

On ophthalmological instrument design, with special reference. To accuracy and rapidity of working

This content has been downloaded from IOPscience. Please scroll down to see the full text.

1914 Trans. Opt. Soc. 15 35

(<http://iopscience.iop.org/1475-4878/15/1/302>)

View [the table of contents for this issue](#), or go to the [journal homepage](#) for more

Download details:

IP Address: 146.201.208.22

This content was downloaded on 02/10/2015 at 04:37

Please note that [terms and conditions apply](#).

ON OPHTHALMOLOGICAL INSTRUMENT
DESIGN, WITH SPECIAL REFERENCE
TO ACCURACY AND RAPIDITY OF
WORKING.

BY WILLIAM ETTLES, M.D., M.S., F.R.C.S.

*Presidential Address delivered 10th December,
1914.*

THE usual plan adopted by presidents of learned societies is to place before their audience a brief resumé of the history, development, and wide-reaching importance of that subject, for the further study of which the individual society has been founded. When delivered by a man of great scientific distinction, the discourse is usually fascinating by reason of the great breadth and depth of knowledge possessed by the exponent. Sometimes, however, as in my case, the president does not possess such qualifications, and then a laboured exposition of historical progress becomes rather a bore to an audience.

In these days, we are inclined to regard tradition as being less binding than in former times, and if a president cannot conform to the older rule with advantage to the society, it is much better that he should make his address on a subject on which he had worked for a long time, and so bring at least the cachet of intimate acquaintance to the consideration of certain special points, which he trusts will interest most of the members.

If, therefore, my remarks to-night savour much more of a simple contribution than of the formal address, I should be glad if you, on your part, would take it in that light; and instead

of relieving me of the exposure to criticism and discussion, which is usually observed on such occasions, I should be very glad if the members present would feel free to raise any points which strike them as needing amplification or criticism.

I am approaching this subject from the point of view of the ophthalmic surgeon engaged in a busy practice, to whom expense of material is a matter of no consideration whatever, if by any outlay he can secure greater efficiency and speed in his work. It is true that a "bad workman quarrels with his tools," but it does not follow that a good workman can work with bad-tools. Indeed, we know that to get the highest quality of work we must give the workman the finest tools obtainable, since none other will satisfy his demands.

There is probably no branch of medical work which requires a more thorough and searching examination than does ophthalmology. The history of the case has to be listened to, and the external and internal appearance of the eye to be inspected, the patient's general constitution, occupations, habits, have to be borne in mind, tension has to be tested, and the refraction to be carefully measured, exploration of the fundus by the ophthalmoscope carried out, the field of vision recorded as well as the muscular balance of the eyes, and all has to be done within thirty minutes. Every minute is of value, and to save ten is to save one-third of the consultation, and is to be able to give mature instead of hurried consideration to the crux of the case which examination has revealed.

I know that it is not enough to provide an oculist with time-saving tools: the minutes may be wasted by the man who has not disciplined his mind, for the consulting-room chair had many interesting occupants. Most people who are engaged in business deal with those in the same line of work or in closely allied branches; but the oculist deals with everybody whose

eyes have become defective, and so cabinet ministers, generals, admirals, solicitors, working men, seamstresses, come one after the other, and the temptation to glean facts as to the movements of the outside world from these many sources, while he himself is chained up in the consulting-room, is a very great temptation. But it is one which must be strenuously resisted in the patient's interest, for the time is all too short for the consideration of the many ocular problems.

We will assume that that lesson has been learnt, and what is wanted now is accuracy, for accuracy is economy of time. To measure and note carefully, and to know the measurement is exact, makes for rapidity. Slack measurement which demands repetition is one of the truest ways of wasting minutes.

I think that at the present time we are too dependent on the workman's ideas. Trial-frames, ophthalmoscopes, lenses, and all the other means at our hand are made by workmen who have never seen a case under examination, and have never handled the instruments in practice. The apparatus is obtained by the professional man who has, at the outset of his career when he purchases his equipment, little experience of what is required. So it comes to this, that the workman introduces a modification which in his view is a good one, and he takes it to his employer, who says, "Yes, that looks very nice; make me a hundred of these, and we will put them in the window"; and this modification is bought by one who does not really know whether it is practically valuable, till long experience has made him feel the faults of the instrument, and perhaps reconciled him to them.

I am approaching this series of observations then, from the point of view of one who has used his apparatus for twenty-five years, and is now about to lay in a fresh stock of tools, knowing very decidedly what he wants, and is com-

ing to you to know whether you can supply them.

Patient's Chair.

It might seem superfluous to say much about a chair on which the patient is to be seated, but in point of fact it does require some consideration.

I do not know if you have ever seen an ophthalmic surgeon doing a retinoscopy on a little child who has been placed under atropine. You would find that he has to develop a marked lateral curvature of the spine in his efforts to bring his head on a level with the patient's; and when he is standing up to do the subjective testing, the strain on his back is indicated by an occasional straighten-up with a deep sigh.

The ideal thing would be to have the patient's head on the same level as the operator's during the sitting examinations. We should therefore so arrange matters that the little patient is lifted to a position where his eyes will occupy the horizontal plane of the adult.

The seat of an ordinary chair is 17 inches from the ground. If it is an armchair, as it should be, the height of the arms from the ground is 36 inches, while the average height of the adult's eyes from the ground level is 50 inches. If a small padded board were laid across the arms of the armchair, and the child placed upon that, it would be found that he had a quite comfortable and secure seat; and the operator is now no longer obliged to undergo the long continued contortion.

The patients' chair ought to be a rotating one. By doing that we can have heavy apparatus, such as the perimeter and the ophthalmometer, on either side of him, well out of the way, and by simply giving the seat a twist round one side or the other, he is placed opposite that apparatus.

Elaborate rising and falling chairs, somewhat on the principle of dental chairs, have

been devised; but in practice they are not so good as the padded-board seat for children.

Lamp.

For focal illumination for ophthalmoscopy, retinoscopy and other manipulations concerned with examination, we require an electric lamp, which can be moved quickly into any position

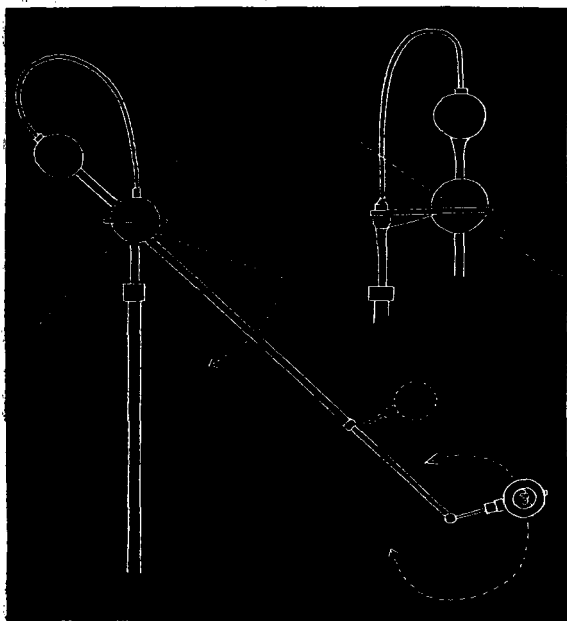


FIG. 2.

within a given perimeter of the patient's head. Numbers of lamps have been devised for this purpose, some of them running on friction rollers up and down a vertical rod. They work all right at first, but after a time one finds that they either stick, or they get loose; in that event they run down and strain the cord, and break the filament of a focus lamp.

Much the best is the balanced ball and socket lamp known as "Lea's Patent" lamp, made by Messrs. Player & Mitchell, Birmingham. This lamp was, I believe, designed for the engineering laboratories of the Mason College, the intention being that it could be easily and quickly moved anywhere over the large surface of a drawing board.

I have used this lamp in my practice now for the last ten years, and I regard it as

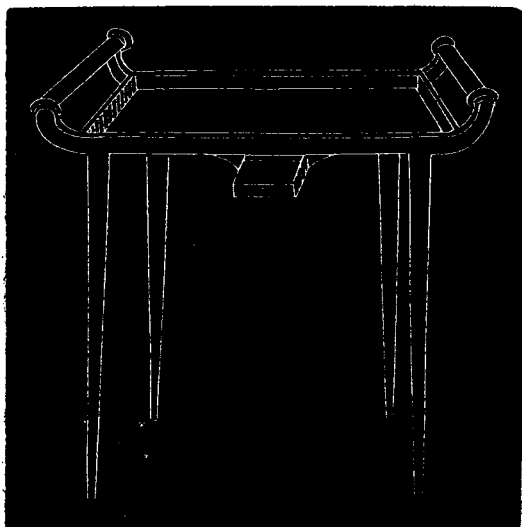


FIG. 3.

superior to any other. The movements are effected with a touch of the fingers, exactly and silently, and the lamp stops instantly in any position in which it is placed. The range of movement, too, is much greater than with any other design.

Table.

The table on which the trial case is placed should be a flat one, and the case itself should be the ordinary tray. All rolled-top desk types

are quite useless for the ophthalmic surgeon's work.

The table should have four slender, rather tall legs, unencumbered with any sort of fitting between. The idea is that it can be placed in front of a patient in the same way as an invalid bed-table. It should also be furnished with handles running along the sides, and, above all, with a little writing pad—such as one uses for telephone messages—screwed to it. It is an advantage to have a small stool on either side of the patient's chair, so that the operator can sit down.

I draw a plan shewing the position occupied by the table when doing each eye, and when doing retinoscopy. By scribbling down the visual acuity and the results of lenses on the pad, one has not to be constantly rising from one's seat to make notes in the case book. When the examination is completed, the paper can be torn off, and the notes then entered.

Test Types.

The ordinary Snellen test types are those generally in use, and no doubt they serve their purpose very well, but those who have worked with them extensively feel that the series is too coarse.

It would take too long to go into the whole matter of test types, but I would briefly draw your attention to a chart which I designed. Practically, on the left hand side, it represents the old one, and on the right hand side, the new series. The chart has to be read downwards in a zig-zag manner, and we get a series of $1/10$, $1/8$, $1/6$, $1/5$, $1/4$, $1/3$, $1/2$, $2/3$, $4/5$, 1. The old series, as you know, is $1/10$, $1/6$, $1/4$, $1/3$, $1/2$, $2/3$, 1.

Thus if we use the old board, a patient may have vision = $6/60$, that is $1/10$, or 0.1. Now, after some treatment, the examination might be repeated, and vision still found to be the same, because the patient could not read $6/36$.

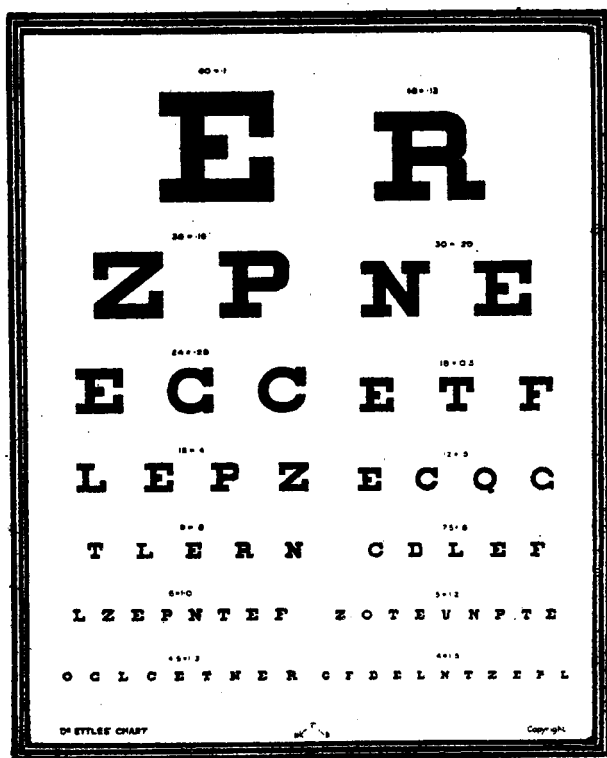


FIG 4.

But in the new test it might be found that vision had risen from $1/10$ to $1/8$, that is $6/60$ to $6/48$, and one would get an idea which way the case was going; whereas on the old chart one would have no idea at all.

You will observe that the test types go beyond $6/6$: they go to $6/5$, $6/4.5$, $6/4$. The reason is that in testing low degrees of refractive error it may be taken as a kind of axiom or golden rule that "low degrees behave to small types as coarse degrees behave to big types." That is one of the reasons why the radiating lines, or any fan tests, have been rejected from ophthalmic clinics. One is dealing with so many unintelligent people, from whom one wants "yes" or "no" answers; while the clearness of the lines on a fan chart are frequently the subject of opinion. If a patient has a low degree of error, $6/4$ cannot possibly be seen. One puts up weak cylinders or spheres, or both, and there is an improvement. You see, the patient either reads these letters, or does not read them, and one can thus get along quickly.

Illumination.

The illumination of the test chart is a very important point. As you all know, much has been done by Mr. Mackinney with the Holographane reflector. It is a mirror shaped like a housemaid's dust-pan, with a tube lamp in the thick end of it. It acts as a cylindrical mirror, and throws a pretty even flood of light over the surface of the chart. But it is not my experience that that evenness is anything like what is claimed for it. It tails off very much just where we want it mostly, namely, in the lower part where the fine letters are; and that, of course, must be the case, because we cannot get past the physical law of the ratio of intensity to distance.

I find a very good plan is to put a piece of mirror underneath the test types. It gives one the effect of a second reflector, and gives the

required added illumination to the lower portion.

Of late a good deal has been done in the way of designing tests which expose only *one line of letters at a time*. The most practical one is that of Mr. Dixey's, which I show you. I have used it, and used it pretty roughly and

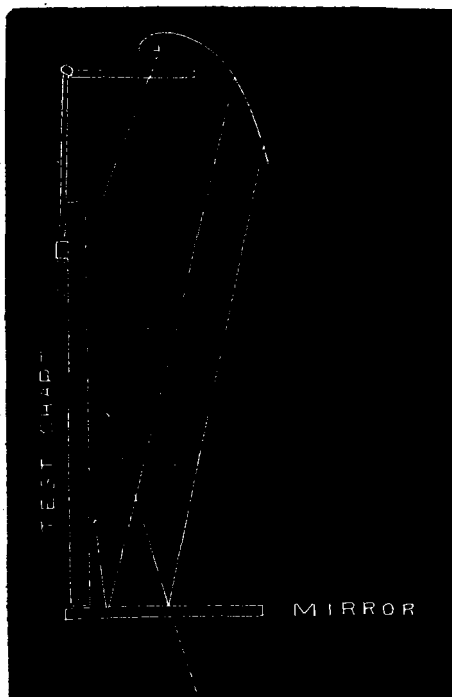


FIG. 5.

continuously for some years now, and it never gives the slightest trouble. The cards drop down in a pleasant way, and we quite get over the difficulty of illumination, as the area to be lighted is so small.

There are subsidiary advantages about the system of exposing one line, because the patient, and the patient's relatives, are not continu-

ally expecting that vision should be raised to a supranormal extent. One may have, for instance, a case of conical cornea in which optical miracles have been done in the way of increasing visual acuity, and you will find that the patient's attention is riveted, not on what has been done, but on what has not yet been done. Now, the drop lines are very useful because the patient, and the patient's relatives, cannot see what there is beyond what has been gained.

Another most important point is that the attention is fixed on what is required. You can imagine the feelings of one pressed for time, who asks a patient to read the letters of 6/9 and who finds that he always begins with the top letter, and then quarrels about whether the fourth or fifth line is really that, because he does not regard the top letter as a line at all!

Trial Case.

I assume that the trial case is familiar to all of you. It does not seem to have altered sensibly since it first came into use a generation or two ago; and that must be to some extent a testimonial to its fitness, otherwise one might assume that important modifications would have been introduced. The trial case is somewhat comparable to a wife. One becomes associated with it for better or for worse before one has had much experience of what it entails; and by the time a man has acquired sufficient clinical skill to be able to criticise its arrangements, he has become more or less wedded to it, and waives these points.

THE SUFFICIENT SERIES.

I think one of the features that would first strike a beginner in looking at the trial case of a man engaged in a busy practice, would be first its dirtiness, and, secondly, the frequency with which the lenses were in their wrong places. Somehow or another one does

not mind that so much as the *lack of a sufficient series*. When the trial case was introduced errors of refraction did not by any means receive the close attention that they do to-day; and although our views have altered, our means have remained stationary. Take, for instance, the concave series at its upper end. We find such a series as half a diopter up to 8, a whole diopter up to 12, and then steps of 2 D. from 12 to 20. Now, that is quite insufficient. Myopia of 13 D., for instance, is an extremely common amount, and yet very few, if any, trial cases have a -13 D. lens. The same with, say, 15 D., it is not a good plan to have to place a -14 D.S. in the back cell, and a -1 D.S. in front of it; for these cases are nearly always associated with some amount of astigmatism, and at least we want to know whether that astigmatism has much significance or not. If we have a three-cell trial frame the loss of light in the internal reflection of six surfaces is a great annoyance, which ought to be eliminated, and can be eliminated, by having a proper series.

In my belief, the day is quite gone past when the trial case of the ordinary pattern can be regarded as meeting modern needs; and everyone who wishes to invest in one and work it seriously should insist on having at least one-diopter intervals from 15 to 22. I say 22 deliberately, because one passes the 20 D. limit quite frequently, and there is no reason at all why our tools should not be beyond the compass of our needs.

It is not to be assumed that the same holds good with regard to the convex series, for this reason, that while high myopia is quite common, high hypermetropia is exceedingly rare. The convex series, in the higher powers, are almost entirely reserved for cataract cases; cases in which the crystalline lens has been removed, and where the supplementary lens to be worn in front of the eye is about 10 D. for distance, and about 14 D. for near work. The 10 to 14 D. series is

in frequent requirement, and above 14 we seldom need the lenses. Again, below 10, from 10 to 5, we seldom need them. Most hypermetropes are under 5 D., unless they are aphakics. On the convex side, therefore, it is not by any means necessary to have the fine series.

The next point I will touch on is that continuous trouble which everyone has in the *difference between the actual and the numerical powers*. We know that with the higher series of lenses, owing to their thickness, refractive index, and the question as to whether they are bi-spherical, or plano-spherical, or meniscus, each trial case seems to be a kind of unit in itself; and every wholesale manufacturer will agree feelingly with me, when I say that most of the lenses returned as erroneous are so because the manufacturer works to one neutralising power, and the prescriber to another.

PLANO-SPHERICAL TRIAL LENSES.

But to go into this question at length would demand much more time than is available for this evening, and I must content myself by just raising the point, and asking whether the trial lenses ought not to be plano-spherical? It seems to me that that would eliminate the difference, and would always make them neutralisable.

A very important point also arises in connection with the *thickness of the lenses*. Surely there is no reason whatever why they should be so very thick and weighty. After all, the width of the cornea is only 10 mm., and the lens itself is practically four times that. This arose in the old days before trial frames had adjustable centres. They were simply rigid frames, like a pair of spectacles. It was necessary, then, to have large diameter lenses, so as to allow for varying sizes of face. But those days have gone past, and it is highly unnecessary to keep that up.

Of course, we all know that the smaller the diameter of the lens, the less is its weight and thickness. Of late, a very nice series has become

available, an instance of which I show you here. (Sample shown.)

The Uni-Bifocal Lens Co. is engaged in making them out of solid glass. They have ample superficies for working, and at the same time the highest powers are no thicker than the low ones.

The makers have provided us with these thin lenses, but not with suitable trial frames. The cells are made of the old breadth to allow for the combination of a thick spherical lens with a thick cylinder. In these spaces the new lenses are placed, and the lenses are just as separated, and just as upset, as to the nodal points, as if they were thick lenses. They are clearly wrong, and we ought to have our trial frames with cells so close together as to get an ideal combination—something which, in the trial frame, approximates to what the patient is going to wear.

There are two minor points to which I will allude: one is the dreadful waste of time caused by *steaming*. Perhaps the patient is late for the appointment, and has been bustling along, and got over-heated. Every lens placed in front of the eyes is instantly clouded with steam, so that in desperation one has to get him to go and cool down. There are two ways by which this can be overcome. We might, it is true, polish everyone of the lenses with one of those cloths which contain "lazin," or preparations of a similar type. That would be substituting one waste of time for another.

The best way, I have found, is to use an electric fan. In warm weather the patient always appreciates it very much, but actually one uses it for the selfish reason that it blows off the moist air. It should be placed so that the air strikes diagonally across the face. In hot weather it is an enormous saving of time: in the cold weather it is not so much appreciated: one might set up an attack of neuralgia; but that can be got over by placing an electric radiating coil in front of the fan so that the air may be heated.

Should these means not be available, another

good method is to warm the trial case and lenses in front of the fire before commencing the day's work, and then place the tray upon a hot-water bottle. Steam, of course, will not condense on a warm surface.

The second point is *dust in the trial case*. If anyone complains that the trial case is dusty, I would ask him to dust it. He will then realise the trouble of getting into the tiny interstices, and the labour involved. A good practical tip is to blow it out with a bicycle pump. Simply squirt the air all over it until the dust and fluff is blown out.

The Trial Frame.

The trial frame is a vastly superior piece of mechanism in its earlier forms, but has, even yet, by no means reached finality in design.

Its intention, of course, is to hold the lenses in their proper positions in front of the eyes, and register the position of the axes of cylinders. Neither of these things does it do as it should. First of all, few, if any, faces are symmetrical, and yet the interpupillary distance is adjusted by means of a two-way screw. The cells ought to be *separately adjustable*, and they ought also to be *vertically adjustable*.

If there is one condition of binocular vision more essential than another in the prescription of suitable glasses, it is hyperphoria, or what is called hyperphoria. That condition in which there is a vertical deviation of the ocular axes. It is nearly always associated with an error of refraction, and therefore can often be corrected by a vertical decentration of the lens.

Can this be done by means of the trial frame? It cannot, and one wants to know why not. I would regard no trial frame as being perfect which did not allow me to make this important adjustment.

POSITION OF LENSES.

Again, I do not know of any trial frame which will allow of the lenses being placed in

the *anterior focal plane of the eye*, in all conditions of face. We often have people with bulging foreheads, and deeply set eyes. There we find that the top of the trial frame strikes the brow and holds the lenses too far away from the eyes. Again, we meet people with high and prominent bridges to their noses. Here again the lenses cannot be set back, and yet we know what an enormous difference it makes whether these lenses are in the anterior focal plane or not. We can often get 20 per cent. more visual acuity by pushing them back into the proper place.

There is no reason why the trial frame should not be so constructed as to perfectly meet the requirements of every conceivable shape of face.

Another point is the scale for *marking the axis* of the cylinder. Only those who have their reputation on the prescription can feel the immense importance of this, and yet if you take the best trial frame obtainable—costing, say, three to four pounds—you will find that the part where careless workmanship is most manifested is in this scale of degrees of axis.

It may be that it is not central with the cell. It is soldered on a little sideways. Horizontal inclinations of the axes are not very much out, but vertical ones may be out 3° or 4° . One places the cylinder in various positions, and one finds that when the cylinder is truly vertical it does not mark 90° on the trial frame. There is only one reason for that, and that is unthinking carelessness.

Then, again, that top portion of the graduated semicircle is nearly always trimmed off by a file. The eminent gentleman who makes these things gives it a final touch up before he puts it into the shop. He does not like that ugly stuck-up edge, but in taking it off to make it look nice he just takes off $2\frac{1}{2}^{\circ}$ by horizontal measurement.

I am sorry to say there is no one single trial

frame on the market, either British or foreign, which is made with anything like the intelligent care which should be devoted to it. It is not a question of price. I am perfectly willing to give £10 or £20 for an absolutely perfect trial frame, and regard it as an excellent investment. The trouble is that these things are made by workmen who do not know what they are doing. They have never seen a patient, and never used the things, and their ideas may be quite rubbishy when tested in the balance of actual experience. The whole trouble is that there is no co-operation between those who know what they want, and those who know how to supply it. I would earnestly advise manufacturers of these articles to take stock of their position, and see if this is not true. The only people who actually do know what is wanted out of a trial frame are those who are using it, and whose reputations depend on the accuracy of the prescription.

Cylindrical Lenses.

These lenses are mounted in two ways, either in *handled rims*, or in rims which are *spun on* and have no handles. Speaking generally, an optician almost invariably uses a trial frame with a rotating cell, in which the cylinder is held by means of a clip. On the other hand, the oculist always uses a drop cell, and prefers to rotate the cylinder with his finger placed against its edge, because by the latter method the speed of manipulation is decidedly greater. The handle is an advantage because there is less likelihood of soiling the marking of the lens in picking it up; on the other hand, it is a nuisance, because it stops the free rotation of the cylinder.

Therefore, as regards mounting the cylinder for rapid work, I strongly recommend the use of the-spun-on rims. Unfortunately, they seem to be unobtainable in this country, although

there is no reason why there should be any difficulty.

Marking of Cylinders.

There are two points of grave criticism affecting cylinders. One is that the *axis* markings are *not invariably right*; the other is that they are *always placed on the wrong side*. The effect of a wrong marking of the axis is, of course, sufficiently obvious; but it is not so generally recognised that the error in a line parallel to the axis is actually greater than in a line which crosses the axis obliquely. I will sketch the cylinder here, and I will draw the axis markings parallel with, and a little to one side of the axis:—

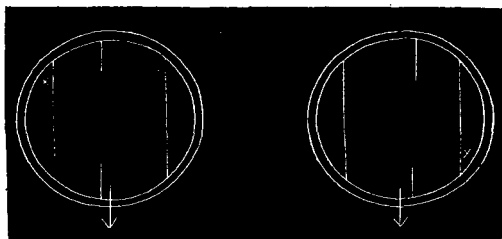


FIG. 6.

Now you will see that if we read the markings at one end it is, say, 5° from 90° , that is 85° . If we turn the cylinder round, what was the upper marking now registers 5° on the other side: in other words, if we read either end of the cylinder, we double the error of such a marking.

I am sorry to say that there is a great deal of carelessness wholly inexcusable, in the way that the markings are usually done. They are often not in alignment, or, if they are, they are often not central.

Next, as regards the position of these markings. They seemed to be always placed on the curved side of the cylinder instead of the flat

side—for I assume that cylinders are practically all plano-convex or plano-concave—and the result is that if the cylinder is placed in the trial frame against the graduated margin, it has to be placed with the curved side forwards, otherwise it would not be capable of being rotated. This introduces a large error of parallax. What with the thickness of the cell, and the thickness of the mount, and the thickness of the cylinder, we may get a very considerable projection, perhaps as much as even 8 to 10 mm. Unless the eye is placed most accurately at right angles to the face and the lens, one will easily get a wrong reading of 4 to 5 or even more degrees. Now, if the markings are placed on the flat surface, that is nearest the scale, the error of parallax is correspondingly eliminated. Then why is it not done?

Retinoscopy.

For the information of those who are not engaged in ophthalmic work, I must point out that one of the best objective methods by which we can measure a defect of refraction is what is usually termed retinoscopy—although, for the matter of that, it has plenty of other names, such as skiascopy, photoscopia, etc.

If an observer reflects the light from a lamp into a patient's pupil, and looks through the sight-hole of the mirror, he will perceive the pupil illuminated with an orange glare. If now he turns the mirror on, say, its vertical axis, the diffused patch of light will appear to pass across the pupil. If the mirror is a plane one, and the patient is short-sighted, the light will pass in the opposite direction to that of the rotation of the mirror. The observer then places increased powers of concave lenses in front of the pupil until this patch of light is made to travel in the same direction as the mirror. A note is made of the reversing lens, and this practically indicates the amount of defect.

The same thing holds good with regard to hypermetropia. In this the patch moves in the same direction as the mirror, and increased powers of convex lenses are placed in front until the reversal point is reached. It is a very simple and exceedingly accurate and dependable method. It is practically our mainstay in dealing with unintelligent people, with children, and so on. Therefore any consideration given to the perfecting of the conditions under which these reflexes can best be watched would be amply repaid.

We will say this process is in operation. The patient is seated in a dark room. The darker we make the room the better will be, by contrast, the colour reflexes, and, so far, therefore,

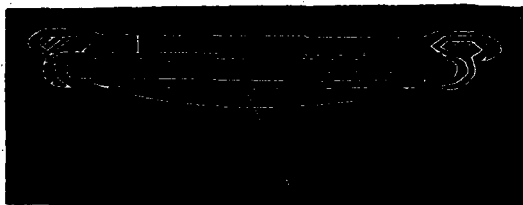


FIG. 7.

the easier will be our retinoscopy. On the other hand, the darker we make the room the less able shall we be to pick out the series of lenses from the trial case, and place them in the trial frame: so that the gain in one direction may be destroyed by the loss in the other.

We can attain working rapidity first of all with the trial frame. No one who does much retinoscopy would dream of using spring clipped cells, where every one has to be slid laterally into a known position. Here the drop cells are absolutely essential, as one can drop the lenses in, and pick them out very quickly. In selecting the lenses from the trial case we have as a rule to be content with the dim illumination of the room.

AUTOMATIC CONTROL OF LIGHT.

At one time I devised a method of having a chair on rockers, which established or broke electrical connections. Thus when bending forward to pick out a lens and place it in the trial frame the light on the patient's face was switched on, and so it could be quickly done; and on bending back to get at a distance of 1 meter or 1.3 meters at which the mirror had to be worked, that light was cut off, and the backward movement lit up the one above and behind the patient's head.

As a matter of fact, I have not relinquished that idea as useless by any means. I think it is a very good one. I only gave it up because I had something better.

A great majority of people whom I have watched at work in retinoscopy waste a good deal of time by placing their trial cases in an awkward position. It is usually on their right hand side. To pick a lens out for a patient's right eye, therefore, the operator has to pick it up with the right hand, transfer it to his left, and place it in the trial frame. The proper position for the box of lenses is almost beneath the patient's chin. The trial case table should have four rather long slender legs, without any encumbrance below the plane of the table, so that, like an invalid's bed-table, it can be placed rapidly in front of the patient's chest. In this way there is the least possible amount of movement between the rack of the trial case and the trial frame. That combined system would, I think, be most useful to many workers. I call it the "invalid table and rocking-chair" system. It certainly gives us the utmost speed of movement, and the best conditions of illumination.

A CHAIN OF LENSES.

I now come to a *mechanical device for putting up these lenses*, which I have been working at off and on for some considerable time, as I feel

that this method—if successful—has a decided future. I dare say most of you know that there have been in the past many elaborate pieces of mechanism devised for the purpose of changing glasses in front of the pupil. So far as I am aware they are all dependent on the same principle, namely, the rotation of the wheel on the periphery of which the lenses are placed. Of course, that is a very good method mechanically, but it does not work in principle, because it is difficult, on account of the movement so close to the eye, to keep the patient's gaze fixed in the proper direction. These machines always cause a good deal of nervousness, and, what is much worse, they very seldom can be placed exactly in the anterior focal plane of the eye. They are well made, and handsome pieces of apparatus, but I do not know of anyone who has ever had one who continues to use it with any enthusiasm. On the other hand, there is no doubt that one of the most prolific sources of wasted time in consultation is the changing backwards and forwards of the lenses into the trial frame, during prolonged retinoscopy—say, in the case of mixed astigmatism, with not very satisfactory reversal points.

That may arise through there being much spherical aberration of the cornea and lens, or it may be defective transparency of the media. On the other hand, there are very many cases where from one cause or another it is impossible for anyone to be certain of the exact reversal point, and one has to do it several times to make sure that it is within definite limits.

Now, all that means, as I have said, much loss of time; and every worker feels that if he could only have some method by which these lenses could be rapidly changed, it would greatly facilitate his task.

That feeling reaches its climax in modern school clinics, which have sprung into being as the result of the compulsory examination of school children. The ophthalmic work in these,

so far as the errors of refraction are concerned, is almost without exception a continuous series of retinoscopies under atropine.

The machine which I have devised depends on this principle. A canvas band passes over a

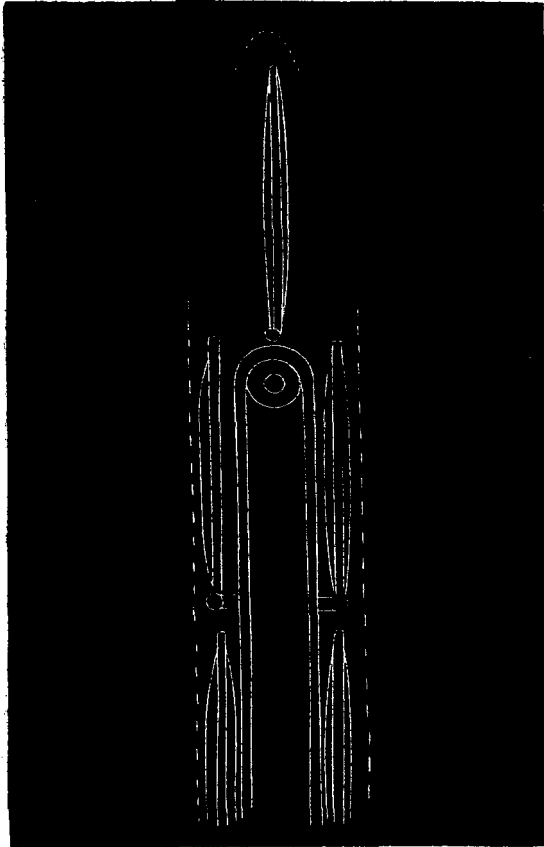


FIG. 8.

tiny roller, and is guided in its passage by a series of sprocket wheels, very much in the same way as the film of a cinematograph projector travels across its lens. Hinged to this band is

a series of full-sized trial case lenses. As the band or tape passes over the little roller these lenses rise, until, at the summit, the lens is sticking up opposite the patient's pupil. As the tape passes on, the lens is dragged down, and its place is taken by the next in series. This apparatus therefore absolutely does away with the wheel principle. The lenses are lifted by mechanical fingers, and one after another held opposite the pupil, and taken away again; and the speed with which it can be operated is far greater than can be used. I mean that the lenses can be changed in position at the rate of about 500 changes a minute; and therefore the speed of retinoscopy is entirely dependent on the operator's skill in judging the movement of the light.

A little time is wasted in setting the patient's face to this apparatus, but, after all, not longer than is needed for the proper centration of a trial frame, and the adjustment of the trial case. In practice it works out that the time spent in retinoscopies is at least half what it is under ordinary conditions; and I am quite sure that in working through a school clinic I could undertake to do three children under atropine for every one done by the ordinary method.

Retinoscopy Mirror.

Just a word with reference to the mirror used. Everyone nowadays uses a plane mirror, and if you imagine the out-patient department is being got through, or that one is engaged in examining a large number of school children, you can see that we want at once to find out whether any defect in visual acuity might be accounted for by disease visible with the ophthalmoscope. What one does ordinarily is to place the trial case aside, get out a magazine ophthalmoscope, and pursue the stereotyped method of ophthalmoscopy.

As a matter of fact, what we want in ophthalmoscopy is to sift out definitely pathological

from non-pathological cases. Direct ophthalmoscopy is rarely needed in the course of a day's work, because one examines the fundus by the indirect method, and if there is nothing remarkable, one passes on. It is only when a detail arrests the attention, that one goes on with the direct method of examining it under greater magnification.

Therefore, while retinoscopy is going on, one has the patient in the dark, with the light suitably situated, and all one has to do is to take a + 1.5 or a + 2 D.S. out of the trial frame, place it in the little clip in front of the mirror, and then one has a very good ophthalmoscope for the indirect method. A convex lens in front of a plane mirror acts as a concave mirror; and the convex lens being placed across the sight hole helps the observer to accommodate for the aerial image. It is one of the trivialities of clinical work, but it has this to be said for it, that it reduces ophthalmoscopy from an affair of minutes to an affair of seconds.

Perimeter.

The perimeter is an invaluable instrument for diagnostic purposes. Thus, in the early stages of glaucoma, when it is difficult to be sure of its presence, there is a characteristic constriction of the visual field, in which the scotoma or blind spot runs up to and includes the optic disc. Again, in optic atrophy there is a narrowing of the field, in retinitis pigmentosa, and very many other conditions. So also the field may be good at its margins, and yet be defective in its centre, constituting so-called scotoma or blind spot. I think that of all the possible affections of the visual field this is the commonest, because it is a condition caused by excessive smoking—a malady which is greatly on the increase.

The ordinary form of the perimeter you are well acquainted with. It is a quadrant graduated on its margin in terms of angular deviation

from the centre of the sphere, and which can be rotated about the line of fixation of the eye, so that it becomes a solid figure—that is to say, a hemisphere.

Now we will assume that we have this very common condition of *central scotoma*, and we are provided by the maker with a perimeter for its measurement. The patient has one eye blindfolded, the other is placed in the axis of

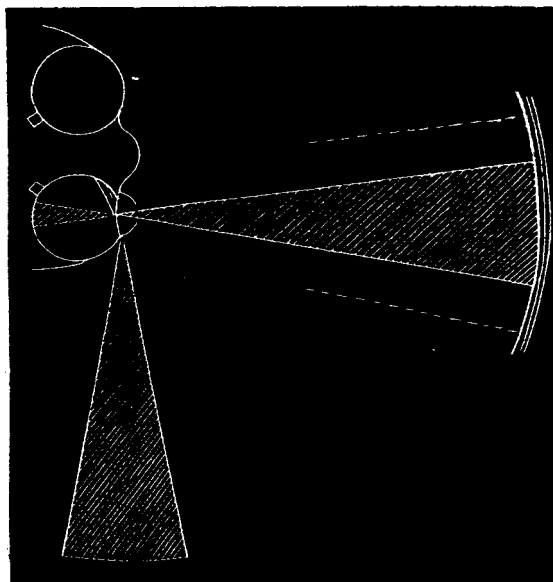


FIG. 9.

the instrument. He is told to keep his eye on the fixation spot, and then the peripheral object is moved outwards until it ceases to be seen. Now, that is very pretty, and seems easy enough; but I can only assume that no instrument maker has ever asked himself, "How is a man with a central scotoma to fix the fixation object on the axis of the instrument?" That is just where his blind area is. So in practice what happens is this. Projecting outwards into

space from the patient's eye is a cone of blindness, and occupying that cone is the fixation object you are asking him to see. He has obviously to rotate his eye in some direction so that the sentient margin of the cone can become aware of the presence of the object. Now it all depends which side of the cone he uses. If it is the side that you are measuring, you will find vision right up to the fixation spot, and if it is on the opposite of the field you are measuring, you will find the scotoma twice the diameter it really is. Now, there is a beautiful state of affairs! The plain fact is that the perimeter is absolutely and totally useless in its present construction for such an estimation.

A lot of time and thought has been given to furnishing it with a sliding test object, and *gearing down a needle* which will prick the test chart; but, unfortunately, owing to ineffective workmanship and design, these geared down mechanisms are worse than useless because, owing to backlash and looseness of working parts, and insufficient adjustments, they nearly always register with a great margin of error.

So much so that, personally, I would not trust, under any conditions, a mechanically pricked out chart of a field of vision.

Now, there is another point, if you look at a printed chart of a field you will see that the visual area extends beyond 90° : that while looking ahead we can see sideways to a considerable extent behind us—in short, on the temporal side the margin is 110° or even 115° , in some eyes 120° . This is a most sensitive portion, it is the first which shows constriction of the visual field, and yet it *cannot be measured* simply because the instruments are so badly made.

When a workman starts to make a perimeter he takes an arm, which is an arc of 90° , and then he mounts upon it this sliding mechanism which requires support; and then, with one thing and another the actual movement of that test object covers an aperture of only about 75° .

You can look at any perimeter you like, which is mechanically propelled, and you will find my words are true. The actual effective aperture of that instrument varies from central, and very badly central, up to about 80° , certainly no more. Now, why in the name of all that is sensible, should that arm not be brought round so as to make the whole field measurable, and that in its most valuable portion? There is no reason at all, except ignorance.

Fusion Testing and Training.

A field of ophthalmic work which is as yet almost untrodden is that of the study of binocular vision. The time is gone past when it was sufficient for an ophthalmic surgeon to cover one eye, and test the exposed one; then cover it up, and test the other. It was generally held that when that was completed, one could sit down with confidence and write the prescription. But nowadays things are getting different. We recognise that an enormous amount of eye strain is due to anomalies of what is called the muscular balance.

Before I touch upon instruments, may I just point out that, although it is perhaps convenient to speak of muscles being at fault, we have to remember that the muscle is only a servant of a nerve centre, and that actually the muscles themselves are perfect.

MUSCULAR CO-ORDINATION.

As I have said elsewhere, if you teach a person to swim or ride a bicycle, you have not strengthened his muscles—it has probably not made the slightest difference—but you have co-ordinated them. What we aim at is the establishment of a normal co-ordinated reflex. That is what skill and practice means.

Let me take, as an analogy, a child learning the violin. Its attention is fixed on fingering the strings by the left hand, and also it has to think of the right way of holding the bow: beyond

that to consider the note it is playing, its meaning, duration, quality of tone, trueness of note, and so forth. You can imagine that a tired and snappy teacher would find fault with it for holding its bow wrongly, while the pupil is honestly considering whether the left hand is properly placed to take the next passage—in other words, on the movements; and mental efforts are directed from the reasoning portions of the brain entirely. But as that child acquires skill, the handling of the bow, and the manipulations of the left hand become more and more unconscious, they are no longer directed to the higher centres of the brain, but are dealt with by the lower centres; and the higher centres of the brain are thus released for fresh mental efforts, leaving the fundamental ones to be automatic. Hence in the highest expression of a trained player the only point on which his mind need be engaged is the interpretation of the spirit of the composer.

A second point has to be borne in mind in connection with deviations, and that is that in all cases we aim at the re-introduction of an instinctive reflex which has been handed down for countless generations. There is therefore an enormous sub-conscious tendency to be able to do it. Now, that is a very different thing from endeavouring to establish an abnormal state of muscular balance. At present we are passing through a stage when muscular deviations and muscular trainings are comprehended but by a few. It might be fair to say that systematic treatment is scarcely done at all. It behoves everyone, therefore, who is interested in the manufacture of instruments, to try and comprehend what is wanted and what line of development is indicated.

Now, first of all, as regards testing. The little bundle of rods which Mr. Berry recommended to Dr. Maddox, has been found to be of the greatest value in telling us whether muscular imbalance is there or not. But if this imbalance is asso-

ciated, as it nearly always is, with an error of refraction, so much the more is it imperatively necessary that the trial frame should be capable of the most perfect centration, as otherwise it is incidentally obvious that we can manufacture no end of imbalance by careless setting.

I shew you here what I believe is the *only objective method of demonstrating imbalance*.

Demonstration.

You know that in the ordinary way the looker-on can see nothing of the red streak. Here I have two mono-chromatic lamps of feeble luminosity, and therefore easily suppressible, yet luminous enough to be readily visible: these lamps move on a tramway under the control of the operator at the patient's end of the room. Muscular imbalance assumes a new depth and gravity of meaning when one sees a person setting them two feet apart, and saying they are directly over one another.

As arising out of that, we come to the question of treatment or training. In that, of course, we are dependent on the stereoscope, either in its original form of Wheatstone—as exemplified in the so-called amblyoscope—or in one of its other modifications, principally Brewster's.

I show you here an amblyoscope, which, although a crude instrument, has done good service in drawing the attention of the ophthalmological world, as has been done by Mr. Worth, to the splendid pioneer work done by Javal. It is to Javal that we owe practically the whole of the foundation of the knowledge of treatment.

The mechanical disabilities of the instrument are that there is no mechanism for altering the *interpupillary distance*, and as the lenses placed in the eye are of great power that is a most important point.

Another point I show you here: Supposing the tubes are set for correct interpupillary distance, since they are turned inwards to deal with squint cases, it is *impossible* for the child to get the conditions of binocular vision. I have long

felt that we have not been fully conscious of the significance of fine movements. In other words, we speak of a 4° or a 5° prism as a strong one, and yet we make a movement of these tubes which would represent the angular displacement of a vastly more powerful prism. A little movement in an amblyoscope corresponds to a very great deviation in a prism.

Another point, I felt, was that the pictures used in these instruments were *far too coarse and primitive* to appeal to our stereoscopic sense. We want as close an approximation to the actual thing as possible, and that, of course, can be done by stereoscopically photographed lantern slides mounted in the ends of the instrument.

I show you here a new model of my training apparatus:—

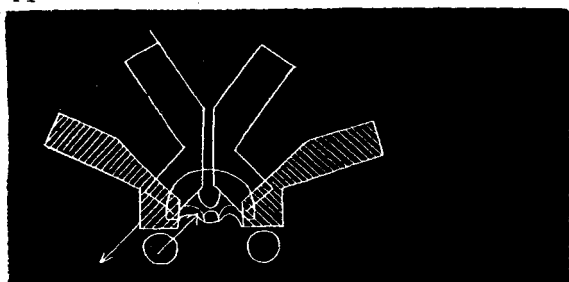


FIG. 10.

Showing how tubes adjusted for parallel eyes are centrally fixed, but when the joint is closed, the axes no longer coincide.

You see that the patient's face is firmly fixed: there is an *adjustment for interpupillary breadth*; the tubes are so hinged below the patient's chin that *their vertical axes correspond to the centres of rotation of the eyes*. In all excursions they are concentric with the movements of the visual axes. We get as perfect a representation as it is possible to get, and the movements of the instrument are so exceed-

ingly fine and registered that one can use the movement of the tube in interchangeable terms of the axis of a prism. We can deal, as you see, with defects of convergence and divergence, of supravergence, and of cyclovergence. The main point that I rely on is this: we establish in any condition of balance a true stereoscopic conception, and having attained that we find that the mind resists the splitting up of this picture so strongly, that we can establish opposite tendencies to an enormously greater extent than could be done by other means, before the binocular sense gives way and the picture is broken up.

Ophthalmometer.

In view of the fact that my friend, Mr. Sutcliffe, one of our past Presidents, will address the Society later on concerning corneal measurements, I do not think that it is necessary for me to touch upon the ophthalmometer

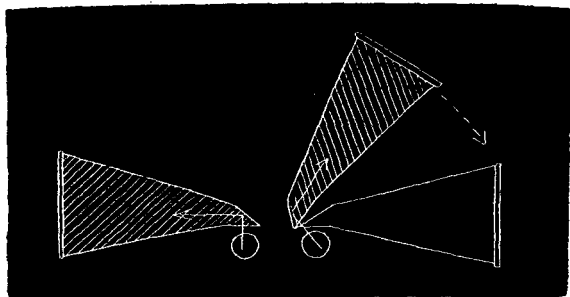


FIG. 11.

In the Synoptophore, the centre of rotation of the tubes is under that of the eyes. The simultaneous picture can therefore be used to draw the axes into parallelism.

from the point of view of design. He has done so much, and his own designs are marked with so much originality that, although I have a few ideas as to what might be done to improve any instrument, from the clinical standpoint, I will reserve those suggestions for his evening.

Discussion on Dr. Ettles' Presidential Address.

MR. CHALMERS, in proposing a vote of thanks to the President for his very able address, said one of the chief objects of such a Society as the Optical Society was to bring makers and users of optical instruments into closer touch with each other. He could not help saying, however, that Dr. Ettles had himself to blame in some ways for the shortcomings of which he had complained. Many years ago he might have said some of the things he had now said to the Optical Society, and those things might have borne fruit; but he had neglected to do so. That was true of a great many other people; they grumbled at the instruments they were using, but they never thought of telling the men who were making them that they were not trying to produce the things which the users wanted; the users were content to keep on waiting until they could themselves design something better. Now, most instrument makers were quite capable of improving the actual design if only they were told that what they were trying to do was not what was wanted, and that was one of the things which ophthalmic surgeons had hitherto rather failed to do; they had not criticised existing instruments in the way Dr. Ettles had so effectively done that evening. Dr. Ettles had referred to the method of illuminating the test chart for which he (the speaker) was jointly responsible with Mr. Mackinney, and had said that there was a less perfect illumination at the foot of the chart, where, because the letters were smaller, a better light was required. Now, it was just as easy to place the mirror below the chart as above, but the right solution undoubtedly was to have the mirror, as originally designed, to extend the full length of the chart. Here the question of price came in, and it was not thought desirable to obtain any more perfect evenness of illumination by using two lengths of mirror. But with the longer mirror the illumination was almost as perfect as one

would desire to have it. There was one suggestion he would like to make to Dr. Ettles, and that was that he was trying to get too much out of one trial frame. He thought that was the solution of the difficulty—that one trial frame could not be made to do all that was required in all cases. If Dr. Ettles was prepared to spend, as he said, ten or fifteen guineas on a trial frame, an expenditure of less than that on a selection of trial frames might quite well meet most of his requirements. One or two of the cases mentioned by Dr. Ettles were obviously cases for specially-designed trial frames. The stereoscope slides used by Dr. Ettles in connection with the amblyoscope were very valuable, and had been used by nearly all those who had seriously employed that instrument. In certain special cases particular types of objects had been found extremely useful; for instance, balloons had been found particularly good objects, and he believed the general experience was that ordinary photographs were not nearly so good as black and white diagrammatic stereoscopic photographs.

MR. W. A. DIXEY, in seconding the vote of thanks, said that it gave him considerable gratification to hear the President's invitation to his audience to comment upon the different subjects dealt with in his address. To most of those in the room the subjects treated of were very familiar, and one of the advantages of such familiarity was that one could afford to indulge more freely in criticism. With regard to test types, he did not know whether it had occurred to Dr. Ettles that there was a very simple and practicable method (it had been adopted, he knew, by many people) of grading the test types more finely even though only the ordinary types were employed—namely, by regulating the distances at which they were seen. There were two factors which went to the estimation of acuity of sight: one was the size of the letters, the other the distance at which the letters were seen. Obvi-

ously the gradations might be increased by multiplying the letters, but that could also be done equally well by dividing the distances. Dr. Ettles had commented on the shape of certain letters for which he (Mr. Dixey) was, he was afraid, responsible. He admitted some of them were a little eccentric in form, but he could assure them those letters had been devised with a full appreciation of the rule Dr. Snelling had originally laid down. Not only did each letter subtend a definite angle, but each limb of each letter. And what was more important still, the space between each limb of each letter subtended a definite angle. It was too long a subject to go into fully, but the letters had been, he was going to say intelligently designed—(laughter)—but at any rate they had been deliberately designed. He would give an illustration which they would all appreciate. Taking the letter O or the letter C, it was easy to draw that letter of such a size as that the whole letter subtended a given angle, but an O could not be drawn (unless it was a very thin and skinny O) in such a way as that not only the limbs but the interstices between the limbs subtended the angle laid down by Snelling. And it was to fulfil that condition that he had designed some of the letters which Dr. Ettles had criticised. With regard to the test case, the paucity of the number of lenses in the test case was obviously a matter for the oculist who gave the order. All through his business career he had made test cases, and quite recently he had made a test case with single diopters right up to 20 D.; indeed, he was not sure that he had not gone beyond 20 D. So that he did not think that in this respect there was any real subject for complaint, or if there were, the remedy lay in the hands of the doctors themselves. Many manufacturers would be delighted to take Dr. Ettles' order for a trial case with any number of lenses he liked to specify. (Laughter.) With regard to thin lenses, it had been quite a common prac-

tice for years and years to supply the deep lenses in the thin form, and to champfer the concave lenses; and with regard to the convex lenses, quite a practical way was to mount the thin lens in a transparent, light-coloured celluloid visual. In his opinion it was not a good method to cement the lens on a plain glass. With regard to the question of dust getting into the trial case, he could not believe that Dr. Ettles had not seen the trial cases in which the whole thing was made to lift bodily out. Such a trial case was quite easy to dust, though, of course, in order to dust the lenses themselves each individual one would have to be taken out.

THE PRESIDENT: I had one, but could not keep it clean; the dust was left all along the edges.

THE BICYCLE PUMP.

MR. W. A. DIXEY: Then in such a case, of course, the bicycle pump would be a good method. (Laughter.) Proceeding, Mr. Dixey agreed with Mr. Chalmers' remark, that in order to fill all the requirements it was possible to wish for in a trial frame, more than one frame was desirable. One great point in an ordinary working trial frame was simplicity. For that reason, if a man were going to rely on a single trial frame, he would not recommend him a very elaborate one. Such a one, for instance, was Dr. Bardsley's frame, which had a special apparatus for adjusting each eye separately, horizontally, of course. The vertical adjustment was another matter. He confessed he had never seen a trial frame in which it was possible to get vertical adjustment, and for various reasons he did not attach, perhaps, quite so much importance to that point as Dr. Ettles appeared to do. He would give them a hint as to why that was when he came to speak about heterophoria. With regard to the cells, they must have noticed his (Mr. Dixey's) reluctance to father the diagram drawn by Dr. Ettles.

At the same time, though somewhat exaggerated, it was more or less what one saw in certain trial frames. Some time ago he had got over the difficulty by making four-fold grooves, in which the thinner lenses could be put in close juxta-position. In the case of the thicker lenses they simply omitted one of the grooves, putting one lens in No. 1 groove, and the next in No. 3, or even in No. 4. Now it was just as easy to make a frame like that, as long as the work was good. Of course the necessary conditions were that the grooves should be very accurately shaped, and also that the trial lenses should be truly circular. Dr. Ettles had said they were dependent upon Germany for spun trial lenses. Now that was absolutely incorrect. (Hear, hear.) Oddly enough, only yesterday afternoon he had gone up to his instrument workroom and found one of his workmen making spun lenses; the lathe was turning and the lenses were dropping out, any number of them—(laughter)—and if anyone liked to order any number of gross of spun lenses from a certain workshop in Bond Street he was quite sure they could be supplied at once. And not only there, but at lots of other places, there was no difficulty about it. He was very glad Dr. Ettles had mentioned the question of centering cylinders. This was a very important point. In centering a cylinder in a test lens, it should be recognised that the essential thing was that the axis mark should be in the real geometrical axis of the rim, and not, as more often it was, exactly on the apex of the cylindrical curve. It ought to be both, but perfection in this matter was impossible; there was sure to be an error of $1/100$ th mm. or so. His point was that the error should be in the centering of the lens, and not in the fixing of the glass into the rim, because if there were an error in the adjustment of the axis mark to the central diameter of the rim, there would be a considerable error in reading off the axis on the scale; whereas if

the axis mark was in the geometrical axis of the rim, the error would be only a very trifling one of decentration. He feared he was trespassing unduly on the patience of the meeting, but he did want to say one word on the subject of heterophoria and stereoscopic vision. He mistrusted all these tests for heterophoria, on grounds to which Dr. Ettles had himself called attention more clearly than anyone else he knew.

He would not attempt to go deeply into the matter, but would content himself with calling attention to what Dr. Ettles had already published on the subject. With regard to Dr. Ettles' test for heterophoria, which he had shown them that night, he, the speaker, mistrusted the notion of putting a person into a dark room with a couple of weird-looking lights, at which that person was to look at an undefined distance, and then testing that person's heterophoria. The person, unless he was very intelligent, did not know what the distance was, his accommodation would wander about, his convergence would wander about, and no sort of clear result would be obtained. It seemed to him the proper way to test heterophoria was by testing the range of convergence under a given degree of accommodation; that, in his opinion, was the only right way to do it, and the best means of doing that was by a properly arranged stereoscope. The stereoscopic arrangement shown by Dr. Ettles was excellent, but he thought there were others which were better because simpler. He could not mention them without giving names, and he did not want to do that, but he was speaking of a stereoscope where the accommodation was fixable, and where the amount of convergence could be varied by the use of rotating prisms. That was the best device for measuring the range of convergence, and the best method of treating cases of squint and so on. In doing that it was quite right, as Dr. Ettles had pointed out, to use the sense of fusion, and if a stronger incentive than the

sense of fusion was needed, they had that in the stereoscope. The subject was a rather involved one, and it would be a mistake to try and deal with it sketchily. It was a subject concerning which there was a good deal to say that, as far as he knew, never had been said, but he would therefore, on the present occasion, bring his remarks to a conclusion without entering any further into so enticing a subject.

Dr. R. S. CLAY cordially supported the vote of thanks. Dr. Ettles had modestly said it was "not much of a Presidential Address," but it seemed to him one of the most practical Presidential Addresses to the Optical Society that he had heard. (Hear, hear.) He was sure that to anyone connected with the manufacture of ophthalmic instruments the suggestions thrown out by Dr. Ettles would prove most valuable. There were two points he would like to refer to. One was, whether there was any need to use such large lenses for retinoscopy and in sight-testing generally. It was usually said that if small lenses were used, people imagined that they were looking through a tube, and that this caused them to accommodate, and brought about wrong results. He would like to ask Dr. Ettles whether the objection had any real physiological basis. If the lenses could be made smaller one could obviously reduce the size of the trial cases: the frame also could be made smaller and lighter. One could also more easily, in the case of a deep-seated eye, get the lenses into the nodal plane of the eye. No doubt, if the lenses had not handles, they would be awkward to use. The other point to which he would like to refer was the design of the instruments for heterophoria. Mr. Dixey had suggested that other types of instruments (with rotating prisms or mirrors) might be as good as the one Dr. Ettles exhibited. Now, it seemed to him that the same fallacy underlies the use of these instruments as that which Dr. Ettles had referred to in another connection, namely, that whilst they would be

all right for a person without diplopia, a person who squinted would look through the rotating prisms or mirrors obliquely. Nor could he see any way of avoiding this, except by the adoption of the principle of rotation round a vertical axis through the centre of rotation of the eye. Dr. Ettles' instrument was the first he had seen based on this principle, and therefore it was a real advance upon anything he, at any rate, had heard of in that connection.

Reply.

THE PRESIDENT, in acknowledging the vote of thanks, said, as regarded trial frames, it was true, as had been pointed out by both Mr. Chalmers and Mr. Dixey, that two trial frames were needed: one with the coarse adjustment and drop cells, which enabled them to do their retinoscopy and "roughing out," and one with a fine adjustment, by means of which they got perfect reading of the axis, and so on. His trouble was that he could not get the fine one fine enough to suit all classes of faces. In dealing with a case of hyperphoria it ought to be possible by decentration lenses to establish orthophoria. As things were, he had to use Silvanus Thompson's tables. If it was an ordinary case, without any astigmatism, he simply tilted the trial frame sideways, one spherical lens went up and the other came down, and he got a kind of correction that way. But, surely, that was not good enough.

MR. W. A. DIXEY: Why do you not use a graduated double rotated prism?

THE PRESIDENT: It cannot be got in the trial frame in combination with lenses already there. It would be much easier to be able to move the cells up and down. Continuing, Dr. Ettles said Mr. Chalmers had remarked that there ought to be two holophane reflectors, one above and one below; but the mirror did quite well. Then Mr. Dixey had told them they could do away with a chart such as he, the speaker, had shown them, and move the test chart backwards and for-

wards. But this would necessitate a very complicated mechanism, as it would have to be moved backwards and forwards for each intervening step

MR. DIXEY : It is only a possibility.

THE PRESIDENT dwelt on the question of intrinsic legibility of letters of test charts. It was not enough to design a letter which presented constant proportions of black and white—it must be capable of easy recognition as a proper letter. If a person read only the O's and L's in 6/6 he was not registered as having 6/6 vision ; the actual value would perhaps be only 6/9. But if he read the K, H and M. they knew he really had read it, and then the point of intrinsic legibility was most valuable as giving a sort of assessment of what a patient really could see. With regard to the series of lenses he had suggested, he knew an ophthalmic surgeon could go into an establishment and say, "I would like a trial case to this specification," but what he wished to point out was that the vast majority of trial cases were sold out of stock to beginners, who did not exactly know what they wanted, and what he said was that that stock case was not the best design.

The point raised by Mr. Dixey about the four grooves certainly got over his difficulty ; it was quite a good point, and he did not know such a plan had been adopted. But when Mr. Dixey talked about "mistrusting" his test for heterophoria, he had a very simple answer, namely, that people whose eyes were all right never made a mistake, and that people whose eyes were wrong did make mistakes. Then with regard to prisms, he feared Mr. Dixey was thinking about one thing while he, the speaker, was thinking of another.

MR. DIXEY : I was thinking of that principle in connection with the stereoscope.

THE PRESIDENT said if they took the case of a child with a squint, one eye directed straight, the other turned outwards, the brain automati-

cally switched off one image on to the other ; the cerebration was gone—the whole mental conception was gone.

MR. DIXEY : We are thinking about different things. I was not thinking of squints at all like that ; we were talking about heterophoria.

THE PRESIDENT : That apparatus is for training squints.

MR. DIXEY : I was not talking about that instrument at all, but about heterophoria tests.

THE PRESIDENT said for heterophoria it was no use unless they had such long levers, and the dial so finely graduated that the prisms and movements were interchangeable terms. After all, the prism only deflected something. They could either deflect the image of that something or deflect the something itself ; it did not matter which they did. Therefore prism powers and movements ought to be scientifically interchangeable. The beauty of the instrument, so far as he could claim any beauty for it at all, was that he had got the movements so exact and fine, that when speaking of the movements they could know what it meant in terms of prisms.

It was usual to think of prisms as being one kind of thing, and of movements as being another kind of thing, whereas really they were the same thing, or ought to be.
