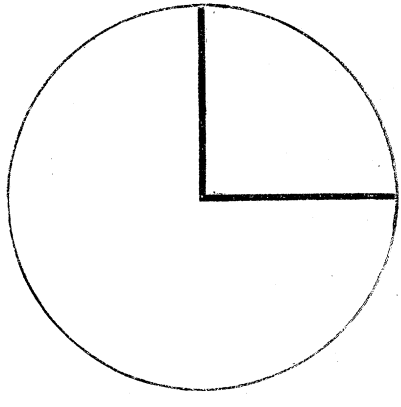
power of my eyes for horizontal and for vertical rays, and to determine whether they possess a power of adjustment for different distances." By T. Wharton Jones, Esq., F.R.S., Professor of Ophthalmic Surgery in University College, London, &c. Received March 8, 1860.

Besides the well-known differences of sight in respect to farness and nearness, there are differences in respect to the power of the eyes of different persons to bring the rays of light to one exact focus.

From observations and experiments in which I have for some time been engaged, I have been led to suspect that *astigmatism* or incapacity of the eye to collect all the rays of light which enter it to one exact focus, is, if not the rule of sight, at least of very common occurrence. I do not here refer to the cases in which astigmatism is of so exaggerated a character as to be a positive defect of sight.

It would be of great importance, both in a scientific and practical point of view, to possess some accurate data as to the frequency of the occurrence of astigmatism; but such can be obtained only by a number of different persons—qualified observers—contributing each an analysis of his own sight. I have thought, therefore, that by bringing under the notice of the Royal Society an analysis of my own sight, some of the Fellows and others accustomed to exact observations might, perhaps, be induced to make similar contributions. The adjustment of the eyes for different distances being intimately connected with the question of *stigmatism* or *astigmatism*, I have included it in my analysis.

If I view a vertical and horizontal line, both equally strong and black, I see them with medium distinctness at the distance of about 10 inches.



At the distance of about $8\frac{1}{2}$ inches, I see the vertical line with greater distinctness and better definition—the greatest distinctness and best definition my eyes are ca-

* Continued from page 374.

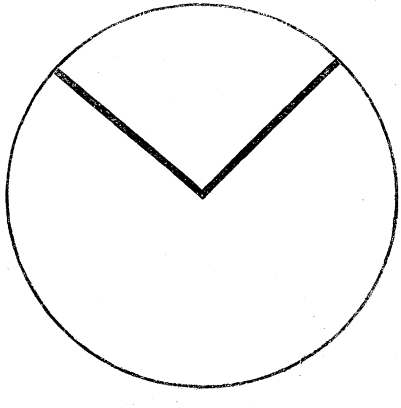
pable of; but the horizontal line I see indistinctly—with much less distinctness than that with which I see any part of the figure at the distance of 10 inches. At the distance of 12 inches, I see the horizontal line with the greatest distinctness and best definition my eyes are capable of; but the vertical line I see indistinctly—with much less distinctness than that with which I see any part of the figure at the distance of 10 inches. It thus appears that my eyes collect to a focus on the retina the rays which diverge horizontally at the distance of $8\frac{1}{2}$ inches; and the rays which diverge vertically at the distance of 12 inches. Whilst seeing the vertical line with perfect distinctness and definition at the distance of $8\frac{1}{2}$ inches, I cannot alter the adjustment of the eye so as to see the horizontal line more distinctly and the vertical one less distinctly; and *vice versâ*, whilst seeing the horizontal line perfectly defined at the distance of 12 inches, I cannot alter the adjustment of the eye so as to see the vertical line more distinctly and the horizontal one less.

In short, I find that I have no power of altering the adjustment of my eyes. I see vertical lines with perfect distinctness and definition only at the distance of $8\frac{1}{2}$ inches, and horizontal lines with perfect distinctness and definition only at the distance of 12 inches, and both vertical and horizontal lines simultaneously with medium distinctness only at the distance of 10 inches.

At the distance of about 7 inches I see the vertical line with medium distinctness, but the horizontal line very indistinctly.

At the distance of about 14 inches I see the horizontal line with medium distinctness, but the vertical line very indistinctly.

At a nearer distance than 7 inches I see both lines indistinctly, but the vertical less so than the horizontal. At a further distance than 14 inches, on the other hand, I see both lines indistinctly, but the horizontal less so than the vertical.



If now I view two oblique lines, both of which are equally strong and black,

I see both legs with medium distinctness at the distance of 10 inches.

At the distance of about $8\frac{1}{2}$ inches I see the two oblique lines equally well, but not so distinctly as at the distance of 10 inches.

At the distance of 12 inches I see the two oblique lines with much about the same distinctness as that with which I see them at the distance of $8\frac{1}{2}$ inches.

It thus appears that I cannot see either of the oblique lines with perfect distinctness and definition at any distance; but that I can see them both simultaneously distinctly enough at any distance from $8\frac{1}{2}$ inches to 12. At a nearer distance than $8\frac{1}{2}$ inches, or a further distance than 12 inches, the distinctness diminishes, and that equally for the two lines.

I cannot by any adjustment of my eyes vary the distinctness with which I see the oblique lines at a given distance.

The preceding analysis of my sight shows that my eyes are *not monostigmatic*, that is, are not capable of collecting all the rays of light which enter them to one exact focus. It shows, on the contrary, that my eyes are *distigmatic*, that is, they have each two distinct foci to which they bring the rays, viz. one focus for horizontal rays, and one for vertical rays.

The preceding analysis also shows that my eyes do not possess any intrinsic power of adjustment whereby they can bring to foci rays diverging from a nearer or further distance than the two distances above specified for horizontal and for vertical rays.

It is true that I can see the different objects in a room distinctly enough without the aid of glasses, and that in the street or open country I can see objects distinctly enough for all practical purposes with the aid of concave glasses Nos. 2 and 3, but, critically speaking, the definition is far from being exact.

Directing my eye to an object 2 or 3 feet from me, I see it distinctly enough whilst an object in the same field of view at the distance of 10 or 12 feet is at the same moment seen very indistinctly. If now, I direct my eye to the object at the distance of 10 or 12 feet, I see it distinctly enough, but the object at the distance of 2 or 3 feet now appears very indistinct.

This is commonly considered an evidence of adjustment of the eye

to the two different distances. There is, however, no real intrinsic adjustment in the case. I see distinctly enough, either the nearer or the more distant object, merely because by directing my eye to it, its image falls on the central and most sensitive part of the retina, whilst the image of the other object falls on the circumferential and least sensitive part of the retina.

It is to be observed that at neither the nearer nor the further distance, do I see the object exactly defined on directing my eye to it. On directing my eye to the further object, I see it, of course, less defined than I do the nearer object when I direct my eye to it; but the difference is not at a glance very striking.

This experiment must not be confounded with another adduced by the late Professor Müller as a proof of the existence of an adjusting power in the eye. The experiment I refer to is as follows:—

If we regard with one eye only (the other being closed) the ends of two pins placed one before the other at different distances in the line of the axis of the eye, one will be seen distinctly when the other appears indistinct, and *vice versâ*. Both images lie in the axis of the eye, one over the other; and yet it depends on a voluntary effort, the exertion of which can be felt in the eye, whether the first or the second pin shall be seen distinctly. "The two images of the pins," says Müller, "fall upon the same point of the retina; one lies over the other, and yet I see the nearer through the cloud-like image formed by the rays from the other more distant pin, and *vice versâ*."

If any person is able to see the phenomena here described, he is undoubtedly endowed with an adjusting power in his eye.

I have never succeeded in seeing the phenomena myself.

In viewing objects at different distances, the sight is no doubt aided by the movements of the eyebrows, eyelids, eyeballs, and pupils; but in this we have no example of adjustment properly so called, viz. intrinsic adjustment.

That the focal power of my eye may become slowly altered, for instance by prolonged examination of near and minute objects, and again slowly return to its former state, I am satisfied; but this, again, is no example of adjustment properly so called.

P.S. It would oblige me very much, if any one, into whose hands this paper may happen to fall, and who may take the trouble to

analyse his sight in the manner herein described, would communicate to me the results of his observation on the following points:—

1. The distance at which the vertical line is seen with the greatest distinctness and best definition.

2. The distance at which the horizontal line is seen with the greatest distinctness and best definition.—Or,

3. If there be no difference in the distance at which the vertical and horizontal lines are seen with the greatest distinctness and best definition.—And, lastly,

4. Whether or not the observer can satisfy himself that he has the power of adjusting the eye, so as to be able to see the lines with perfect distinctness and definition at any other than one distance.

N.B. If spectacles are worn, mention the kind of glasses—whether convex or concave—and their power.

Note also if there be any difference in the sight of the two eyes.

II. "On the Light radiated by heated Bodies." By Balfour Stewart, Esq., A.M. Communicated by J. P. Gassiot, Esq. Received February 7, 1860.

In two papers read before the Royal Society of Edinburgh in the years 1858 and 1859, and published in their Transactions, I have described some experiments on radiant heat, which would seem to involve an extension of Prevost's theory of radiation, known as the theory of exchanges.

As the paper which I have now the honour to submit to this Society will detail analogous experiments on radiant light, I may be permitted briefly to refer to those points in my previous papers which are thus intimately connected with the present subject.

In attempting to unfold the logical consequences of Prevost's theory, certain properties of radiant heat present themselves to our view, many of which are capable of experimental verification.

The following are some of these; and, for convenience-sake, I shall follow up the statement of each (before proceeding to the next) with a description of the analogous property of radiant light, as in this way the similarity which exists between heat and light will be most readily perceived.